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A REPLY TO MY CRITICS

I PROPOSE to consider the comments and criticisms, which have been contributed to this volume, under the following nine general headings, viz., (I) *Nature, Subdivisions, and Methods of Philosophy*, (II) *Philosophy and Religion*, (III) *Formation of Empirical Concepts*, (IV) *Substance, Process, and Causation*, (V) *Induction and Laws of Nature*, (VI) *Time in general, and Precognition in particular*, (VII) *The Psychophysical Individual*, (VIII) *Sense-perception and Matter*, and (IX) *Moral Philosophy*. I think that this covers the contents of all the contributions, except the parts of Professor Patterson's paper which are devoted to details in my exposition of McTaggart's philosophy. In some cases different parts of a single essay fall under different headings.

(I) *Nature, Subdivisions, and Methods of Philosophy*

Under this heading come the whole of Professor Körner's essay and parts of the essays by Professors Nelson and Patterson.

(A) "CRITICAL" AND "SPECULATIVE" PHILOSOPHY. I find nothing to dissent from and little to comment upon in Professor Nelson's remarks on my dealings with the branches of philosophy which in some of my later writings I distinguished under the names of "Analysis," "Synopsis," and "Synthesis." The history of English and American philosophy since 1928 has shown clearly that, when I wrote *Scientific Thought*, I greatly overestimated the certainty which could be hoped for in what I called "Critical Philosophy." It is no less true that I failed to notice the extent to which a philosopher's practice of Analysis is influenced (often unwittingly) by "metaphysical" presuppositions which, if made explicit, would fall within the province of Synopsis and of Synthesis.

Since the days when I first used the expressions "critical" and "speculative" philosophy, and alleged that an essential part of the former is "analysis of concepts," much work has been done in analysing the concept of analysis, and distinctions have been drawn which I had not recognised. Professor Körner may be said to be examining through a modern telescope a nebula which I had scanned with an old-fashioned opera-glass.

Professor Körner distinguishes two kinds of analysis, which he calls

"exhibition" and "replacement." Both presuppose "rules which we or others have accepted for the use of signs as concepts." We are told that a sign is made "conceptual" by being used by a person in accordance with a certain rule which he accepts, and that a person may habitually *conform* to rules which he could not formulate. The business of *exhibition-analysis* is to elicit and formulate the *de facto* rules governing the use of certain words, phrases, etc., without attempting to criticise those rules, or, if they should be unsatisfactory, to substitute others for them.

It seems to me that this does include an essential part of the business of critical philosophy, but that, unless certain restrictions are put on the terms "rule," "sign," "usage," etc., it covers much that would not commonly be included in philosophy. Surely e.g., as it stands it would include the work of a grammarian, in the strict sense of that term, and of a prosodist. What is a writer of a Greek grammar doing, except to formulate the rules which ancient Greeks unwittingly followed in using words, phrases, and sentences when speaking or writing? And what is a writer on Greek prosody doing, except to formulate the rules in accordance with which words are strung together in lines, and lines in verses, in various kinds of Greek poetry?

Professor Körner says, quite rightly, that exhibition-analysis leads to *empirical* propositions about usage. In view of this it is important to note the following fact, and neither to underestimate nor to exaggerate its importance. With nearly all general names, e.g., "body," "animal," "person," etc., the situation is as follows. There are (a) innumerable cases where hardly anyone familiar with the language would *refuse* to apply the name; (b) innumerable cases where hardly any such person would *consent* to do so; and (c) a great many marginal cases where such a person would hesitate whether to apply it or to withhold it. In the group of marginal cases various possibilities exist. It may be that many such persons would unhesitatingly consent to apply the name, that many would unhesitatingly refuse to do so, and that many would hesitate. Again, it is often possible to present, not merely one, but several different series of marginal cases of the following kind. At one end of such a series most of such persons would feel little hesitation in consenting to apply the name; at the other end most of them would feel little hesitation in refusing to do so; and there would be a more or less continuous change in this respect as one presented intermediate instances in order.

This is certainly an important fact, and neglect of it may lead to tiresome and futile controversy. But it does seem to me to have gone to the heads of some contemporary philosophers, and to have produced the impression that endless dithering about series of marginal cases is all that is

required of them. What is needed is, not to stand moonstruck at a very simple fact which has been well recognised since the time of Locke (to go no further), but to proceed roughly as follows. In the first place, to compare and contrast the cases where people *unhesitatingly consent* to apply the name with those where they *unhesitatingly refuse* to do so, and to note the features common and peculiar to the first group. We thus get a set of rules for the application of the name "N" to what might be called "typical" or "indubitable" N's. Next, in the light of this, to look into the various series of marginal cases which diverge in different directions from the indubitable N's, and to note (a) what features distinguish one such series from another, and (b) the characteristic ways in which the distinguishing features of each such series vary as it diverges further and further from the indubitable N's. Thus we may hope to end with a set of rules for the application of "N" to *indubitable* N's, qualified by a set of generalisations as to typical ways in which the applicability of "N" shades off in various directions from the indubitable N's to the indubitable non-N's.

The above reflexions are not meant as a criticism on Professor Körner's remarks on exhibition-analysis. Passing now to what he calls "replacement-analysis," I find myself in general agreement with him. Certainly a person would have little motive for attempting to replace rules of usage, formulated as a result of exhibition-analysis, unless he held them to be defective in one way or another. That being granted, it is plainly desirable to make explicit the standards or requirements by which one is judging them, and the precise respects in which one thinks that they fall short. Lastly, it is important to make explicit what Professor Körner calls the "analysing relation," i.e., the kind of logical relation in which the replacing rules are supposed to stand to the rules which they are intended to replace. I have no doubt that I have often failed to fulfil these desiderata (if for no other reason, because they were not explicitly before my mind), and that the clear formulation of them by Professor Körner should help future analysts to do better in these respects.

Professor Körner says that exhibition-analysis leads to contingent propositions, and replacement-analysis to necessary ones. What he has in mind is true; but it is important to understand precisely what that is, and not to confuse it with something else.

It is true that any proposition to the effect that correct users of a language L have no hesitation in applying the name "N" when and only when the conditions $c_1c_2\dots c_n$ are fulfilled, is *contingent*. And it is true that any proposition to the effect that conditions $c'_1c'_2\dots c'_m$ are a replacement of conditions $c_1c_2\dots c_n$, given that the analysing relation is R , is *necessarily* true or *necessarily* false. But a proposition to

the effect that the conditions c_1, c_2, \dots, c_n are fulfilled in a given case may be either contingent or necessarily true or necessarily false. That will depend on the nature of the subject-matter. If, e.g., a particular animal fulfils (or fails to fulfil) the conditions under which an animal would unhesitatingly be called a "bird," that is a *contingent* fact about it. But, if the sum of a certain infinite series in pure mathematics fulfils (or fails to fulfil) the conditions under which a number would be unhesitatingly called "transcendental," that is a *necessary* fact about it. If the fulfilment (or the non-fulfilment), in a particular case, of the conditions formulated in an exhibition-analysis is *contingent*, then the fulfilment (or non-fulfilment) of the conditions substituted for them is also *contingent*. And the same would hold with "necessary" written for "contingent" in both places in the previous sentence.

I pass now to Professor Körner's discussion about the nature of what I have called "speculative philosophy." In order to state clearly what I take to be the points at issue, I will begin with the notion of a *sentence in the indicative mood*. When a person utters or writes such a sentence he *prima facie* intends to assert or to deny or to offer for consideration or to put on record something which can significantly be said to be *true* or *false*. And when a person hears or reads such a sentence in a language which he understands, he expects *prima facie* to have presented for his consideration something which can significantly be said to be *true* or *false*. Every such sentence, then, serves *prima facie* to state, record, or convey *factual information* (correct or incorrect). We can sum this up by saying that every sentence in the indicative is "ostensibly informative."

Now it is plausibly alleged that certain kinds of sentences in the indicative are in this respect misleading. Those who utter them are not in fact offering or recording information (correct or incorrect), though they may think that they are doing so. They are really only expressing an emotion, issuing a command, proffering advice, or so on. And those who hear or read such sentences understandingly are not thereby receiving any information (correct or incorrect), but are being emotionally stimulated, commanded, admonished, or so on. (This does not, of course, exclude the possibility that the hearer or reader may be led, either through explicit inference or through association, from hearing or reading such a sentence to forming a more or less confident opinion about the intentions, emotions, etc., of the speaker or writer.) I will describe such indicative sentences as "non-informative." Non-informative indicatives can then be classified in accordance with the positive functions which they perform, e.g., as evocative, admonitory, and so on.

Now any treatise on speculative philosophy certainly consists of sen-

tences in the indicative, and there is no doubt that most speculative philosophers have thought that most of the indicatives which they wrote or read in such treatises conveyed *factual information*. So one set of questions to be raised is this. Is this a complete mistake? Are *all* such indicatives really non-informative? If so, what functions do they perform (a) for those who speak or write them, and (b) for those who hear or read them understandingly? Again, if so, why are those functions habitually performed by the inappropriate and misleading means of sentences in the indicative?

Suppose that this extreme position were rejected. Suppose it were alleged that some at least of the indicatives in treatises on speculative philosophy really do *state propositions* (which the writers accept or reject or are uncertain about), and really do present those propositions to the consideration of those who read such treatises understandingly. Then two kinds of question could be raised, one logical or ontological, and the other epistemological.

The logical or ontological question is this. Is every proposition, stated by those indicatives in treatises on speculative philosophy which are really informative, either *necessarily true* or *necessarily false*? Or are they all *contingent*? Or are some of one kind, and some of the other kind?

The epistemological question is closely connected with this. But it is a different question, and it is important to distinguish the two. We may introduce it as follows. If a proposition is *contingent*, the only legitimate ground on which it can be accepted or rejected is *empirical*. If, on the other hand, a proposition is *necessarily true*, it is theoretically possible to have purely *a priori* grounds for accepting it; and, if it is *necessarily false*, it is theoretically possible to have purely *a priori* grounds for rejecting it. But a proposition might in fact be *necessarily true* (if true) and *necessarily false* (if false), and a person might even know that this is so, and yet he might have nothing better than *empirical* grounds for accepting or rejecting it. (That is the case, e.g., in regard to some propositions in the Theory of Numbers, such as Fermat's "last theorem.") So the epistemological question is this. Supposing (i) that some at least of the indicatives of speculative philosophy are informative, and (ii) that some at least of them state propositions which are necessarily true or necessarily false, has any speculative philosopher produced cogent *a priori* reasons for accepting any of the former or for rejecting any of the latter?

The above questions were certainly not all of them distinctly before my mind when I wrote about the nature of speculative philosophy and its relations to critical philosophy. Therefore no unambiguous answers to them will be found in my writings or can be elicited from them. What

little I can now say on this topic is perhaps best discussed in connexion with one section of Professor Patterson's essay. For, in the summary at the end of it, he raises the general objection that I have failed to make a satisfactory case against the *a priori* deductive method in metaphysics.

B) NATURE AND METHODS OF SPECULATIVE PHILOSOPHY. I should evidently be in a much stronger position, if I held that metaphysical sentences in the indicative must from the nature of the case be non-informative. But I have never been in the least impressed by the *general* argument, from the alleged nature of significant assertion, which has been put forward to prove this. It seems to me to depend on taking a very narrow and highly arbitrary definition of "significant assertion," and then ruling out by definition those indicatives as non-informative. I suppose that one may fairly say by this time that this kind of argument is characteristic of a philosophical school which (in the words of Oscar Wilde) "has a great future behind it." It seems to me plain that those who use this argument have at the back of their minds a number of ontological and epistemological premisses which constitute a part of an unformulated system of speculative philosophy. It seems to presuppose *inter alia* that there can be no *a priori* concepts, that all necessarily true propositions are analytic and all necessarily false ones are self-contradictory, and that all significant synthetic propositions are such that they can be validated or invalidated by reference to sense-perception or introspection. I am not convinced of any of these presuppositions.

That would of course leave it open to me to hold that some (or even all) of the indicatives which occur in works on speculative philosophy are *in fact* non-informative. It may well be that some of them are, but it seems to me that many of them convey to the reader propositions for consideration, for reasoned acceptance or for reasoned rejection or for suspension of judgment. Take, e.g., Leibniz's doctrine that what appears to us as an inorganic material thing, e.g., a stone, is in fact a collection of a vast number of animated organisms of a very low order; that what appears to us as an animated body is in fact a collection of minds of a lower order of intelligence related in a certain specific way to a single mind of a higher order; and that what we take to be the laws of inorganic matter are statistical regularities concerning such groups of very numerous minds of very low intelligence. I can understand these statements in outline by analogy with what I know, e.g., of a swarm of gnats appearing as a cloud, of habitual and instinctive action in men and animals, of crowd-psychology, and so on. They do not seem to me to be radically different in nature from the extremely difficult statements which theoretical physicists make about the ultra-microscopic constituents of macroscopic phenomena. They may happen to uplift, depress, or ad-

monish the reader; but, if they should do so, that seems to me to be incidental, as contrasted with the informative function which they perform.

Certainly they cannot be tested, as scientific theories can, by deducing from them consequences as to what should be perceptible by the senses under assigned experimentally producible conditions. A metaphysical theory has to be appraised by reference to such criteria as (i) its internal consistency or inconsistency, (ii) its coherence or incoherence with certain very general principles (positive or negative) which seems self-evident to the reader, and (iii) its ability (given that it fulfils the first two conditions to the reader's satisfaction) to unify in an illuminating way a number of very general and pervasive features of the inorganic, the organic, and the psychological aspects of the world. It is evident that universal or even very general agreement can hardly be expected, in view of the fact that general principles which seem self-evident to some persons will not seem so to others even of the same period and culture, and that principles which seem self-evident to most persons of a given culture at a given period may not seem so to those of other cultures or at other periods.

Suppose we take what Professor Patterson calls the "*a priori* deductive method in philosophy" to be the attempt to infer a set of far-reaching and surprising speculative conclusions from a comparatively few premisses, each of which is either found to be self-evident on reflexion or states a very general and obvious empirical fact which no-one would be likely to question. Then I am certainly not in a position to assert *a priori* that "the *a priori* method in philosophy" *must* be futile.

For here too I am in certain respects much less fortunately situated than many contemporary "anti-metaphysicians." I am not convinced that every proposition which is necessarily true must be *analytic*. And, if there be propositions which are synthetic and necessary, I see no reason why some of them should not be *self-evident* on careful inspection to most intelligent persons of appropriate training and interests. Again, it is obvious that there are very general empirical facts, e.g., that there appears to be *change* in general and *motion* in particular, which no sane person is likely to question. I am therefore not prepared to deny in principle that there might be premisses available for a satisfactory system of deductive metaphysics.

Granted this, it would be idle to make the general objection that no important and surprising conclusions are likely to be deducible from a few very abstract premisses. For in geometry the most beautiful and surprising consequences *have been* deduced from such premisses. And in theoretical physics such extremely abstract and negative principles as

the Entropy Principle, the Relativity Principle, and the Uncertainty Principle *have led* to highly interesting and unexpected results.

I am therefore reduced, as Professor Patterson says, to appealing to the alleged lack of success of deductive metaphysicians in the past. That is certainly not a very strong argument, and Professor Patterson questions even the empirical basis of it. Have Descartes, Spinoza, Leibniz, and Hegel, e.g., he asks, accomplished *nothing*?

To this I would answer as follows. They have done much to illuminate the problems which they have discussed, but I do not know of anything of importance which they have *established deductively*. Spinoza is in fact the only one of them who even claimed to do this, and I think that most of his readers would feel that what is valuable in his work is independent of, and tends to be obscured and distorted by, his deductive method of expounding it.

It does seem to me that nothing comparable to the results obtained in geometry and theoretical physics by the deductive method has in fact been achieved by that method in speculative philosophy. There seems to me to be good empirical ground for thinking it very unlikely that others will succeed where so many men of such outstanding ability through so many centuries have failed. And it may well be that a careful study of the peculiarities of the subject-matter of geometry and of theoretical physics would provide a more positive and detailed ground for scepticism as to the possibility of a system of mainly deductive speculative philosophy.

(II) *Philosophy and Religion*

From Speculative Philosophy there is a natural transition to the Philosophy of Religion. So I will consider next Professor Stace's essay.

I will begin with his remark that the really important conflict between science and religion is that the general spirit of science, as expressed in what he calls "the philosophy of naturalism," conflicts with any sort of religious view. We must either abandon naturalism or abandon religion or find some way of reconciling the two.

The "philosophy of naturalism," as I understand it, holds *inter alia* that all consciousness (and *a fortiori* personality) is completely and one-sidedly dependent on the fulfilment of certain physico-chemical, physiological, and anatomical conditions. Every particular experience depends one-sidedly on a particular occurrence in a certain brain or nervous system, and each person's dispositions, character, personality, knowledge, and skills depend one-sidedly on the particular minute structure and organisation of his brain and nervous system.

Now, on the one hand, everything to which we attach value or dis-

value seems to reside in or to relate to *persons*, who experience sensations, thoughts, desires, emotions, etc., and have elaborately organised cognitive, conative, and emotional dispositions. On the other hand, the physico-chemical, physiological, and anatomical conditions of consciousness in general and of organised personality in particular seem to be highly specialised, narrowly localised in time and space, extremely delicate and unstable, and altogether at the mercy of that part of nature which is organised at a lower level of complexity.

Any such view is plainly incompatible with what most people in the West and many in the East have understood by religion. For Christians, Jews, and Mohammedans, at any rate, the following propositions, taken quite literally, are essential. (1) The specifically *moral* values and dis-values, which inhere in human persons and express themselves in their volitions, emotions, thoughts, and actions, are *not* just transitory by-products of conditions to which no kind of intrinsic value or disvalue can significantly be assigned and which cannot significantly be said to have any preference for the one over the other. On the contrary, there is in every human being an essential factor which is existentially independent of his body and is destined to endure endlessly, though it may always need to be connected with an appropriate organism of some kind in order to constitute a full personality.

(2) Again, it is held that an essential part of the total environment in which human beings live falls outside the range of sense-perception and the ken of natural science. It contains non-human spiritual beings, good and evil, who are either bodiless or embodied in organisms composed of a kind of matter with which natural science has not hitherto been concerned. This non-human and non-material environment is so organised that a human being who makes morally wrong choices and entertains morally evil thoughts, desires, and emotions during the life of his present body, will inevitably suffer after his death, not only *moral* degradation, but also unhappiness, pain, and misfortune. And a similar proposition is held to be true, *mutatis mutandis*, of those who make morally right decisions and entertain morally good thoughts, desires, and emotions.

It seems to me certain that this much is held *quite literally*, in outline, by nearly all sincere Christians and by many other religious persons. Moreover, it is not held only by simple and ignorant men, though the wisest of those who hold it are the most ready to admit that we know very little of the details and can speak of them only in metaphors and analogies drawn from our present experience. The most usual and the most intelligible analogy is that of a society of spiritual beings, with one supreme spirit in complete control of their environment and stand-

ing to them in the relation of a wise, just, and loving father or king. This is certainly an essential part of what religion means for ordinary religious persons, and of what it has meant for such men as Aquinas, Leibniz, Berkeley, Kant, James, and Ward. I can see no good reason for ignoring this, and confining the connotation of the word to what it may have meant and may still mean for Hindu philosophers and mystics of one particular school and for some few Christian and Mohammedan mystics of dubious orthodoxy.

Now there is no doubt that religion, in this sense, is in head-on collision with the philosophy of naturalism, as I have described it above. It is to religion in this sense that the results of psychical research might possibly be relevant, in view of this collision. I will now explain what this possible relevance might be.

I do not need to be told that the temporary survival of bodily death, or even the endless duration of a human personality, if it could be empirically established, would not entail theism and would be compatible with a wholly non-religious view of the world. It would, e.g., be consistent with the view that each of us will persist endlessly as a sequence of embodied persons, more or less like oneself and one's neighbours as we now are, living on earth or on other planets much as we now do. Plainly that, in itself, is completely irrelevant to religion. I am sure that I have never been under the least illusion on that point.

Where psychical research might conceivably be relevant is here. It might establish facts about human cognition and about the effects of human volition which are extremely difficult or impossible to reconcile with the epiphenomenalist view of consciousness in general and of human personality in particular. That might happen as a result of experiments and observations which have no *direct* bearing on the question of human survival of bodily death, e.g., those concerned with alleged cases of clairvoyance, of telepathy, or of telekinesis. I do not suggest that this is the *only* way to attack the philosophy of naturalism. I think that it can be shown, on purely logical and epistemological grounds, to be an incoherent doctrine based on shaky foundations. But such arguments are difficult to follow, and there is little agreement among experts as to their validity. On the other hand, the philosophy of naturalism is supported psychologically by the immense prestige which the methods of natural science now enjoy, and for many persons it could be undermined only by counter-instances established by the same methods.

Now it is undoubtedly true that there have been deeply religious men who explicitly rejected the religious ideas and beliefs which I have outlined above. I do not know enough in detail about Hindu philosophy

or religion to venture to speak about it. Instead, I shall take as my example Spinoza, a man brought up in a religious and philosophical tradition with which we are all more or less familiar.

I have studied Spinoza's *Ethics* carefully, and have striven to understand it in order to explain it to my pupils. What I think I understand of the first four books is enough to convince me that he was a great and a very honest thinker, of an extremely "tough-minded" sort, and the last man in the world to indulge in edifying verbal mystifications. But, when I come to the dividing line in Book V, where he says: "It is now time for me to pass to those things which concern the duration of the mind without relation to the body," I begin to be lost. I am sure that the language used in the latter part of Book V is sincerely meant, not only to *express* a deep religious conviction, but also to *justify it rationally* to others. But I am quite unable to grasp what Spinoza has in mind when he talks of "the Third Kind of Knowledge," "human immortality," and "the intellectual love of God." And when, after the account of the human mind and the human body in the previous books, he says in Book V: *Sentimus experimurque nos aeternos esse*, I am left gasping. Either he is expressing in appropriate language an experience, of which I have never had a glimmering; or he is expressing an experience, which I have had, in language so inappropriate that I cannot recognize and identify his reference. Naturally the former alternative is much the more likely.

However that may be, the following things seem certain. Spinoza must have thought it *intelligible* to talk of a person's mind existing out of relation to his present body, at any rate as that body is known to the person himself by organic sensation and to others by external sense-perception and its elaboration by natural science. He must have held that a person's mind, out of that relation, exists *timelessly* (and therefore neither for a short time nor a long one nor *sempiternally*). He must have regarded this as an *essential doctrine of religion*. And he asserts explicitly that the doctrine of the unending duration of the human mind is an attempt to express its *timeless* existence in temporal terms suited to the needs and intellectual limitations of the vulgar. He argues that there can be no coherent thought answering to the phrase "unending duration." But he is no less certain that there is a clear positive idea answering to the phrase "eternity" or "timeless existence," and that it can be grasped by any intelligent person, of philosophical training and aptitude, who will take enough trouble.

I do not know whether Spinoza had mystical experiences or not. If he did, he never (so far as I am aware) mentioned the fact, and he certainly never appeals to such experiences in himself or in others.

I think he would have regarded any such appeal by a philosopher in a philosophical work as a breach of the rules of the game,—an unsportsmanlike attempt to hit his readers below (or above) the intellect.

Now to this kind of religion the results of psychical research would be irrelevant. The utmost that psychical research could do would be to produce overwhelming evidence for believing that an essential element in a person *persists after* the death of his body and *continues* to have experiences, to initiate actions, and so on. It moves in the same sphere, viz., that of succession and duration, as that in which the philosophy of naturalism moves. But, when Spinoza alleges that a person's mind has an existence independent of his body, and that that existence is eternal, he is plainly intending to assert something which falls *outside* that sphere of temporality which is common to the orthodox scientist and the psychical researcher. The difficulty is to attach a meaning to what he asserts, to understand his reasons for holding it, and to see how it can be reconciled with the kind of facts with which *both* orthodox science and psychical research are concerned.

On the far side of Spinoza comes the kind of extreme monistic mysticism which Professor Stace seems to regard as the only form of religion which an intelligent and instructed person nowadays need seriously consider. It certainly enjoys all and more than all the advantages, ascribed by Oscar Wilde to the writings of contemporary liberal theologians, of "leaving the unbeliever with nothing to disbelieve in." Evidently it would be futile for me to write at length about an experience which I have never had, and of which I learn from Professor Stace that the only significant statement which can be made is that no statement about it could possibly be significant.

I will content myself therefore with a few platitudinous comments on one typical sentence, viz., that there is for a mystic "no distinction between himself and his experience on the one hand and God on the other hand, because he and his experience are simply identical with God."

Now, if we try to get down to brass tacks, I suppose that what this comes to is roughly the following. When Mr. Chatterji, who has had a mystical experience and returned to normal consciousness, tries to recollect it and to describe it to himself and to his friend Mr. Mukerji, he notes that while having it there seemed to him to be no distinction between (i) himself and his experience, (ii) his experience and God, and (iii) himself and God. He also, we will suppose, recollects that the experience seemed to him at the time to be a clear and illuminating one, and not a confused and muzzy one, such as he has had when about to faint or to go to sleep.

Now, if I were in a position to interrogate Mr. Chatterji before he "passed out" again, I should raise the following questions. (1) When you say that there seemed at the time to be no distinction between yourself, your mystical experience, and God, do you mean that you then considered the question whether there was or was not a distinction, and that you *noticed* on inspection that there was *not*? Or do you mean only that the question of identity or difference did not present itself to you at the time, and that the experience was so absorbing that you *did not notice* that there was any distinction, and probably *would not* have done so even if there had been one?

(2) Your mystical experiences, like your other experiences, begin at certain moments, last for so long, and then cease. There are innumerable other experiences, going on simultaneously or successively in yourself or in other men or animals. If you say that *this* experience of yours is identical with God, and mean that statement to be taken literally, then God is identical with *it*. If so, God must have any characteristic that belongs to it, and so must begin when it begins, end when it ends, and be one among innumerable other items simultaneous or successive. Obviously that is not what you believe.

(3) When you speak of "identity" in this context, do you mean identity in the strict sense in which it occurs, e.g., when we say that the 49th word, reading from the left and downwards on a certain page of a certain book, is identical with the 476th word, reading from the right and upwards on the same page? Or are you using the expression only to assert and to emphasize a *specialty intimate relationship* between several *diverse* entities, which are commonly but mistakenly thought to be existentially independent of each other and only externally inter-related?

(4) As regards your recollection that the experience seemed at the time to be a peculiarly clear and illuminating one, I would remind you (for what, if anything, it may be worth) that this feature has often been noted by persons in the experiences which they have had when going into or coming out of the anaesthesia produced by nitrous oxide and certain other narcotics.

Let us now leave Mr. Chatterji "alone with the Alone," and turn to some other matters which are more susceptible of rational discussion.

Professor Stace asserts, on grounds which are independent of any reference to mystical experience, that all arguments for the existence of God *must* be futile. The reason alleged is this. "Existence," as predicated of God, does not mean the same as "existence" when predicated of a particular thing, e.g., the Albert Memorial, or of a class of such things, e.g., cows. In the latter sense "to exist" means to be a part of

the universe; in the former application it cannot mean that. Now arguments from "certain observed facts about the world" could prove existence only in the sense of being a part of the universe. On this I would make the following comments.

(1) The point about the ambiguity of "existence," as applied to God (or to the universe) and to finite individuals or classes of such, has not escaped the notice of such men as Aquinas, Leibniz, and Spinoza. They have explicitly insisted on it. Yet, in full consciousness of it, they have not hesitated to deploy arguments for the existence of God. This suggests that the case may not be so simple as one might think on reading Professor Stace's remarks.

(2) When Professor Stace speaks of arguments starting from "certain observed facts about the world," this seems to apply primarily to the Teleological Argument. This argues *from* certain concrete characteristics, especially in living organisms and their environment, which may be described as "internal teleology and external adaptation," to the existence of an intelligent designer and controller of nature. (As Professor Stace rightly points out, it is an argument *to* design, not *from* design.) Then, again, it makes use of the notion of causation, in the sense in which that occurs in ordinary life and in natural science. I think it would be generally agreed that such an argument, if valid, could establish only the existence of a certain very powerful and (in some ways) very intelligent *inhabitant* of the world, and that is *not* what the higher religions understand by "God."

(3) But it is not at all obvious that this applies to such an argument as the Cosmological Argument in its various forms. What this sets out *from* is the *contingency*, not only of each particular thing and event in nature, but also of the *whole causal network*, in which the existence of each thing and the occurrence of each event is "explained" only by reference to things which existed and events which happened before it, and in which the "explanation" is only in terms of causal laws which have no trace of necessity. It argues, from this extremely general *modal* feature of the whole order of nature, that the latter cannot be self-subsistent. And it infers from this that the whole order of nature must stand in a relation of one-sided dependence (quite different from the relation of cause-and-effect, which connects finite things and events *within* nature) to something else, whose existence is *intrinsically necessary*.

I think that this line of argument is open to serious criticism, and I have stated my objections to it in my writings. But it must be criticised on its own grounds. I am quite sure that Professor Stace's *general*

objection, quoted above, to all arguments for the existence of God, is irrelevant to it.

(III) *The Formation of Empirical Concepts*

Under this heading I shall deal with a part, but not the whole, of Professor Turnbull's admirably acute and thorough paper, viz., that which is concerned with the account which I have given of what is commonly called "abstraction" and of what I call "descriptive ideas." In discussing this essay I shall not attempt to use the formidable technical terminology which Professor Turnbull deploys at the beginning of it. That is not because I have failed to understand it, or because I think it unsuitable for its purpose. It is because I am sure that all that I have to say here can be stated accurately enough in a simpler and more homely way.

(A) ABSTRACTION. This is the process by which, it is alleged, a person forms a dispositional idea of such a characteristic as *red* or *round* from perceiving with his senses objects which present themselves sensibly to him as red or as round. I must confess that I took over the account which I gave of this process very uncritically from a tradition which goes back (I suppose) through Locke to the Scholastics. I tried to state it as clearly as I could, but I did not seriously question its presuppositions. When one is forced to consider them, they do seem rather shaky. It is plain that Professor Turnbull rejects the whole traditional story and would offer a very different one in its stead. But he also claims to detect inconsistencies between certain statements *within* my exposition of the traditional theory. I shall here deal mainly with these alleged internal inconsistencies.

(1) The first point which I will consider is this. In my account of the origin of the dispositional idea of a determinate colour, e.g., *red*, I assumed that what a person compares and contrasts are *physical objects*, e.g., skeins of coloured wool, coloured crayons, the tablets of pigment in a paint-box, and so on. Much later on, when I came to deal with the analysis of sense-perception I suggested that to see a physical object consists in (i) being sensibly acquainted with ("prehending") a certain particular which sensibly presents itself as having a certain determinate colour, shape, etc., and (ii) taking it uncritically and non-inferentially as part of the surface of a *body*, with certain other parts and certain other properties which are *not* at the moment being sensibly presented to one. In this connexion I threw out (without laying much weight on it) the following suggestion about such sentences as "That body is red," as contrasted with "That body is *now presenting itself to my sight as red.*" I thought that the propositions expressed by the former might

have to be analysed in terms of those expressed by the latter, e.g., as asserting that that body *would* present itself as red to the sight of any person with normal eyesight, if he were to view it directly under normal illumination. At any rate, I was inclined to think that it was only if such sentences were understood in some such way as this, that there would be good reasons for accepting what they state as *true* in the cases where we all do in fact do so.

Now Professor Turnbull seems to think that these latter reflexions about the analysis of sense-perception, and about the interpretation of such sentences as "That body is red," somehow *conflict with* the original account of abstraction and demand a *radical revision* of it. If he does think so, I do not agree with him. The account of abstraction is and remains in terms of colours, shapes, etc., of *bodies as seen*. We are all perfectly familiar with such situations as we should describe as "seeing two skeins of wool of the same shape and different colours" and as "seeing a skein of wool and a postage-stamp which are visibly alike in colour and unlike in shape." According as there are various suggested analyses of "seeing a body as of such and such a shape and of such and such a colour," there will be as many different *analyses* of the process of comparing and contrasting seen bodies in respect of the colours and shapes which they present to sight. But the process is in any case just the one which I have indicated, and which I am sure that anyone can recognize from my description.

(2) I pass now to a much more radical criticism. If I understand him aright, Professor Turnbull holds that my whole account of abstracting (no matter what analysis, if any, be given of sense-perception) is *logically circular*. The reason alleged is that to see a body as, e.g., red, presupposes that the person who has that experience already has the dispositional idea of *red*. Professor Turnbull does not argue this point, he seems to find it obvious.

Now it is not in the least obvious to me. Suppose that a young child (or possibly a cat or a dog) with normal eyesight looks in daylight at a body, such as a ripe tomato, which Professor Turnbull and I (who have acquired the dispositional idea of *red* and the use of the word "red") would describe as red. Then I assume that the percipient would have a visual experience which differs in a characteristic way from the one which he would have if he were to look, in similar circumstances, at another thing, similar to it in shape and size, such as an unripe tomato, which Professor Turnbull and I (who have acquired the dispositional idea of *green* and the use of the word "green") would describe as green.

All that my account of abstraction presupposes is that there are *in*

fact such characteristic unlikenesses and such characteristic likenesses between certain visual perceptual experiences of young children. Obviously a percipient could not *describe* these differences as "seeing this as red and seeing that as green," and he could not *understand* such descriptions, unless and until he had acquired the dispositional ideas of *red* and of *green* and the proper use of the words "red" and "green." But that is no reason why he should not have experiences which *in fact* differ in the ways which *we* so describe, nor is it any reason why he should not come to *recognise* that fact about his experiences.

(3) Professor Turnbull raises the question: What do I suppose to be *innate* in reference to the dispositional ideas of *red*, of *blue*, and so on? I suppose that, in order to be able to acquire the idea of *red*, the idea of *blue*, and so on, one must have the innate capacity to have visual experiences which in fact differ in all those characteristic ways which we come to describe as "seeing a thing as red," "seeing a thing as blue," and so on. Since it is logically possible (and also causally possible, as the facts of "colour-blindness" show) to have some of these capacities without the others, one must presumably postulate a number of logically (and to some extent causally) independent innate colour-sensation capacities. Given these, I should have thought that one and the same general innate capacity to notice, compare, contrast, and abstract would be involved in regard to each particular colour-likeness and colour-unlikeness experienced. However that may be, I do not think that any valid objection can be made to the traditional doctrine of abstraction, on the ground that it may need to postulate a very large number of innate capacities. Since *some* have to be postulated by everyone, the *precise number* required by a particular theory seems to be a minor matter.

(4) The questions so far considered might be called "psychological" or "epistemological." But Professor Turnbull also raises a question which might be called "ontological," viz: *What* is supposed to be abstracted from *what*?

I fear that I cannot offer much positive information on this point. What is supposed to be *abstracted* is, of course, a quality or a relational property or a relation. What it is supposed to be *abstracted from* is a particular which sensibly presents itself as qualified by that quality or relational property, or a set of particulars which sensibly present themselves as inter-related by that relation.

Obviously all expressions which suggest that a particular stands to a characteristic which characterises it as a whole stands to a part of it, and that abstraction is analogous to the physical separation of a part from a whole, are hopelessly misleading. When the idea of *red* has been

abstracted from a number of red particulars, they have not thereby lost their redness, as if abstraction were a process of leaching or bleaching! What has happened is that a person has acquired the ability to *think of* a characteristic, which did (and, for all that concerns the process of abstraction, may still) characterise those particulars, without needing at the time to *perceive* those or any other particulars as characterised by it. In some sense it is certain that this phrase describes something with which we are all perfectly familiar. The difficulty is to give a satisfactory analysis of this state of affairs. I am quite willing to believe that the traditional account of abstraction fails to do this, and that it may tempt one to ask silly questions or may call up absurd associations. I find little difficulty myself in resisting the temptations and ignoring the associations.

(5) This leads on to a fundamental objection raised by Professor Turnbull. I said that, when a person has acquired the dispositional idea of, e.g., *red*, he has *ipso facto* acquired the ability to "contemplate" the characteristic *red* without needing at the time to be acquainted, either in sense-perception or in imagery, with a particular which presents itself as red to him. And I said that a person is having an occurrent idea of, e.g., *red*, whenever he is in fact "contemplating" the characteristic *red*.

Now some of Professor Turnbull's comments on this do seem to me to be mainly verbal. He shows quite conclusively that the experience described as "contemplating the characteristic *red*" is extremely unlike various other experiences which are commonly and more literally described as "contemplating so-and-so." That in itself would show only that the name "contemplating" (which, like *all* names applied to intellectual operations, is used metaphorically) is not a happy one, and is more likely to mislead than to illuminate.

But behind this verbal skirmishing there is an attack which I regard as serious, and to which I have no satisfactory defence. The point may be put as follows. I have been inclined uncritically to regard the experience of thinking of a characteristic (e.g., of *red*), when one is not being presented either sensibly or in imagery with anything that presents itself as red, as analogous to *being acquainted with a particular*. I have spoken as if the only difference were on the side of the *object*, viz., in one case a particular and in the other a universal. I have in fact been still more specific, for I have undoubtedly tended to regard the experience as analogous to that acquaintance with a particular which is an essential factor in *seeing* a body or such a physical event as a flash of lightning. Moreover, I have tended to think of the process of becoming aware of a *necessary connexion or disconnexion* between two charac-

teristics, e.g., between equilateral and equiangular triangularity, as analogous to prehending two coloured particulars and noting, e.g., that the red one is *adjoined* to the blue one.

The use of language drawn from visual perception evidently comes naturally to speakers of Indo-European languages. (I do not know whether it extends beyond them.) We all talk of "seeing" or "failing to see" a logical connexion. If it be a bad habit, it is one that we have inherited from our prehistoric ancestors; for the Latin *video* and the English *wit* (with its kith and kin *wissen* in German, *veta* in Swedish, *witan* in Anglo-Saxon, etc.) have a common root, which means "to see." Nevertheless, when one is made aware of the habit and begins to reflect on what one has been doing, the analogy suggested by the verbal expressions is found to be faint in the extreme. I am now fully aware of the fact that the experience of thinking of a characteristic, in the absence of any perceived or imaged instance of it, is and must be utterly unlike the experience of seeing in the literal sense. I am sure that, through applying the language of visual perception to intellectual operations, I have often been led unwittingly through its associations to make unjustifiable assertions about them. The driving home of this point is for me the main positive outcome of Professor Turnbull's criticisms.

(6) As regards the relation between acquiring a dispositional idea, e.g., that of *red*, and acquiring the power to use and understand correctly the corresponding word, e.g., "red," all that I have to say is this. I take it that Professor Turnbull's view as to the correct analysis of the notion of having a dispositional idea of *red* is such that it would be *logically* impossible either (i) to acquire the dispositional idea without acquiring the corresponding verbal ability, or (ii) to acquire the latter without the former.

Now I certainly gave an account of the acquirement of the dispositional idea which made no mention of the acquirement of the corresponding verbal ability. So it may fairly be concluded that I held that the acquirement of the former is *logically* independent of the acquirement of the latter. It cannot fairly be concluded that I held it to be *causally* possible for a human being to acquire the dispositional idea without acquiring the corresponding verbal ability. Still less could it fairly be concluded that I held that there are any known instances of a human being acquiring the former without acquiring the latter. I should think it most unlikely that there have been or will be. And I am inclined to think (though without any strong conviction) that it may be causally impossible that there should be such a case.

(B) "DESCRIPTIVE IDEAS." I developed this notion in reference to a question which Hume raises in Sect. I of Part I of Book I of his *Treatise*

of *Human Nature*. Suppose that Jones has never seen anything which presented to him any shade of red between the two shades s_1 and s_2 . Suppose he has seen things which presented to him the shade s_1 , and has seen things which presented to him the shade s_2 , and has formed what I called an "intuitive idea" of those shades. Then I alleged that the sentence: "Jones is thinking of the *shade of red* between s_1 and s_2 " means what is meant by the sentence: "Jones is thinking of the *proposition* that there is one and only one shade of red between s_1 and s_2 ." Professor Turnbull objects to this, on the ground that to think of a *shade of colour* cannot be the same as to think of a *proposition*.

That certainly seems very plausible at first sight, since a shade of colour and a proposition are obviously entities of two entirely different types. Nevertheless, I do not find the objection conclusive on reflexion. Remember that there may in fact be *no* shade between s_1 and s_2 (for the series of possible shades may be discontinuous, and these may be immediate successors in the series), or, on the other hand, that there may be *more than one*; and yet that the fulfilment of either of those possibilities would be quite compatible with Jones having an experience correctly describable as "thinking of *the shade* between s_1 and s_2 ." In view of this, is it not somewhat naive to argue, from the premiss that a shade of colour is not a proposition, to the conclusion that "Jones is thinking of the shade of colour between the shades s_1 and s_2 " *cannot* mean what is meant by "Jones is thinking of such and such a proposition"?

I would make a similar rejoinder *mutatis mutandis* to Professor Turnbull's in principle similar objection to my account of such experiences as would be described by saying, e.g., "Jones is thinking of a fire-breathing serpent."

But I agree that his hypothetical example shows that a case is conceivable where an *idea* which would be simple, according to my criterion, would have as its *ideatum* a characteristic which could quite properly be called "complex." I would say that, when a person has an idea which is simple, in accordance with my criterion, he has *no positive reason to think* that its *ideatum* is complex, in the sense illustrated in Professor Turnbull's example. But a person in that position ought always to be ready to admit the possibility that the *ideatum* may be complex, in that sense.

(IV) *Substance, Process, and Causation*

I shall be concerned in this Section with parts of Professor Blanshard's and of Professor Patterson's essays and with the whole of Professor L. J. Russell's.

(A) SUBSTANCE. I propose to open the discussion by distinguishing

between an "empirical substance" and "substance in the metaphysical sense."

There is no difficulty in giving clear instances and clear counter-instances of *empirical* substances. They include such existents as would commonly be described as "things" or "plants" or "animals" or "persons." A stone, an oak-tree, a pig, and a man are instances which everyone would admit. On the other hand, I suppose that everyone would decline to describe as an empirical substance either (a) a flash of lightning or a twinge of toothache, or (b) such a localised and dated occurrence of redness and hotness as exists when a poker has been held in the fire for a time and is withdrawn. The last mentioned would be described as a temporary "state of" a certain thing, viz., the poker, or as a more or less prolonged "phase in the history of" that thing. A flash of lightning seems to be very much like it in its intrinsic nature, but there is no very obvious empirical substance of which a flash of lightning or a peal of thunder could be said to be a "state." I will class together such existents as clearly fall under either (a) or (b) under the technical name of "empirical occurrents."

By comparing such existents as would unhesitatingly be described as "things" or "plants" or "animals" or "persons," and by contrasting them with such existents as flashes of lightning, twinges of toothache, etc., we could discover a set of properties which might be described as together characteristic of a *typical* empirical substance. In a similar way we could discover a set of properties which might be described as together characteristic of a *typical* empirical continuant. We must not assume, however, that every existent will fall neatly into one or other of the two classes thus demarcated. There are marginal cases, e.g., a whirlpool, which have some features of the one and some of the other, and not all the features of either. Again, there may be existents which do not answer very well to *either* description. What is one to say, e.g., of a man's *mind*, as distinct from his *body* (which is a typical living thing of the animal kind), and from the *man himself* (who is a typical person)?

Now the notion of substance in the *metaphysical* sense arises when one begins to philosophise about typical substances in the *empirical* sense. One feature of any typical empirical substance is the specially close unity between a number of dissimilar *contemporary* occurrents, so that they together constitute a single total state of that thing or plant or animal or person. Another feature is the specially close unity between certain *successive* total states, so that they together constitute the *history* of that empirical substance, with various overlapping subordinate strands (monotonous or variegated) within it. A third feature

is the presence of *dispositional properties*, some invariant and others variable in accordance with dispositional properties of "higher order." (An example of the former would be mass, and of the latter magnetisation, in the case of a bit of iron.)

The notion of a substance in the *metaphysical sense* is an attempt to account for these features of substances in the *empirical sense*. It involves the notion of a peculiar existent, other than the various empirical occurrents which are counted as states of an empirical substance. This is held to be utterly different in kind from each of the latter severally, and from the complex whole composed of them all collectively in their mutual relations. Let us call such a supposed existent a "substratum."

For each empirical substance there is supposed to be *one and only one* substratum, and for each *different* empirical substance a *different* substratum. The substratum corresponding to a given empirical substance is supposed (i) to unify various contemporary empirical occurrents into a single total state of that empirical substance, (ii) to unify successive total states of it into the total history of it; and (iii) to carry its dispositional properties. In order to perform the first function a substratum is supposed to be completely *unvariegated* at any given moment, as against the many and various simultaneous empirical occurrents which it unifies. In order to perform the second function it is supposed to be completely *invariant* through lapse of time (either by enduring without variation or by existing timelessly), as against the many and various successive total states. The substratum corresponding to a given empirical substance is supposed to stand to each empirical occurrent which counts as a state of that substance in an asymmetrical dyadic relation of a unique kind. This is called by the metaphorical name "supporting." The converse of it is called by the equally metaphorical name "inhering."

Now I think that the word "substance," in the metaphysical sense, has been used in two different ways by philosophers. Sometimes it denotes a *substratum*, considered *apart from* the empirical occurrents which inhere in it and constitute the states (contemporary or successive) of an empirical substance. At other times it denotes the *complex whole*, consisting of a substratum *together with* the empirical occurrents which inhere in it, considered as organised by the relation of inherence. For anyone who accepts the theory and uses his terms in the *former way*, a substance in the metaphysical sense is *one constituent*, of a unique kind, in a substance in the empirical sense. For one who uses his terms in the *latter way*, "substance in the empirical sense" and "substance in the metaphysical sense" coincide in *extension*. Any empirical substance is so constituted as to be a substance in the metaphysical sense, i.e., a

unified whole, consisting of a substratum and the occurrents which inhere in it, organised by this relation of inherence.

Now so far as concerns the application of this metaphysical theory to such empirical substances as stones, trees, pigs, etc., I should agree with what I take to be Professor Blanshard's view of it. The theory is hardly worth discussing in that connexion. And that is because there is nothing to be said about the alleged substratum except either (i) to reiterate the properties which constitute the definition of the term "substratum," or (ii) to talk about the *particular* occurrents and the *particular* dispositional properties which belong either (a) to the substratum of a *particular* empirical substance (e.g., Bucephalus), or (b) to the substrata of all members of a *species* of empirical substances (e.g., horses). As regards (i), it is true that you can say of a substratum that it "supports" the states, qualities, and dispositions of an empirical substance, and thus provides them with their characteristic unity at each moment and through successive moments. But does this really tell us anything? As regards (ii), the reference to substrata seems to be idle. Nothing is lost if we talk simply of particular empirical substances and species of empirical substance, and drop all mention of their alleged substrata. So far I agree with Professor Blanshard's criticisms on what I have written.

We must note, however, that the notion of a *particular* in the empirical sense is wider than that of a substance in that sense; for it covers both empirical substances and empirical occurrents and some existents which we might hesitate to classify as either, e.g., a stone, a flash of lightning, and a whirlpool. Now it might be asked whether we are not brought back to the notion of something like a substratum when we reflect on the nature of empirical occurrents. Certainly we have to distinguish in the case of any empirical occurrent (e.g., a flash of lightning) the following two aspects. One of them is the completely determinate, but none the less *universal*, characteristics, of which it is an instantiation or manifestation, e.g., a certain absolutely determinate shade of blueness, and a certain absolutely determinate shape, extension, and duration. The other is the particularity of *this* instantiation of those absolutely determinate universals, as contrasted with *other* actual or possible instantiations of precisely the same determinate universals, either simultaneously at other locations or successively at the same location with an interval of time between.

Now there certainly is a temptation to deal with these two essential and correlated aspects of any empirical occurrent in somewhat the way in which the substratum-theory treats empirical substances. An empirical occurrent is then thought of in one or other of the two following ways. (i) As consisting of (a) a short-lived substratum, in which cer-

tain determinate qualities inhere so long as it lasts, and which stands while it lasts in determinate spatial relations to other such substrata which are contemporary with it; and (b) the determinate but universal qualities which inhere in it. (The name "occurrent particular," in the *metaphysical* sense, might then be given either to such a short-lived substratum considered in *abstraction from* the qualities which inhere in it; or to the empirical occurrent, considered as consisting of such a substratum *together with* the qualities which it "supports" and thus unifies.) (ii) The other alternative is to take seriously the existence of Absolute Space, as a kind of single persistent substratum, and to think of any empirical occurrent as a region of Absolute Space, of determinate shape, size, and location, pervaded and thus marked out from the rest by certain determinate qualities for a certain period from a certain date.

I know that all that I have been saying about empirical occurrents must be as familiar to Professor Blanshard as it is to me; and I find the two alternatives, which I have tried to formulate, as unilluminating as he no doubt does. But I have nothing positive of my own to offer, and I must content myself with the following platitudes.

(1) It is not worth while to get rid of substrata in connexion with empirical *substances*, if they have to be re-introduced in connexion with empirical *occurrents*.

(2) The alternative which presupposes Absolute Space, as a kind of *materia prima* for all empirical occurrents, seems unfitted to deal with *mental* occurrents. Can one plausibly (or even intelligibly) allege that, e.g., an experience of anticipating with apprehension a forthcoming visit to one's dentist consists of a certain region of Absolute Space qualified for a certain period by apprehensiveness? The late Professor Alexander was capable of saying such things, but the stomachs of most of us are not strong enough to swallow and digest them.

(3) In the case of those empirical substances which are *persons*, we cannot perhaps dismiss the substratum theory so cavalierly as in the case of non-living bodies and plants and non-rational animals. To be a person involves being aware of one's own *unity*, as contrasted with one's various contemporary experiences, and being aware of one's own *identity* throughout the sequence of one's successive experiences. Now it has been argued that this is unintelligible except on the hypothesis of a Pure Ego (which would seem to be a substratum of a very special kind), and on the hypothesis that each person is acquainted with his own Pure Ego as well as with his own experiences. In that case he would presumably be intuitively aware of the relation of "supporting," in which the former stands to the latter. And, having acquired the notion of that relation in this way, he might proceed to apply it (justifiably or unjusti-

fiably) to empirical substances other than persons, e.g., to animals, plants, stones, etc. I do not say that I find these contentions convincing. But no account of the unity of a person, with which I am acquainted, seems to me very convincing. So I think that they deserve to be taken seriously, and to be met on their own ground and not just waived aside under the pretext of the *general* futility of the notion of a substratum. I do not suppose that Professor Blanshard would seriously disagree with this.

(B) **THING AND PROCESS.** Under this heading I shall be concerned mainly with one part of Professor Russell's paper. But before doing so I will comment very briefly on some remarks by Professor Patterson on this topic.

Professor Patterson says that the phrase "absolute process" conveys nothing intelligible to him. In so far as that is the case, it is not worth while to argue with him about it. But he proceeds to develop some consequences which he thinks would follow from certain statements of mine about processes. I strongly suspect that there is a misunderstanding here, and I will try to clear it up.

Professor Patterson ascribes to me the opinion that a process is always composed of shorter successive phases which *partially overlap*. The consequences which he develops seem to be derived from this alleged partial overlapping of successive phases.

Now, so far as I am aware, I spoke of partial overlapping *only* in reference to the doctrine of the Specious Present. I thought that the combination of discreteness and continuity, which seems to be involved in the facts (i) that what is speciously present to a person at any moment stretches back for a short period from that moment, with the degree of presentedness tailing off from the later to the earlier extremity of it, and (ii) that nevertheless his experience does not come in successive "jerks" or "pulses;" could best be represented as follows. I supposed that the short slice which is speciously present to him at any moment t , and the short slice which is speciously present to him at a later moment t' , *overlap* to some extent, if and only if t' be near enough to t ; and that the *nearer* t' is to t the *more* does the slice speciously present to him at t' overlap that which was speciously present to him at t . It will be evident therefore that my talk of partial overlapping occurs wholly in a psychological or epistemological context, viz., in reference to the contents *speciously present to a person* at successive instants in his experience. It has nothing to do with the successive phases of a process as such. I have always taken for granted that any process of finite duration can be regarded as composed of, or divisible into, a sequence of successive shorter phases, each adjoined to its immediate predecessor and

its immediate successor without gaps and without overlaps. If a process be completely uniform (e.g., an invariant whistling noise), or if it vary continuously (e.g., a whistling noise changing continuously in pitch), there is indeed no *natural* division into such a sequence of adjoined successive phases, each of finite duration. But there is also no question here of partially overlapping successive phases. So it seems to me that the difficulties raised by Professor Patterson, on the assumption that I hold that any process must consist of a sequence of *partially overlapping* phases, do not arise for me.

I turn now to Professor Russell's essay. This seems to me to give in every instance an extremely fair and adequate summary of what I have written in my *Examination of McTaggart's Philosophy* on the present subject. I will therefore confine myself to a few reflexions which I have been led to make on re-reading those sections in the light of his comments.

(1) *The beginning-to-exist and ceasing-to-exist of a Thing.* I said that I found no particular difficulty in the notions of *beginning-to-exist* and *ceasing-to-exist*, when applied to a thing which is admittedly composed of other things, which enter at a certain time into certain characteristic intimate inter-relations, remain in them for a period, and then gradually or suddenly cease to be inter-related in that particular intimate way. A typical example is provided by an artificial thing, such as a table or a clock. The account would cover also such natural things as, e.g., a crystal of rock-salt. It would need to be considerably elaborated and modified to deal with the case of a living organism, such as an oak-tree or a cat; but I do not think that any fundamental change of principle would be required.

I said, on the other hand, that I found it difficult to make intelligible to myself the notion of a *simple* thing beginning or ceasing to exist. For that purpose I meant by a "simple thing" one that does *not* consist of other things of various kinds inter-related in a characteristic intimate way for a longer or shorter period.

It seems to me now that this needs more careful consideration. Let us confine our attention to *material* things, in a fairly wide sense. We ought, I now think, to begin by distinguishing the following two cases, viz., (i) a thing which is extended but *continuous* and *completely homogeneous*, and (ii) a thing which is supposed to be *literally punctiform*, having position but no extension. As examples of (i) we might take (a) a drop of pure water, as it would be if it were just as it appears and if we ignore all chemical theories about its composition, and (b) an old-fashioned "billiard-ball" atom. As an example of (ii) we can take an atom as it would be on Boscovich's theory.

Now anything that is extended is, in a sense, composite, even if it be completely continuous and homogeneous. It is therefore *in principle* capable of ceasing to exist through the separation of smaller extended parts (all qualitatively exactly like itself and like each other), which were formerly adjoined so that their volumes together exactly made up its volume. It is also *in principle* capable of coming into existence through the coalescence of such things of the same kind, which were formerly separated. No doubt the old-fashioned "billiard-ball" atom was held to be "indivisible." But that was a contingent physical fact about it, or simply a matter of definition.

I think that a difficulty might still be felt about the notion of a completely continuous homogeneous extended thing breaking up *spontaneously* into parts. For it would, so to speak, have no "natural joints." If it is to break up, it must do so in a certain definite way. And, if it is to break up *spontaneously*, it is difficult to see why it should do so in any one rather than in any other of the innumerable ways which are geometrically possible. But, if it were to break up as a result of *external forces*, the configuration of these would no doubt determine the particular way in which it would do so.

Moreover, it seems to me that one could also conceive of another manner in which such a thing could cease to exist or come into existence. This would be by something analogous to *evaporation* or *condensation*, as those processes appear at the level of unsophisticated common-sense, and not as a person familiar with molecular theories would regard them. What I have in mind is this. A billiard-ball atom, e.g., might gradually become smaller and smaller without limit through the literal *annihilation* of one layer after another from circumference to centre, and not through a mere *change of state* without annihilation of stuff as when water changes gradually from the liquid to the gaseous state. Similarly, one could imagine such a thing coming into existence. A billiard-ball atom, e.g., might gradually grow from nothing to its limiting size through the literal *generation* of one layer after another from centre to circumference, and not through a mere *change of state* without generation of stuff as when water changes gradually from the gaseous to the liquid state.

It might be remarked, however, that reflective persons have *not* rested content with the *prima facie* appearances in the case of evaporation and condensation or in that of chemical generation and destruction. This might suggest that there is some intellectual difficulty in the notions of the literal annihilation or generation of a homogeneous continuous extended thing. Against this it might be said that the sophis-

licated interpretation (in terms, e.g., of the physical theory of molecules and the chemical theory of atoms) synthesises a large number of very pervasive facts, which reveal themselves only at a fairly advanced stage of observation and experiment, and that it is *these facts* which originally suggested it and are the only evidence for it. Yet, on the other hand, it should be noted that atomic theories were put forward long before these facts were known or suspected. So one may be inclined to think that they were motivated by difficulties felt in the notions of the literal annihilation or generation of a homogeneous continuous extended thing.

However that may be, I conclude that I must greatly tone down my remarks about the *a priori* objection to the coming-into-existence or the ceasing-to-exist of a simple thing, when "simple" is taken to include as one possibility being extended but completely continuous and homogeneous.

I pass therefore to the case of a thing which is "simple," in the sense of being literally punctiform, e.g., a Boscovichian atom. Obviously it could not come into existence *either* by the coalescence of pre-existing smaller things of the same kind, which were formerly separated, *or* by gradual generation of fresh layers about a centre. Nor could it cease to exist by the converse of either of those processes. Now, in order to count as a "thing," such an existent would have to have *some* dispositional properties. It would be possible to distinguish, at any rate *verbally*, between the following two cases, viz., (i) beginning to exist at *t*, and (ii) having existed before *t* with nothing but unactualised dispositions, and some or all of these being actualised for the first time at *t*. One could also distinguish, *verbally* at any rate, between ceasing to exist at *t*, on the one hand, and, on the other hand, continuing to exist after *t* with nothing but unactualised dispositions which will never again be actualised. By taking the second alternative in each case, one could always avoid admitting in so many words the generation or annihilation of a punctiform thing. But the distinction is certainly very thin, and anyone who is inclined towards a "verificationist" account of meaning, might fairly describe it as insignificant.

I would sum up about things which are simple, in the sense of punctiform, as follows. Since such a thing could not begin or cease to exist in any of the ways which are familiar and seem intelligible to us, there is a temptation (into which I have tended to fall) to say that it is unintelligible that it should begin or cease to exist. A person who wished to maintain that proposition, and yet was presented with *prima facie* instances to the contrary, could always *verbally* save his case by adopting

the expedient mentioned in the preceding paragraph. But whether anything significant corresponds to the distinction which he draws in words, might fairly be doubted.

(2) "*Absolute Process.*" Professor Russell says that I have given an account of "processes" which seems to suggest that a process could not be perceived by more than one person, and to make it doubtful whether there could be unperceived processes. If anything that I wrote gives that impression, it is to that extent misleading, for I certainly had no intention of denying the possibility *either* of a process being perceived by more than one person, *or* of there being unperceived or even imperceptible processes.

I suspect that the misunderstanding may have arisen in the following way. (i) I gave the instance of a *sound* (in the sense of something which has the *sensible* features of pitch, loudness, etc., as actually experienced) as illustrating *in certain respects* what I had in mind when I talked of an "absolute process." (ii) I then remarked that it might be, and has been, doubted whether a sound, in that sense of the word, could exist except as a factor in an auditory experience of some one particular individual on some one particular occasion. I think it is fairly plain that Professor Russell took me to be giving this example as illustrating *in all respects* what I meant by an "absolute process." For he proceeds (quite rightly) to point out that it does not answer all the requirements, in view of the facts or possibilities stated under (ii) above.

The example of a sound (in the sense in question) is useful only in so far as it illustrates the notion of a process which is not at all obviously a *state of invariance* or a *state of change* in the qualities or the relationships of any "thing." Its *prima facie* defect as an illustration (apart from the one just mentioned, which led Professor Russell astray) is that we all believe a sound to be *causally dependent* on processes in what we commonly take to be things, viz., the outer air, the auditory nerves, the brain, and so on. But I do not think that this is really a defect. It was no part of my notion of an "absolute process" that it should not be *causally dependent* on processes which are themselves states of invariance or of change in the qualities or relationships of things. (The use of the word "absolute" may have been misleading here.) The essential point was that it should not *itself be* a state of invariance or of change in the qualities or relationships of any thing. I admit, of course, that the example, even if it were certainly an instance of an absolute process, could not be used in support of the alleged possibility of *dispensing* with the notion of things and describing *all* the facts in terms of absolute processes.

I fully agree with Professor Russell that there is no hope of realising the latter possibility unless we are prepared to admit the existence of absolute processes which are not in fact perceived, and of ones which are in principle imperceptible. But I see no objection to the possibility of such absolute processes.

When physicists talk of electro-magnetic fields, of trains of electro-magnetic waves, and so on, and when they at the same time disclaim all belief in the old-fashioned substantial ether, what *can* they have in mind except what I have called "absolute processes"? If that be so, it would reinforce what Professor Russell contends as to the possibility and the necessity of ascribing *dispositional properties* to absolute processes. Obviously a wireless beam, e.g., carrying a pattern of modulations, has plenty of dispositional properties. And, if there be no substantial ether, these must be ascribed to the *beam itself*, and not to any "thing," of which the beam is a state.

(C) CAUSATION. A part of Professor Blanshard's essay is concerned with questions about causation which are mainly epistemological, and a part of Professor Russell's paper with questions about causation which are mainly analytical. I will take the two in turn.

(1) *Epistemological Questions*. What Professor Blanshard has to say on this topic occurs in his discussion of the power of "intuiting necessary connexions," which is one of the functions that has been ascribed to "Reason." I would make the following comments.

(i) I do not think that there would be any inconsistency in combining the following two views:— (a) That we have *a priori* knowledge of certain *general principles about causation*, and (b) that we have *no a priori* knowledge of any *particular causal law*.

(ii) Again, there seems to me to be no inconsistency, at any rate at the first move, in combining the following two views:—(a) That the notion of causation is (or contains as an essential ingredient) a concept which is *not empirical*, and (b) that our knowledge of any particular causal law *is empirical*.

Suppose, however, that we then raise the question:—Assuming that the notion of causation is not wholly empirical, how do we come to have it? Suppose, further, that we are not content to say that the capacity and the tendency to formulate causal judgments is *innate*, though requiring certain specific kinds of experience to activate and direct it. Then we might be forced to conclude that we must have derived the notion (or at any rate the non-empirical ingredient in it) from being acquainted, in some non-sensuous and non-introspective way, with instances of it. And that would seem to be equivalent to saying that in

some cases we must have had a priori knowledge of particular instances of causal connexion.

(iii) Professor Blanshard gives examples where he thinks it plausible to hold that we do in fact see, by merely reflecting on the natures of the events in question, that any event of a certain kind has at least an *intrinsic tendency* to be accompanied or immediately followed by an event of a certain other kind in a certain relation to it. One alleged example is that an experience of expecting to suffer severe pain has an intrinsic tendency to be accompanied or immediately followed in the same conscious being by an experience of fear.

I admit the *prima facie* plausibility of such examples; but I suspect that the apparent synthetic *a priori* judgment may really be a conflation of one which is *a priori* but analytic with another which is synthetic but empirical. I think that the word "fear" connotes (a) an experience involving certain *feelings* associated with certain bodily states (e.g., a "sinking feeling" in the stomach, the feelings associated with a cold sweat, and so on), and (b) a reference to certain *kinds of situation* (e.g., dangerous, painful, or embarrassing ones). Now it is an *analytic* proposition that a person will tend to experience "fear" (in the sense of that complex of feelings, whatever it may be, which are commonly felt in dangerous or painful or embarrassing situations) when he is or expects to be in such a situation. It is a *synthetic* proposition that he will tend to experience "fear" (in the sense of a certain familiar complex experience, including a sinking feeling in the stomach, the feeling associated with a cold sweat, and so on) when he is or expects to be in a dangerous or painful or embarrassing situation. And the latter proposition seems to me to be purely contingent. But, since the word "fear" combines *both* these features in its connotation, we are liable to think that we are contemplating a *single* proposition, which is *both* synthetic and necessary. I have taken one particular example, but I have a strong suspicion that any other example adduced for the same purpose could be dealt with on the same lines.

(2) *Analytical Questions.* Professor Russell very justifiably finds much that is obscure in my remarks about causation in *Examination of McTaggart's Philosophy* Vol. I Chapter XIII. He tries to lighten the darkness by restating what I may have had in mind in terms of the notions of necessary condition and sufficient condition. I am sure that that is the right course. I have pursued it myself in later writings. I think that the simplest way for me to clear up the matter is to begin by giving some definitions and making some statements based on the contents of Pp. 15 to 18 of the first of my papers entitled, "Hr. von Wright on the Logic of Induction," in *Mind*, Vol. LIII.

(i) *P* is a *sufficient precursor* ("S.Pr.") of *Q*, if from any instant into which a *P*-event were to enter a *Q*-event would issue.

(ii) *P* is a *necessary precursor* ("N.Pr.") of *Q*, if into any instant from which a *Q*-event were to issue a *P*-event would have entered.

(iii) *P* is a *smallest sufficient precursor* ("S.S.Pr.") of *Q*, if (a) it is a sufficient precursor of *Q*, and (b) it is either (α) a simple characteristic *p*, or (β) a conjunctive characteristic $p_1 \& p_2 \& \dots \& p_n$, such that, if any of the conjuncts be omitted, what remains is *not* a sufficient precursor of *Q*.

(iv) A *contributory precursive condition* ("Cy.Pr.Cn.") of *Q* is any simple characteristic, or any conjunction of such characteristics, which is a conjunct in a S.S.Pr. of *Q*.

(v) *P* is a *smallest necessary precursor* ("S.N.Pr.") of *Q*, if (a) it is a necessary precursor of *Q*, and (b) it is either (α) a simple characteristic *p*, or (β) a *disjunctive* characteristic $p_1 \text{ or } p_2 \text{ or } \dots \text{ or } p_n$, such that, if any of the alternants be omitted, what remains is *not* a necessary precursor of *Q*.

(vi) A *substitutable precursive requirement* ("Sb.Pr.Rq.") of *Q* is any simple characteristic, or any disjunction of such characteristics, which is an alternant in any S.N.Pr. of *Q*.

(vii) If *Q* has *only one* S.S.Pr., every conjunct in it may be described as an *indispensable contributory precursive condition* ("I.Cy.Pr.Cn.") of *Q*. If, on the other hand, *Q* has several alternative S.S.Pr.'s, then any characteristic which is a *conjunct in all of them* may be so described.

So much by way of definition. It is important to be clear about the logical relationships of the two notions of *necessary precursor* and *indispensable contributory precursive condition*.

(a) It is logically possible for *P* to be an I.Cy.Pr.Cn. of *Q* *without being* a N.Pr. of *Q*. For, whether there be only one S.S.Pr. of *Q* or several alternative S.S.Pr.'s of *Q*, it remains *logically possible* that there should be cases in which a *Q*-event issues from an instant into which *no* S.S.Pr. of *Q* has entered. Now, in order for *P* to be a N.Pr. of *Q*, a *P*-event would have to enter into *every* instant from which a *Q*-event issues. So a *P*-event would have to enter *inter alia* into those instants (if any) from which a *Q*-event issues *without any* S.S.Pr. of *Q* having entered. But, in order for *P* to be an I.Cy.Pr.Cn. of *Q*, it has only to be a conjunct in *every* S.S.Pr. of *Q*. Obviously that does not guarantee the entry of a *P*-event into those instants from which a *Q*-event issues *without any* S.S.Pr. of *Q* having entered. Since it is logically possible that there should be such instants, it is logically possible for *P* to be an I.Cy.Pr.Cn. of *Q* *without being* a N.Pr. of *Q*.

(b) This possibility would be ruled out, if and only if we were to

assume that in *every* case in which a *Q*-event issues from an instant there is *some* S.S.Pr. of *Q*. This might be called the *Postulate of Smallest Sufficient Precursors*. On that assumption any I.Cy.Pr.Cn. of *Q* must be a N.Pr. of *Q*.

(c) It is logically necessary that a conjunction of all the I.Cy.Pr.Cn's of *Q* should be a S.Pr. of *Q*. But it is *not* logically necessary that a conjunction of all the N.Pr's of *Q* should be a S.Pr. of *Q*. The latter proposition would, however, follow from the Postulate of Smallest Sufficient Precursors.

Now it is certain that neither the above distinctions, nor consequently the logical relations between the notions distinguished, were clearly before my mind when I wrote the chapter on Causation. In terms of them I will make the following comments on certain things which I wrote there.

(i) I think that what is generally understood by the phrase "*total cause* of such and such a change issuing from an instant *t*" is a S.S.Pr. for changes of that kind. Therefore the most obvious interpretation of the sentence: "All changes of such and such a kind are caused" would be that in every case where a change of that kind issues from an instant there is a S.S.Pr. for it. That would be quite consistent with holding that there is a *plurality* of alternative S.S.Pr's for changes of that kind; that in some cases one is present and in other cases another; and that perhaps in some cases several of them are present together. The most obvious interpretation of the sentence: "*All changes whatever* are caused" would be a generalisation of the above statement about all changes of *such and such a kind*. It would in fact be the Postulate of Smallest Sufficient Precursors.

(ii) Any reader might be excused for thinking that it was *this* proposition which I claimed to find self-evident when I wrote (*Examination* Vol. I P. 232) "Every change has a cause," and said that this was to me evidently true. But in fact I did *not*, and do *not*, find it self-evident that for every case in which a change of any kind issues from an instant there must be a S.S.Pr. for a change of that kind issuing from that instant.

If the reader should continue until he reaches the discussion of voluntary decision on P. 238 of the volume in question, he will find that what I there claim to be self-evident would be expressed (at any rate to a first approximation) by the following sentence:— "In every case in which a change of any kind issues from an instant there must be a change entering into that instant, such that a change of the former kind *would not have issued unless* one of the latter kind had entered."

Now this, as Professor Russell rightly points out, is an assertion about *necessary* precursors, and not about sufficient precursors.

The above statement needs a certain amount of elucidatory comment, in view of the fact (which I did not recognise at the time) that a N.Pr. need not be simple, and that the S.N.Pr. for a given kind of change may be a *disjunction*. (I owe the recognition of this to Professor von Wright.)

It might be that an event of the *Q*-kind would not issue from any instant unless an event of a certain kind *P*₁-or-*P*₂ should have entered into that instant, but that in some cases the entering event is of the kind *P*₁ and there is none of the kind *P*₂, that in others it is of the kind *P*₂ and there is none of the kind *P*₁, and that in yet others perhaps there is either a single entering event of the two kinds or two entering events one of each kind.

What I claimed to find self-evident might therefore be re-stated as follows. The issuing of an event of any given kind (say *Q*) from any instant must be preceded by the entry into that instant of an event which is either (a) of a certain *one* kind (the same in all such cases), or (b) of one or another of a *certain limited number* of alternative kinds (in some such cases of one, and in other such cases of another, of these alternative kinds.)

I think I may say of this proposition the following two things. (a) The contradictory of it is certainly not self-contradictory. (b) When I reflect on the contradictory of it, and try to consider "what such a state of affairs would be like," I find it almost impossible to think that it could be true.

(iii) I should not now be inclined to attach much, if any, weight to the proposition which I asserted, at the bottom of P. 233 of Vol. I of *Examination*, to be self-evident. This to the effect that a given change issuing from a given instant cannot have "more than one total cause." I should now identify "a total cause" of a particular change with any S.S.Pr. of such changes which enters into the instant from which that change issues. If there should be only one S.S.Pr. entering into the instant in question, we can talk of "*the* total cause" of the change on that occasion. But such a change may have several alternative S.S.Pr.'s, and it does not seem to me self-evidently impossible that more than one of them should enter into a given instant from which such a change issues. In that case, it seems to me, we must be content to say that the particular change in question has *several coexisting* total causes, and therefore that there is nothing that can be called "*the* total cause" of it. I should describe such a change as "over-determined." It would be easy to produce quite plausible *prima facie* instances of over-determination.

(V) *Induction and Laws of Nature*

It will be convenient to discuss this topic immediately after the above discussion of the notion of Causation. It forms the subject of essays by Dr. Hanson, Professor Nelson, and Professor von Wright.

(A) LAWS OF NATURE. I take it that the main point which Dr. Hanson is concerned to make is that "law-sentences" (and in particular those which are said to state the laws of motion and the law of gravitation) have a number of different, though interconnected, uses; that the same law-sentence is often used in different ways by one and the same scientist in the course of a single spell of work or a single bit of scientific writing; and that it is futile for anyone philosophising on the topic of these laws to pick out one sense, and claim that it is the only legitimate one. I certainly do not wish to dispute this, and I will content myself with the following remarks on it.

(1) The question of fact could be settled only by a careful examination of the writings, the conversations, and the behaviour on relevant occasions of representative scientists from Newton's time to the present day.

(2) I suspect that it would often be very hard to be sure of the sense in which a given scientist was using a given law-sentence on a given occasion. I should not expect to get much useful information by asking the scientist himself. If he had *not* had a philosophical training, he would probably not understand the question or see the point of asking it, and he would certainly not have the technical equipment to answer it intelligibly. If he *had* had a philosophical training, the chances are that he would not be a first-rate working scientist; and, even if he were, he would probably be committed to some particular (and often already exploded) philosophical view, which would bias his answers. In fact, those two modern oracles, "the plain man" and "the working scientist," resemble in one respect their ancient forerunners. The artless prattlings of the former and the sophisticated technicalities of the latter stand in as much need of expert interpretation as did the inspired ravings of the Pythia at Delphi or the Sybil at Cumae.

(3) If the question of fact can be settled, I agree that it is then the business of the philosopher of science to accept the situation, and not to pretend that there is one and only one legitimate sense in which a law-sentence can be used.

Dr. Hanson mentions five different uses of a law-sentence. The second, third, and fourth of these agree in that in each of them such a sentence expresses something that can significantly be described as "true" or "false." In the first use such a sentence formulates a *definition* of the

technical meaning of a term, e.g., "force," which may already have had a long-standing use in ordinary speech. In that case it can hardly be said to be true or false, but it can be judged by various criteria to be well or ill fitted for its purpose. The fifth use itself covers five very different alternatives, according to Dr. Hanson; but they all agree in making a law-sentence express something which (though not a definition) can hardly be said to be true or false.

On all these matters I will confine myself to the following comments.

(1) Dr. Hanson distinguishes, among others, the following uses of a law-sentence, viz., (i) to express a proposition which it is "psychologically impossible" to think of as false, and (ii) to express a proposition, the rejection of which would have extremely upsetting repercussions in departments of science which have for long been regarded as models of complete and detailed explanation and absolutely reliable prediction.

I should suppose that the state of affairs described under (ii) is an important factor in causing that which is described under (i). The alleged psychological impossibility of contemplating the falsity of what is expressed by a law-sentence would seldom be of much philosophical interest unless it sprang from some such cause.

(2) In his discussion of the attitudes of scientists toward the law of gravitation, Dr. Hanson makes the following remark. A most important function of such a law is that it unifies a great many empirical facts through being a *common premiss* from which they all follow. He speaks of the typical situation in modern theoretical physics as "observation-statements in search of a premiss." And he alleges that philosophers of science have failed to recognise this, and have concentrated their attention too much on empirical correlations. By these I take him to mean straightforward inductive arguments from all the observed *S*'s having been *P* to the conclusion that all past, present, and future *S*'s respectively have been, are, or will be *P*.

My comments on this are as follows. (i) I agree that the procedure in question is most important in all advanced sciences. (ii) I do not agree that it has been ignored or underrated by philosophers of science. I should have thought that it was adequately recognised, e.g., by Mill in his *Logic* (Cf. Book III, Chapters XI to XIV, both inclusive) and by Jevons in his *Principles of Science*, to mention only two of the older English writers.

(iii) As a matter of logic, it seems to me that philosophers of science nevertheless do well to lay great stress on the problem of straightforward inductive generalisation. For all the "facts," which are shown to follow from the supposed law, are themselves propositions which have been

accepted as straightforward inductive generalisations. If the original inductive arguments for accepting them cannot be defended, then there is to that extent a doubt whether they *are* facts; and, unless they are, they cannot support the more general law which entails them.

Of course I am well aware that the evidence for each of these more restricted general propositions is greatly strengthened by its being entailed by a single more general law, which also entails many other such propositions, each of which was originally accepted only as an independent inductive generalisation. In this way the whole system becomes comparable to a net, as contrasted with a lot of separate threads. But the net is made of threads, and the ultimate strength or weakness of the threads depends in the last resort on the validity or invalidity of straightforward inductive generalisation.

(3) Among what I may call the "non-informative" uses of law-sentences Dr. Hanson mentions their use (a) as rules of inference, and (b) as principles for constructing instruments. It seems to me that both of these *presuppose* an "informative" use of such sentences, i.e., to state a proposition which can significantly be described as "true" or as "false," and which is in fact held to be *true*.

This is surely obvious when it is a question of constructing an instrument. A sane person constructs an instrument to perform certain functions, and he designs it in such a way as he believes will ensure its performing those functions efficiently. If he uses a law-sentence prescriptively in giving directions for constructing an instrument, or if he understands it in that sense in receiving and following such directions, it must be because he already accepts as true the propositions about nature which it states when used, not prescriptively, but informatively.

This is perhaps less obvious in reference to the use of a law-sentence as a *rule of inference*. If so, that is because it is not altogether clear what that phrase means in the present connexion. But surely this at least can be said. Such a "principle" is admittedly *not* one of pure logic or of pure mathematics, like the principle of the syllogism or the formula $(x + y)(x - y) = x^2 - y^2$. If, then, the conclusions which one "deduces in accordance with it" are to have any relevance to actual or causally possible natural phenomena, the "principle of inference" must surely rest on a general proposition which is held to be *true* of the relevant department or aspect of nature.

(B) **INDUCTION.** The topics treated in Professor Nelson's and Professor von Wright's essays partly overlap and partly diverge, so in some of the sub-sections which follow I shall be concerned with what is common to both and in others with what is peculiar to one or the other.

(1) *The so-called "Problem of Induction."* This question is treated

by both writers. Professor von Wright quotes a dilemma, in which I summed up my position in the paper entitled "Mechanical and Teleological Causation," in Aristotelian Society's *Supplementary Volume XIV*. He suggests that, instead of pursuing the course which seems inevitably to end in that dilemma, we should begin with the question: What do we *mean* by calling an inductive belief "grounded" (as opposed to "groundless" or "ill-founded"), and what do we *mean* by "rationally believing" in reference to an inductive generalisation? He is inclined to think that, if we do this, we shall see that what we call "grounds of rational belief in induction" are *just* empirical premisses *without* support of any general principles. He does not attempt to argue his case, but hopes that I may comment on it. So I will take this as the text of my sermon in this sub-section.

I would suggest that what must presumably have happened in the case of *deductive* logic may be useful as an analogy and a contrast. Here, I suppose, we could distinguish in theory three stages, though very likely they overlapped historically.

(1) There would have been a number of particular bits of deductive argument which all or most sane persons *accepted* in the law-courts, in monetary calculations, in mensuration, and so on, except when under the influence of some strong desire or emotion which was known to distort a person's judgment. There would have been a number of particular bits of deductive argument which all or most sane persons, with similar qualifications, *rejected*. Finally, there would be a number of particular bits of deductive argument which were accepted by some and rejected by other sane persons when in an emotionally calm state.

(2) It would be natural, then, to compare and contrast the generally accepted with the generally rejected arguments, in order to see whether there were other features, beside general acceptance, common and peculiar to the former. This stage might be illustrated by the discovery and formulation of the traditional rules of the syllogism. At this stage it might be agreed to be a fair test, in the case of a disputed argument, to note whether it did or did not have the characteristics which had been found to be in fact common and peculiar to arguments commonly accepted by sane men in their calmer moments.

(3) One might still, however, see *no reason why* an argument having all the characteristics in question should be valid, and *why* one which lacked any of them should be invalid. There is nothing, e.g., *obviously* wrong with a syllogism having a negative conclusion and two affirmative premisses. The next stage, then, would be to try to get behind the empirical tests, and to show that they are consequences of more fundamental principles which are *self-evident*. That can be done in various alter-

native ways, which I need not describe here, for the rules of the syllogism.

Let us now compare and contrast this with the case of inductive arguments. In the case of *deductive* inference we are all, I suppose, agreed as to what we *mean* by calling an argument "valid." At any rate there is one condition which would generally be acknowledged to be necessary and sufficient for the validity of a *deductive* argument. It is this. It must be *impossible* that the premisses should be true and the conclusion false; and this impossibility must rest, not on the impossibility of the premisses (though they *may* be impossible, as in a *reductio ad absurdum* argument in pure mathematics), nor on the necessity of the conclusion (though it *may* be necessary, as it always is in the case of any valid argument from true premisses in pure mathematics), but on a certain relationship between the *logical form* of the premisses and the *logical form* of the conclusion. The task for philosophers of *deduction* is to classify arguments which answer to this admittedly necessary and sufficient condition of validity; to elicit the formal features common and peculiar to them; and then, if possible, to bring them under one or a few general principles, which all or nearly all sane and competent persons find self-evident. That programme has in the main been accomplished.

But it is not obvious what we *mean* by calling an *inductive* argument "valid"; or, if you prefer it, there is no one condition which is generally acknowledged to be necessary and sufficient for the validity of such an argument. What is quite certain is this. If we use the accepted definition or criterion of "validity" as applied to a *deductive* argument, and if we take the *complete* premiss of an *inductive* argument to be: *This, that, and the other S (which are all that have so far been observed) have been P, and the conclusion to be: All S's, past, present or future, respectively have been, are, or will be P, then all inductive arguments are invalid.* Now we all use inductive arguments, and we all accept the conclusions of many of them and guide important actions by reference to these. So reflective persons cannot but find this situation intellectually disturbing.

Now at this point there seem to be two alternatives open to us. One is to suppose that the definition or accepted necessary and sufficient condition of "validity," as applied to *deductive* arguments, applies also to *inductive* ones. The other is to deny this, and to set out from that point. I will now say something about each of these alternatives in turn.

(1) If we are going to use the old definition or accepted necessary and sufficient condition of "validity," and yet to admit the possibility that some inductive arguments are valid, we must try to save the situation in one or other or a combination of the following ways. We might suppose either (i) that a valid inductive argument has an additional *implicit premiss* beside the instancial propositions which are its only explicit

premisses, or (ii) that the *conclusion* of a valid inductive argument must take a *weaker form* than the unqualified *All S is P*. I think that it is now quite plain that anything on these lines needs *both* expedients in order to be at all hopeful, viz., adding some kind of universal premiss to the explicit instancial premisses, and stating the conclusion in terms of probability.

If that were done, it is evident that the *principles* (as distinct from the premisses) of inductive inference would include, beside those of non-problematic deductive inference, at least the formal principles of the calculus of probability, e.g., the axiom of addition concerning the probability of a disjunctive proposition, and the axiom of multiplication concerning that of a conjunctive one. I do not think that this in itself would be felt to raise any special difficulty.

Anyone who follows this line will have to deal with the following three questions, which might be described respectively as "logical," "ontological," and "epistemological." (i) What are the minimal universal premisses which, if added to the explicit instancial premisses, would make very highly probable the conclusions of those inductive arguments which are commonly accepted as practically certain by sane and instructed persons? (ii) What account of the structure of nature as a whole, or of certain departments of nature, would best fit in with the assumed truth of these universal premisses? (iii) How, if at all, do we know that these premisses are true or that they are highly probable? I think that this agrees almost exactly with the scheme outlined by Professor Nelson.

Before considering the other alternative, suggested by Professor von Wright, I will make the following comments on the alternative outlined above.

(a) There is no guarantee that the whole enterprise may not break down at the first stage. In that case we should have to admit that, so far as we can tell up to date, *no* inductive arguments are valid, in the sense of "validity" supposed, even when their conclusions are stated in terms of probability.

(b) Even if the logical problem can be solved satisfactorily, the epistemological problem would (as both Professor Nelson and I have emphasised) remain very troublesome. The additional premiss (and still more obviously the propositions about the "structure of nature" which have to be assumed in order that it shall be applicable) must be *general*, and it cannot be *merely analytic*. Yet our acceptance of it cannot, without circulatory, be based on *induction*; and, even if the possibility of necessary synthetic propositions were admitted (which it is not by most contemporary English and American philosophers), no additional

premiss which has been plausibly alleged to fulfill the conditions has any trace of *self-evidence*.

(c) This leads me to the following two reflexions, (α) Even if the epistemological difficulties should be insoluble, that would not diminish the value of the analytic and the ontological sections of this line of thought. The justification of induction, where it is thought to be justifiable, would have to be stated *conditionally*, and not categorically. But even that would be no small gain in insight. (β) The situation would be remarkably like that which Kant (as I understand him) contemplated in regard to such allegedly synthetic *a priori* propositions as he held to be capable of "transcendental proof." People claim to *know*, or to have *good grounds for very strongly believing* certain general propositions as a result of inductive reasoning. Suppose we grant their claim. Suppose we can show that it can be valid, *if and only if* certain propositions about the structure of nature are true. Then we are entitled to accept those propositions, even though they be synthetic and though they have no trace of self-evidence. They would be "synthetic *a priori* propositions" in precisely the sense in which Kant held that, e.g., the law of universal causation and the conservation of mass are so.

(2) Let us now consider the other alternative, suggested by Professor von Wright, which is nowadays much the more popular of the two. The contention is that if an *inductive* argument can properly be described as "valid" or "invalid," those words must be understood in a special sense, appropriate to such arguments. On that supposition, it is of course quite possible that certain inductive arguments may be "valid," in the appropriate sense, *without* the addition of any implicit general premiss to their explicit instantial premisses, and perhaps without reformulating their conclusions in terms of probability. On this suggestion I would make the following comments.

(i) Plainly the first task would be to formulate a definition, or generally acceptable necessary and sufficient condition, of what I will call "inductive validity." Here we may compare and contrast this enquiry with Stage (1) of what I supposed above to have happened in the case of *deductive* arguments. We should have to consider typical *inductive* arguments, which all or most sane persons in their calmer moments accept, and compare and contrast them with typical inductive arguments which all or most of such persons under such conditions reject. But the difference would be this. In the case of *deductive* arguments there was from the outset no doubt as to what is *meant* by "valid" and "invalid" as applied to them. The object of the comparison and the contrast was not to elicit the *meaning* of "validity," but to discover, and if possible to rationalise, *tests* for its presence or absence in any deductive

argument. But in the case of *inductive* arguments the primary object of this comparison and contrast would be to discover what competent persons, who use and criticise such arguments, *mean* when they call some of them "valid" and others "invalid."

Unless it turned out that inductive validity had some fairly close and important analogies to deductive validity, it would be better not to use the word "validity" or "invalidity" of inductive arguments, but to coin some other technical term. I should think that the irreducible minimum of analogy would be that the "validity" of an inductive argument should depend in some assignable way on relationships of *logical form* between its premisses and its conclusion.

(ii) However that may be, it might still be worth while, after having elicited an agreed definition of "inductive validity" in this or in some other way, to proceed thenceforth as logicians did with deductive arguments. That procedure would be as follows. (a) To try to discover features, other than those which enter into the definition of "inductive validity," which are common and peculiar to arguments which are inductively valid. (b) If that can be done, to try to show *why* the presence of all these features entails inductive validity, and the absence of any of them entails inductive invalidity. (c) To try to reduce these features as far as possible to one or a few very general headings. If all this could be accomplished, there would remain the following typically philosophical questions. What is the nature of the ultimate principles on which the tests for inductive validity rest? Are all of them analytic, or are some of them synthetic? If some of them are synthetic, how are they known or rationally believed to be true?

(iii) Now it *might* happen that, when one elicited the meaning of "inductive validity," the consequence which Professor von Wright thinks would follow, viz., that the grounds of rational belief in induction are *just* empirical premisses *without* support of any general principles, would be seen to follow. Or it might not. All that I will say in conclusion is this. We must of course distinguish between the *premisses* of a valid argument, and the *principles* which the argument exemplifies and which ensure and make evident its validity. In the valid syllogism, e.g., *All men are mortal, and all Greeks are men, therefore all Greeks are mortal* the only *premisses* are the two propositions which are stated before the word "therefore." The *principles* which the argument exemplifies, and which together make evident its validity, are such propositions as the following:—(a) If a class is empty, every sub-class of it is empty; and (b) If every member of an exhaustive set of sub-classes of a class is empty, then that class is empty. Now I should think it certain that, if there are any *principles* for the "validity" of inductive arguments

(no matter what meaning be attached to "inductive validity"), they must be *general* propositions. But that would leave open the question whether a valid deductive argument does or does not have to include one or more general propositions among its *premisses*.

(2) *Professor Nelson's account of Inductive Argument.* I found this of very great interest. I will first try to state it, as I understand it, in my own words, and will then make a few comments on it. To simplify the exposition I will confine myself to inductive generalisation where the instancial premiss is that N instances of S have been observed (say S_1, S_2, \dots, S_N) and that all of them have been P . With that understanding I would summarise Professor Nelson's theory as follows.

(i) If the argument is to be defensible, the conclusion must not take the unqualified non-modal form *All S is P*. It must take the form *It is likely, to such and such a degree, that all S is P*.

(ii) This must be carefully distinguished from any statement of the form: "The proposition *All S is P* has such and such a degree of probability with respect to the datum q ." The following points are very important to notice here. (a) "Likely," in the sense in which Professor Nelson uses it, is analogous (except in that what it stands for is capable of degree) to "true." (b) On the other hand, the statement that p has such and such a degree of probability with respect to q is comparable to the statement that p is entailed by q . Like it, it is a statement which is *necessarily* true or *necessarily* false, as the case may be. And, like it, its truth or falsity depends on certain relations between the *forms* of p and of q , and not on their individual necessity or impossibility, truth or falsity, likelihood or unlikelihood.

(iii) Nevertheless, in order to establish inductively the conclusion *It is likely to such and such a degree that all S is P*, we require such a proposition as is expressed by the sentence: "With respect to the proposition that N instances of S have been observed and all of them have been P , it is probable to such and such a degree that all S is P ." I will symbolise the proposition, expressed by the sentence in inverted commas, by $\Pi_N(\sigma)$ where σ is the degree of probability in question.

(iv) The part played by $\Pi_N(\sigma)$ in establishing a conclusion *inductively* may be compared with that which is played, in establishing *deductively* that all Greeks are mortal from the premiss that all men are mortal and all Greeks are men, by the proposition which is expressed by the sentence: "*All men are mortal & All Greeks are men* entails *All Greeks are mortal*." The following points are important to notice here:—

(a) In the deductive argument we use a principle of "Deductive Detachment." Knowing that in fact all men *are* mortal and all Greeks *are* men, we are entitled to drop those premisses and to accept as *true* the

proposition that all Greeks are mortal. In the inductive argument we need a comparable principle of "Inductive Detachment." Knowing that in fact N instances of S have been examined and that all of them were P , we are entitled to drop that premiss and to accept as *likely to such and such a degree* that all S is P .

(b) According to Professor Nelson, the degree of likelihood which it is justifiable to assign to *All S is P* , under the conditions supposed, is a function of the degree of probability σ , which *All S is P* has with respect to the premisses in the complex proposition $\Pi_N(\sigma)$. As to this function he will say no more than the following. The degree of likelihood of *All S is P* , given that the premisses in $\Pi_N(\sigma)$ are known to be true and can therefore be dropped, increases with σ , the degree of probability of *All S is P* in respect to those premisses.

(v) The last point in the theory is this. Professor Nelson holds that we never have any good reason to accept such a proposition as $\Pi_N(\sigma)$ on its own merits, as we have, e.g., to accept the proposition expressed by the sentence: "*All Greeks are mortal* is entailed by *All men are mortal & All Greeks are men.*" The only ground for accepting such a proposition as $\Pi_N(\sigma)$ is that it is entailed by a certain other proposition, which he calls the "Principle of Induction," and that we know this to be true. We will denote this principle by P_I . It is important to note the following points about it. (a) Professor Nelson does not claim to be able to formulate it satisfactorily. But he thinks that progress has been made towards doing so, and that this is illustrated, e.g., by the substitution of Keynes's "Principle of Limited Variety" for Mill's "Uniformity of Nature." (b) He draws a distinction between P_I itself, and the characteristics which we must ascribe to the actual world if P_I is to be true and applicable to natural phenomena. The proposition that nature has these characteristics is ontological, whilst P_I itself is described as "formal."

Supposing this to be a fair account of Professor Nelson's very interesting theory, I will make the following comments.

(i) I wonder why he uses P_I as a premiss which entails $\Pi_N(\sigma)$, instead of modifying $\Pi_N(\sigma)$ by introducing P_I into it as an additional premiss. The modified proposition, which we will denote by $\Pi'_N(\sigma)$, would then be expressed by the sentence: "The proposition *All S is P* has probability of degree σ with respect to the conjunction of P_I with the premisses of $\Pi_N(\sigma)$." I do not see any obvious objection to this. And, unless there be some objection, I should think it would have one obvious advantage. For $\Pi'_N(\sigma)$ would hold in virtue of the *form* of its premisses and its conclusion, just as a valid syllogism does; whilst $\Pi_N(\sigma)$ would not do so (if I understand Professor Nelson aright), though I suppose that the proposition that P_I entails $\Pi_N(\sigma)$ would do so.

(ii) Professor Nelson puts the argument in terms of a definite degree of probability σ , and a definite degree of likelihood, which increases with σ . I take it that he does not suppose that these can be exactly measured in any particular case. It would be enough that in favourable cases one should know that σ was high enough to ensure that the degree of likelihood of *All S is P* is considerable.

(iii) As regards P_1 itself I have two remarks to make. (a) Taking it as a "formal" principle, I feel rather uncomfortable about a premiss which it is admitted that no one has so far managed to formulate satisfactorily. In order to "detach" P_1 in Professor Nelson's form of the argument, one must *know* that it is true (or at any rate "highly likely"). But unless one knows what it is, how can one know this about it? I suppose we should have to say that what one knows is that there is *some* formulable proposition (never as yet satisfactorily formulated), which has the logical properties ascribed to P_1 and which is true or highly likely.

(b) I think that the distinction between the "formal" principle and its ontological ground might be rather difficult to define. Would it come to this? The *formal* principle would state in extremely abstract terms the conditions which must be fulfilled in *any possible world* in which inductive generalisation would be a valid process leading in favourable cases to highly likely conclusions. The *ontological* principle would be a much more concrete statement as to the structure of the *actual world* which ensures that these conditions are fulfilled in it.

(iv) On the notion of "likelihood" I will make the following comments:—

(a) When a person accepts a proposition (rightly or wrongly, reasonably or unreasonably) as *true*, he is prepared (so far as he is not hindered by temperamental or occasional defects, intellectual or moral) to apply it without hesitation in practice where it is relevant, to accept without question in theory any consequences which seem to him to follow from it, to use it unhesitatingly as a basis for his further reflexions and investigations, and so on. Now there is undoubtedly an attitude which we often have towards a proposition, where all this holds good with the substitution of "with very considerable confidence" for "unhesitatingly." The latter may fairly be described as accepting a proposition (justifiably or unjustifiably) as *more or less likely*.

(b) One important way in which a person comes to accept a proposition as *true* is by noting that it seems to him to be logically entailed by certain other propositions, which he accepts as true. In such cases we may say that he accepts it as "deductively established." One important way in which a person comes to accept a general proposition as *more or less likely* is by what he takes to be a valid inductive argu-

ment from premisses which he accepts as true. These always include *at least* a number of favourably instancial propositions, together with a proposition to the effect that these are all the relevant instances that have been observed. In such cases we may say that he accepts it as "inductively supported."

(c) If a person accepts a proposition as *true*, because *deductively established*, he cannot hope to strengthen his case through the possible discovery of additional true propositions which entail that conclusion. These will only provide him with *alternative* lines of proof, all of which could be dispensed with, and each of which could be substituted for his original line of proof. They are like a lot of ropes, each attached to a different hook, and each amply sufficient to support a certain weight. But suppose a person accepts a general proposition as *likely to at least a certain degree*, because *inductively supported*. Then he can hope to strengthen his case (though he must also fear its complete collapse) by the examination of further relevant instances. The mere addition of further true premisses of the same kind (provided that the proposition that they include all the observed instances remains true) will inductively support the conclusion *still more strongly* and will justify one in accepting it as *likely to a still higher degree*. Here the additional true premisses are comparable to additional strands in a single rope, which is always liable suddenly to give way.

(v) Lastly, I would like to say how fully I agree with the following contention of Professor Nelson's. It is hopeless to consider the principles of induction in isolation from the other principles and categories which are involved in the notion of a world of persistent things with varying states, co-existing and inter-acting in a single spatio-temporal system. Whatever defects there may be in Kant's discussion of the "Principles of Pure Understanding," he had at least grasped this essential point, which his predecessors had failed to note and which most of his successors seem to have forgotten.

(3) *Assumptions about Antecedent Probability*. Professor von Wright discusses this in connexion with problems in probability concerned with drawing counters from a bag, noting their colours, and thence arguing to the probability of various propositions about the colours of the counters in the bag. I have considered such problems in "Induction and Probability" and in "The Principles of Problematic Induction".

In the former I assumed that the $n+1$ alternatives, that a bag containing n counters should contain 0 or 1 or n counters of an assigned colour (e.g., white), would be equi-probable antecedently to any of them being drawn and looked at. In the latter paper, after having read Keynes's *Treatise on Probability*, I argued that this assumption leads

to a contradiction. I there assumed instead that there are v distinguishable colours (including black and white), and that it is antecedently equiprobable with regard to any counter in the bag that it would have any one of these colours. Professor von Wright mentions a third possible assumption, which I did not consider in either paper, viz., that every possible "constitution" of the contents of the bag with respect to an assigned colour (e.g., white) is antecedently equally probable. He expresses regret that I did not work out the consequences of this.

Now I think that this third possible assumption can be dismissed quite briefly. I take it to be equivalent to assuming that it is antecedently equiprobable with regard to any counter in the bag that it would either *have* or *not have* the assigned colour (e.g., white). For all purposes of mathematical deduction that is equivalent to putting $\sqrt{=2}$ in the calculations in "Principles of Problematic Induction". It seems to me obvious that the assumption as to equiprobability which I made there is more defensible than the assumption of equiprobability of "constitutions." For the latter lumps together under the heading "other-than-white" all the remaining colours, and then counts this *disjunction of colours* as precisely on a level with the *single colour* white.

Professor von Wright says that he thinks there is no possibility of proving or of disproving any of these alternative assumptions about equiprobability. I am inclined to agree with him as to the impossibility of *proving* any of them without making factual assumptions. I think, e.g., that the assumption which I made in *P. of P. I.* would be reasonable only if one had the following information, or something formally equivalent to it, viz., that the bag had been filled by drawing n counters from *another* bag, which contained *equal large numbers* of counters of each of the v colours, well mixed with each other. But I should have thought that it was possible to *refute* some assumptions by showing that they lead to consequences which are plainly absurd. I do not see anything wrong with the argument by which I tried to show in *P. of P. I.* that the assumption made by me in "Induction and Probability" leads to absurdities, if we admit that there is *more than one colour* (e.g., red and blue) besides the assigned one (e.g., white), which might belong to one or more of the counters in the bag.

(4) *The notion of "Loading."* From problems concerned with drawing counters from bags the transition is natural to problems concerned with throwing dice, spinning roulette-wheels, and so on. The notion of "loading" has its most obvious applications in reference to the latter problems. It is discussed both by Professor von Wright and by Professor Nelson. I will take their remarks in turn.

In *P.P.I.* I made the following assertions. (i) "The notion of loading

is the notion of a constant cause-factor which operates throughout the whole series of throws, and co-operates with other and variable cause-factors to determine the actual result of each throw." (ii) "I shall say that the counter is loaded to degree s in favour of red, if and only if the antecedent probability of its turning up red would be s for anyone who knew in detail how it was constructed." Professor von Wright finds this obscure. He says that he would understand by "load" a certain *antecedent probability*. And he asks whether I suppose the "constant cause-factor" to be this probability *itself* or some feature in the *physical world* which may be held *responsible* for the "load," in his sense.

The answer is that I meant the following. I thought of the load, *not* as a probability, but as a *physical* factor (e.g., the location of the centre of gravity at such and such a position in relation to the geometrical centre of the body in question) *determining* the antecedent probability of a face of such and such a colour coming up. It would strike me as linguistically barbarous to talk of a *probability* as a cause-factor, and I should not wittingly do so.

My statement that induction, in such cases, presupposes a reference to causation was therefore intended to mean something different from the minimum which Professor von Wright suggests that I might have meant by it. In the context it was intended to mean something like the following. The fact that the antecedent probability of a loaded die turning up a 6 on any occasion is so-and-so is determined jointly by the following facts. (i) That the position at which it comes to rest on any occasion is causally determined jointly by (a) the position of its centre of gravity in relation to its geometrical centre, (b) its geometrical, elastic, and other permanent properties, (c) the correlative properties of the surface on which it falls, and (d) the angle at which it hits the surface. (ii) That it is antecedently equally likely to hit the surface at any one of the innumerable alternative geometrically possible angles. (I suspect that this second statement would need some modification, but I think that the notion of the equi-probability of certain alternative geometrical possibilities being fulfilled would still enter.)

Passing now to Professor Nelson's "roulette-wheel," I would make the following comments:—

(i) He contrasts the case of a wheel which is "honest" and one which is not. But ought we not rather to contrast one that is *known* to the player to be honest, and one which is *not known* to him to be so or not to be so. In the latter case the possibility that it is biased is admitted from the beginning.

If the wheel is *known* to the player to be honest, then no run of a single number, however long, and no sequence of numbers, however

often repeated, would give him any rational ground for betting in favour of a repetition of that number or of that sequence. That is almost, if not quite, an analytical proposition. But, if the bare possibility of bias is admitted from the first, then it might be argued that a sufficiently preponderant proportion of a certain number, or of a certain sequence of numbers, would provide a ground for a rational belief that it is biased in a certain way. That in turn would provide a reasonable ground for betting in a corresponding way on its future behaviour.

Professor Nelson does in fact consider this kind of argument in connexion with his criticism of the "Precept Theory." The essential point seems to me to be one which he himself makes. A glance at the formula for the application of the principles of inverse probability shows that all that an accumulation of uniformly favourable instances can do for a hypothesis is continually to multiply by a new factor its initial probability. Now, if that is to lead to a final probability whose upper limit is 1, we must have reason to believe beforehand, not merely that the initial probability is greater than 0, but that the lower limit of its possible values is greater than 0. Now that is not secured merely by the negative fact that it is not impossible that the wheel may be loaded in one way or another.

(ii) About artificial cases, such as roulette-wheels, the following points may be worth making:—

(a) No one in practice is in a position to *know* (even in the popular sense of that word) that a roulette-wheel is honest. At most he may have extremely good reasons to believe that it has been made by a competent and reliable firm in accordance with the accepted methods for making honest roulette-wheels, that it has not become worn or tampered with, and so on.

(b) Conversely, in certain circumstances one might have very good reasons for thinking it quite probable antecedently that a certain roulette-wheel would *not* be honest. In all artificial cases an essential part of one's ground for holding any reasonable opinion on the antecedent probability of the machine being honest or being biased is knowledge of the general laws of human motivation and of the characters and motives of certain particular individuals. Again, an essential part of one's ground for inferring, from the supposed construction of the machine and its observed performance up to date, to any conclusion about its future behaviour in any assigned respect, is one's knowledge of the general laws of physics and of the properties of specific kinds of matter.

(c) It might therefore seem that there is a risk of *circularity* in taking, as a model for the inductive inference of natural uniformities from observed regularities of co-existence or of sequence, the case of inferring

from the past results of spinning a roulette-wheel to the probable results of further spins. I mention this *appearance* of circularity only in order to say that I do *not* think it harmful for the purpose for which the analogy is used. That purpose is simply to exhibit the presuppositions of an inductive argument in a case where they are very obvious, and to suggest (α) that inductive generalisation *everywhere* presupposes the finite antecedent probability of something *analogous to bias* in the case of a roulette-wheel or a die, and (β) that this *always* rests on some view about the "concealed structure and mechanism" (to use those words very widely) of nature as a whole or of a particular department of it.

(5) *Induction by Simple Enumeration and the Hypothetical Method.* Under this heading I will discuss a number of inter-related points raised by Professor von Wright.

I alleged that induction by simple enumeration (so far as it is exemplified by taking counters out of a bag, noting their colours, and then drawing conclusions with more or less probability as to the original proportion of counters in the bag) is a particular case of the hypothetical method. Professor von Wright objects to this. I think that his objection rests partly on a mere difference in the use of words, and partly on an important matter of principle.

(i) The matter on which I think there is no real difference is this. Let h_0, h_1, \dots, h_n be a set of mutually exclusive and collectively exhaustive alternative propositions, which it is proposed to test by specific experiment or observation. Let f be any relevant data which one may have *before* undertaking the test, and let Q_N be a summary of the relevant information that has accumulated at the N -th stage of carrying out the test. Then for any typical one of these alternatives h_r , the probability relative to the conjunction of f with Q_N is given by the equation

$$h_r/f \& Q_N = [(h_r/f) \times (Q_N/f \& h_r)] \div [\sum_{r=0}^n (h_r/f) \times (Q_N/f \& h_r)]$$

where any symbol of the form " p/q " stands for the probability of the proposition p given the proposition q .

Now in the case of bag-problems the propositions of the form h_r are alternative "hypotheses" to the effect that exactly so many of the n counters in the bag are of such and such a colour. The proposition Q_N is a summary, at any given stage of the experiment, of the accumulated information as to the whiteness or non-whiteness of the counters drawn and inspected up to that point.

In what is commonly called "the hypothetical method" we use what is *in principle* the same formula, but there are the following important differences in detail. (a) Instead of considering a number of mutually ex-

clusive and collectively exhaustive alternative propositions h_0, h_1, \dots, h_n , we consider just a *single* proposition H and its *logical contradictory* \bar{H} . (b) H is such that at every stage $Q_N/f\&H$ is either 0 (in which case the hypothesis is *refuted* and the experiment comes to a natural end), or 1 (in which case there is no reason why the experiment should not be continued). (c) In the bag experiment H is analogous to the single alternative h_n , viz. that *all* the counters in the bag are white. And $Q_N/f\&H$ is either 0 (if Q_N includes the information that at least 1 *non-white* counter has been drawn), or 1 (if it consists of the information that *all* the counters drawn up to that stage have been *white*). The formula therefore reduces to

$$H/f\&Q_N = (H/f) \div [(H/f) + (\bar{H}/f) \times (Q_N/f\&\bar{H})].$$

So what I was trying to say could be more accurately expressed as follows. The reasoning in induction by simple enumeration (so far as this is accurately represented by experiments in drawing counters from a bag), and the reasoning in the hypothetical method, are instances of essentially the *same general formula* in the calculus of probability. And the latter can fairly be regarded as in certain respects a *more restricted* case of that formula, since it is by definition subject to the three conditions stated above.

(ii) The important difference in principle is this. Is the kind of hypothesis which is tested in what is ordinarily called the "hypothetical method" *really* on all fours with the $(n + 1)$ -th. of the alternative "hypotheses" which are tested in an artificial experiment with counters in a bag? Is *All swans are white* a proposition of the same logical kind as *All the n counters in the bag are white*? Professor von Wright objects that the former are propositions about what he calls "open classes" and that the latter are about "closed classes," and that these two are fundamentally dissimilar kinds of proposition.

I think that he is right to object, and that I was wrong to overlook this distinction, but that his objection hardly goes far enough. It seems to me now that we have to contrast at least *three* fundamentally different kinds of proposition, (a) "All S is P " might express simply the proposition that S_1 is P & S_2 is P & S_n is P , and that these are all the S 's that there are. (b) It might express a rather complicated proposition of the following form. Consider a sequence of collections of the following kind, viz., $(S_1), (S_1 \& S_2), \dots, (S_1 \& S_2 \& \dots \& S_n), \dots$. Let the percentage of the members of these collections which are P be respectively $p_1, p_2, \dots, p_n, \dots$. Then "All S is P " might be taken to mean the same as " p_n tends to the limiting value 100% as n tends to infinity." This latter sentence is itself a highly condensed expression for a rather complicated proposi-

tion, but we need not unpack it further here. (c) "All S is P " might be taken to mean that in the actual world (though not in all possible worlds) *any* instance of S *would be* an instance of P . I do not know how to analyse such propositions further. But I can perhaps indicate their peculiarity by remarking that one is tempted to say of any such proposition (α) that, if it is true, it is *necessary*, but (β) that the fact that it is necessary is *contingent*. (In contrast with this, one can say of the necessity of a true *a priori* proposition that its belonging to that proposition is itself a *necessary* fact.)

We might call these respectively the "enumerative," the "limiting-frequency," and the "nomic" interpretations of such a sentence as "All S is P ." It is immediately obvious that (b) differs from (a). If it is not immediately obvious (as I think it should be) that (c) differs from (b), this becomes evident when one reflects that (b) is compatible with there being any finite number of S 's which are *not* P , whilst (c) is not compatible with there being a single S which is not P .

Now the "limiting-frequency" interpretation certainly presupposes "open classes," in the sense of classes which contain an infinite number of members. For that is involved in the notion of a limit. For that very reason I doubt whether it has any application outside pure mathematics. The "nomic interpretation" does not presuppose "open classes" in that sense. For the proposition that *any* instance of S *would be* an instance of P in the actual world is consistent with the number of actual instances in the whole course of the world's history being finite or even zero. What it does presuppose is the notion of classes determined by *intension* as distinct from by *enumeration* of their members.

It seems to me that what we commonly try to test by the so-called "hypothetical method" is universal propositions in the *nomic* sense. If so, they *are* fundamentally different from such propositions as *All the n counters in the bag are white*. But the difference is even more fundamental than would be suggested by the contrast between "open" and "closed" classes.

(6) *The Theory of "Generators."* I have very little to object to in Professor von Wright's comments in what I said about this in *P. P. I*.

(i) He is correct in saying that the argument on P. 27 of that paper does not presuppose that the number of *generated* characteristics is finite. It presupposes only that n , the number of *generating* characteristics, is finite. The further argument, in the section entitled *Effect of the Relative Values of n and N* certainly assumes N to be finite when considering the alternatives that N is less than or equal to n , since n is assumed throughout to be finite. In discussing the alternative that N is greater than n , I certainly did assume in my own mind that N is

finite; and, although the mere supposition that N is greater than n does not entail this, there are many steps in the argument which presuppose it.

(ii) He is correct also in saying that I have nowhere shown that the factor $(\mu_r \& \nu_s)/h$ in the formula on P. 27 is greater than 0. This is the probability (relative to the general assumption of the theory of generators, and to the special assumption that each generated characteristic is generated by *only one* set of generators) of the proposition expressed by the sentence: "In a generalisation, whose subject is a conjunction of μ generated characters, and whose predicate is a conjunction of ν generated characters, the former require exactly r , and the latter exactly s , generating factors respectively to generate them."

(iii) He says, rightly, that all my arguments presuppose that the antecedent probability of a generalisation "can be linked with a ratio of true generalisations among a class of generalisations." But he complains that the nature and justification of this link are not made clear. I do not see exactly what the difficulty is here. If it could be shown that at least a certain proportion of possible generalisations of a certain kind *must* be true, e.g., at least $p\%$ of generalisations with a μ -fold subject and a ν -fold predicate, surely the antecedent probability of any generalisation of that kind would be at least

$$\frac{p}{100}.$$

(iv) He mentions my remarks on P. 41 of *P. of P. I.*, that the generating factors must be supposed to be *determinable* characters, and that it would follow that the generated characters must be so too. He finds the notion of "determinables" and "determinates" obscure, and asks me to try to clarify it.

I regret that it is impossible for me to go into this very large question here. The following very sketchy and therefore rather obscure remarks must suffice. (a) My account of generating factors explicitly assumes that no conjunction of such factors is either logically necessary or logically impossible. The statement about generating factors having to be determinable characters is bound up with this. (b) That is because of the following properties of determinable characters and of determinate characters. Supreme determinables are all logically independent of each other. But it is logically necessary that any thing which possesses a determinate character should possess all the determinables, of whatever order, under which this falls. And it is logically impossible that any thing should possess two determinate characters of the same order which fall under one and the same determinable.

I *think* that a more accurate statement of what I had in mind would

run as follows. A complete collection of generating factors would have either (a) to contain nothing but *supreme determinables*; or (b) to contain nothing but *determinates*, each of which falls under a *different* supreme determinable; or (c) to be a *mixture* of (α) supreme determinables, and (β) determinates, none of which fall under any of these determinables, and each of which falls under a different supreme determinable.

(7) *Necessary Conditions and Sufficient Conditions*. I take some credit for seeing by 1930, when I published my two papers on "Demonstrative Induction," that these are the essential concepts involved in demonstrative induction, and for having worked out the formal logic of them in some detail and without serious mistakes, though not without one very serious omission. But all that I have written on this topic has now been superseded by Professor von Wright's more thorough and more accurate work.

I agree with him that it sounds odd to say: "The ground becoming wet is a necessary condition of rain having fallen in the neighbourhood," and I agree that both he and I are committed by our definitions to saying such things. I agree too that the verbal paradox is bound up with the conviction that a *causal* condition must be fulfilled *before* that which it conditions begins. That is why, in the present essay, I have introduced the terms "necessary *precursor*" and "sufficient *precursor*," when discussing, in Section IV, C, 2 above, Professor Russell's comments on my account of Causation in the *EMcP*.

I should be inclined to say that we must distinguish between a "condition," in the sense of a *ground for inference*, and a "condition" in the sense of a *factor in causation*. Given a knowledge of causal laws, one can often infer from knowledge of a *later* event to the conclusion that such and such an *earlier* event must have happened. (Unless one is a prophet, one cannot of course infer from knowledge of a *future* event to the occurrence of such and such an event in the *present or the past*, since one cannot be in possession of such knowledge.) But, when a person makes such an inference from a later to an earlier event, he does so because he has reason to believe that the later state of affairs (e.g., the ground being wet) would have come into being *only* if it had been preceded by a state of affairs containing such and such an event (e.g., a fall of rain in the neighbourhood) as a cause-factor.

(VI) *Time in general and Precognition in particular*

Under this heading I shall be concerned with a part of Professor Ducasse's essay and with the whole of those by Professors Flew and Mundle.

(A) **TIME IN GENERAL.** Professor Mundle's researches show that I have "boxed the compass" about time, and in the course of doing so, have written some things which now make me blush. It may be interesting and possibly illuminating to mention very briefly the main influences under which the three accounts of time considered by Professor Mundle were written. At the back of all of them is McTaggart's paper "The Unreality of Time," published in *Mind* in 1908. I felt from the first, and I still feel, that the difficulty which he raises is (a) embarrassing enough *prima facie* to demand the serious attention of anyone who philosophises about time; but (b) almost certainly due to some purely linguistic source (common, and perhaps peculiar, to the Indo-European verb-system), which it ought to be possible to indicate and make harmless.

At the period when I wrote the *Encyclopaedia* article (which, I must confess, I had wholly forgotten until Professor Mundle's essay reminded me of it) I was almost completely under the influence of Bertrand Russell in his extreme realist phase, and of Meinong as I understood him. By the time I wrote *Scientific Thought* I was greatly influenced by books recently published by Alexander and by Whitehead. The talk in *Scientific Thought* about "the sum-total of existence continually increasing by Becoming," and in the *Examination of McTaggart's Philosophy* about "Absolute Becoming," goes back to this source. To the influence of Whitehead was due the shocking remarks in *Scientific Thought* about a thing or a person *being* "a long event."

By the time I wrote the *Examination* I had got free from the worst of that kind of crudity, largely through the careful work which had been done in the meanwhile by Moore and others on the notion of "logical constructions." What I was putting in a terribly slovenly way in *Scientific Thought* on this topic could be stated with more polish as follows. A sentence, whose grammatical subject is the name or a description of a *thing* or a *person*, and whose grammatical predicate is appropriate to such a substantive word or phrase, can be replaced without loss, gain, or distortion of meaning, by a complex of sentences, in each of which there occur *only* names or descriptions of *processes*, with grammatical predicates appropriate to process-words. (This is certainly not "snappy," and it may not be true; but at any rate it is not "sick-making," like: "A thing or a person is a long event.")

In writing each later account of time I started afresh, and was not concerned with its consistency or inconsistency with earlier accounts. What I have said on this topic in the *EMcP* was meant to supersede what I had said on former occasions, wherever there was a conflict

between the two. The reader may assume that I continue to hold (though with much hesitation in view of the difficulty of the subject) any opinion which I expressed in the *EMcP*, unless I explicitly question or withdraw it in what follows below. I hope that this statement will justify me in confining my attention here, as I intend to do, to the *third* account and to Professor Mundle's comments on it.

(1) *Qualitative Change and "Absolute Becoming."* It seems to me that there is an irreducibly characteristic feature of time, which I have called "Absolute Becoming." It must be sharply distinguished from qualitative change, though there is no doubt a connexion between the two. In the experience of a conscious being Absolute Becoming manifests itself as the continual *supersession* of what was the latest phase by a new phase, which will in turn be superseded by another new one. This seems to me to be the rock-bottom peculiarity of time, distinguishing *temporal sequence* from all other instances of one-dimensional order, such as that of points on a line, numbers in order of magnitude, and so on.

It is plain that Absolute Becoming is different from qualitative change. An example of the latter would be the gradual melting of a lump of ice in the sunshine, the sudden alteration in the pitch of the sound heard when a whistling locomotive rushes by one, and so on. The contrary opposite to qualitative change is *qualitative invariance*. An example would be the sound heard when a whistling noise of constant pitch, loudness, and tone-quality is made by a locomotive in presence of a hearer who is at rest relatively to it. Now Absolute Becoming is indifferent to whether there be qualitative variation or qualitative invariance. A superseding phase may be qualitatively indistinguishable from that which it supersedes and from that which supersedes it. Again, in the case of a qualitative variation it is sensible to ask: At what speed is it taking place? We know that the speed of some such changes is greater than that of others, and the speed of any particular qualitative change is a matter for empirical investigation. But there is no sense in asking: At what speed does a certain phase, which was present, retreat into the past? And there is no sense in the suggestion that some might do this faster than others.

Nevertheless, there is undoubtedly a very strong temptation to talk of Absolute Becoming in terms of qualitative change, and particularly in terms of some kind of *motion*. I am quite sure that all such ways of talking are misleading for the reasons given above. Moreover, if offered as an analysis of Absolute Becoming, they involve a kind of vicious circle. For the notions both of qualitative change and of qualitative invariance

plainly *presuppose* that of Absolute Becoming, in the sense of that phrase which I have indicated. This circularity is the fundamental objection to *all* such metaphors. Particular forms of the metaphor have, in addition, particular defects characteristic of each. The "policeman's bulls-eye" metaphor, e.g., if taken seriously, presupposes that what has not yet supervened and what has already been superseded in some sense "coexist" with each other and with what is now occurring. Again the metaphor of the history of the world "growing continually longer in duration by the addition of new slices," which I took seriously in *Scientific Thought*, presupposes that phases, which have already supervened and been superseded, in some sense "co-exist" with each other and with that which is now happening.

Let us, then, avoid metaphors and similies and concentrate on the following very simple example, viz., a prolonged sound, continuing for a minute without any variation in pitch, loudness, or tone-quality. Here there is the minimum temptation to imagine that the phases which have been superseded, e.g., the first 30 seconds of this sound, "*continue* somehow to exist," or that the phases which have not yet supervened, e.g., the last 30 seconds of the sound, "*already* somehow exist." And, since we have explicitly excluded all variation in quality, there is no temptation to confuse Absolute Becoming, i.e., the supersession of earlier phases by later ones, with qualitative variation.

(2) *The notion of "Successive Phases."* We seem now to be faced with a serious difficulty. I have spoken of one "phase" of a process "superseding" another, and of its being in turn "superseded by" another. But what is a single phase? Is it supposed to have duration, or is it supposed to be quite literally momentary?

(i) Suppose we ascribe *any* duration, however short, to a phase which has supervened and has not yet been superseded. Then it seems plain that it must consist of an earlier *sub-phase* adjoined to a later one, and that *either* the earlier one has already been superseded by the later one, *or* the later one has not yet supervened on the earlier one. On either alternative *only one* of the two actually exists now. Obviously the same argument applies to each sub-phase itself, and so on without end.

If that is denied, it would seem that the denier is committed to some such view as the following. He must suppose that the sequence of successive moments is *discrete* (like the sequence of integers); that there is an *intrinsically indivisible unit of duration* (viz., the interval between one moment and the next); and that each phase supervenes at one moment and is superseded at the *next*, and therefore has the intrinsically indivisible unit duration. Now I find this quite unintelligible. I can

write the words "phase of finite, but intrinsically indivisible, duration," but I can attach no clear idea to what I have written. So I cannot regard this as a genuine alternative.

(ii) Suppose, then, that we say that each phase is *literally momentary* and has literally *no* duration. Then, assuming the continuity of time and therefore that the phrase "next moment" is meaningless, we shall have to say that *at one and the same moment* a phase supervenes and is superseded. To many this may sound palpably absurd, but I am not sure that it is so.

Let us, for once and for this special purpose, do what I have been warning the reader against, and compare Absolute Becoming with *motion*. Everyone must admit that a moving particle *leaves* each point which it traverses at, *literally the same moment* at which it *enters* it. "Entering" refers essentially *back* to positions occupied *before*, and "leaving" refers essentially *forward* to positions occupied *after*, the moment and the point in question. Might not similar remarks apply *mutatis mutandis* to "supervening" and "being superseded?" These refer respectively backwards to phases which have been, and forward to phases which will be; but any momentary phase just *momentarily is*.

(iii) Even if this answer to the alleged difficulty in question be accepted, I think that one tends to feel dissatisfied with the notion of literally momentary phases on another count. Surely the notion of a literally momentary phase (like that of a geometrical point or line or surface) is the notion *either* of a boundary between successive adjoined phases, each of *finite* duration, *or* of a limit to an endless sequence of shorter and shorter durations, one inside another, like an endless nest of Chinese boxes? If so, it *presupposes* the existence of phases of finite duration. And surely (it might be added) the latter alone could be actual existents. The literally momentary, like the literally punctiform, bears all the marks of an abstraction, incapable of actual concrete existence, as opposed to an existent particular.

As a preliminary comment on this last objection I will ask the reader to consider for a moment the following geometrical analogue, *viz.*, points without any spatial magnitude, lines with length but no area or volume, and surfaces with area but no thickness. We, whose spatial experiences are of the 3-dimensional kind, consider all these to be abstractions, of the nature of boundaries or limits. We regard objects extended in three dimensions as the only possible kind of extended particular existents. But a creature whose spatial experiences were of the 4-dimensional kind would presumably think of what we call a "solid" in the sort of way in which we think of a 2-dimensional surface. He would

think of it as a *boundary* or *limit* with reference to objects extended in four dimensions, and he would regard the *latter* as the only possible kind of extended particular existents. Conversely, a creature whose spatial experiences were of the 2-dimensional kind would presumably think of what we call a "surface," *not* as a mere boundary or limit with reference to objects extended in three dimensions, but as the only possible kind of extended particular existent.

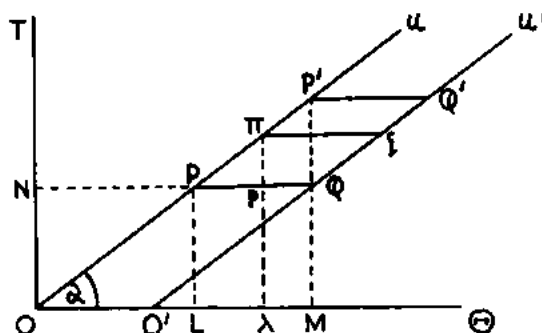
These reflexions seem to show that the question whether a person will regard a *spatial* entity of a given number of dimensions as a particular existent or as a mere boundary or limit, depends on the number of dimensions characteristic of his spatial experience. If the entity is of *that* number of dimensions (e.g., 3 in the case of human beings), he will regard it as a particular existent (e.g., as a cubical block, a spherical globe, and so on). If it is of *less* than that number of dimensions, he will regard it as a mere boundary or limit (e.g., as a face of a cube, the surface of a sphere, and so on). If it is of *more* than that number of dimensions (e.g., 4 or more in the case of a human being), he cannot perceive it as such. He can perceive only what a 4-dimensional being would regard as various 3-dimensional *boundaries* of it, and he will take these to be *particular existents*. This at least enables one to see that the question whether a given *spatially extended* entity is a particular existent or a mere boundary or limit, is not so simple and unambiguous as it might seem at first sight.

But I doubt if this really helps us in the present case. The question is whether we could regard *literally momentary* phases as actual existents, or whether we must regard them as limits or boundaries of phases of finite duration. Now our temporal experience is at least 1-dimensional, whilst a momentary phase would be an entity of *zero* temporal dimension. So, on the principles laid down in the preceding paragraph for *spatially extended* entities, it would seem that we could not help regarding a literally momentary phase as a mere boundary or limit, and not as a particular existent.

(3) *The theory of 2-dimensional Time.* The only solution that I can think of is to allege that Time is of at least *two* dimensions, and that a phase which has zero duration in the dimension which we commonly recognise has a finite "duration" in the other dimension.

A theory on these lines has been put forward and argued in detail by my friend and former pupil, Mr. H. A. C. Dobbs, in the *British Journal for the Philosophy of Science* for August 1951. His object was primarily to deal with (a) the facts which are summarised under the phrase "the specious present," and (b) certain notions of quantum physics. I shall here state in my own way a simplified form of the theory, as I understand

it, without reference to the specious present or to the quantum theory. The reader should direct his attention to the diagram below:—



I am going to call the two temporal dimensions the “ Θ -dimension” and the “ T -dimension.” A *completely* instantaneous “phase-particle,” as I will call it, would be represented by a *point* in the diagram, whose co-ordinates are $\Theta = \theta$, and $T = t$. It might be denoted by the symbol $p(\theta, t)$. What we have been calling a “momentary phase” occurring at the instant t is represented in the diagram, *not* by a point, but by a *straight line* of finite length and no thickness parallel to the Θ -axis. It may therefore be described as “ T -instantaneous,” but it has a certain extension, which we will call “ Θ -duration” in the Θ -dimension. Suppose that such a phase occurs at $T = t$ in the T -dimension, and that it extends from $\Theta = \theta_p$ to $\Theta = \theta_q$ in the Θ -dimension. Then we can denote it by $\phi(t, \theta_p \rightarrow \theta_q)$. In the diagram the line PQ represents such a phase. ON represents $T = t$, OL represents $\Theta = \theta_p$, and OM represents $\Theta = \theta_q$.

So much by way of notation and diagrammatic representation. We can now formulate the details of the theory as follows:—

(i) We assume that there is a certain fixed direction in the 2-dimensional time-field, represented in the diagram by a fixed straight line OU , making an angle α with the axis $O\Theta$. (It does not matter for our present purpose what the magnitude of α may be, provided it is between 0 and $\frac{\pi}{2}$.)

(ii) Every T -instantaneous phase stretches in the Θ -dimension from a phase-particle represented by a point, such as P , on the line OU , to a phase-particle represented by a point, such as Q , on a line $O'U'$ parallel to OU and at a fixed distance from it along the Θ -axis. (For the present purpose it does not matter what may be the magnitude of the Θ -duration represented by the distance OO' .)

(iii) For each *successive* T -instantaneous phase, as the value of T

continuously *increases*, the initial phase-particle is *further* along the line OU.

(iv) Between any two *T*-instantaneous phases, no matter how near together be the respective values of *T*, there is a *third T*-instantaneous phase.

It will be noted that we have secured by these suppositions a consistent combination of (a) *continuity* of transition, (b) the *finite* Θ -duration of each *T*-successive term, and (c) the *instantaneity* of each *T*-successive term. This is secured by the fact that *T*-instantaneous phases, though *completely* successive in the *T*-dimension, *partially overlap* in the Θ -dimension provided that the difference in their *T*-dates does not exceed a certain maximum, and that the nearer their *T*-dates are to each other the more nearly complete is this overlap.

A glance at the diagram will show that there must be a kind of "natural unit" of *T*-time-lapse, correlated with the "natural unit" of Θ -duration represented in the diagram by the distance OO'. (This might be compared with the natural unit of 4 right-angles in the case of angles.) In the diagram let the straight line MQ be produced upwards until it cuts the fixed line OU at P'. Then the phase P'Q' is the *first* successor to the phase PQ which does *not* overlap PQ *at all*. Thus the line QP' represents a kind of natural unit of *T*-time-lapse. This is obviously connected with the natural unit of Θ -duration and the fixed angle α by the relation $\frac{P'Q}{PQ} = \frac{P'Q}{OO'} = \tan \alpha$. Let us denote the natural unit of *T*-time-lapse by τ , and the natural unit of Θ -duration by σ . Then $\tau = \sigma \tan \alpha$.

It is plain that τ , the natural unit of *T*-time-lapse, can belong only to a *T*-sequence of *phase-particles*, all of which have *the same* value of Θ . Such a sequence *begins* with a phase-particle (such as Q) which is at the *terminal end* of a complete *T*-instantaneous phase (such as PQ), and it *ends* with a phase-particle (such as P') which is at the *initial end* of a certain later complete *T*-instantaneous phase (such as P'Q').

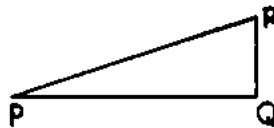
It will be of interest to consider next the *T*-time-lapse belonging to a sequence of *sub-phases*, all of which have the same initial value and the same terminal value of Θ . For this purpose we can consider the sub-phase represented by the segment pQ of the line PQ (which represents a complete phase). Through p draw a line parallel to OT. Let it cut the fixed line OU at II, and the axis O Θ at λ . Then it is evident that there will be a sub-phase extending from $\Theta = O\lambda$ to $\Theta = OM$ in every successive complete phase from PQ to IIq, both inclusive, and in no others. So the *T*-time-lapse belonging to this sequence of sub-phases is repre-

sented by Πp . Now $\Pi p = Pp \tan \alpha$. But $Pp = (PQ - pQ) = (\sigma - pQ)$. And we have already shown that $\tan \alpha = \frac{\tau}{\sigma}$. So $\Pi p = (\sigma - pQ) \frac{\tau}{\sigma} = \tau(1 - \frac{pQ}{\sigma})$. If we put $\Pi p = t$, and $pQ = s$, we can write this in the form $t = \tau(1 - \frac{s}{\sigma})$. So the T -time-lapse belonging to such a sequence of sub-phases varies between the limits 0 (when $s = \sigma$, and the "sub-phase" is supposed to swell into a *complete* phase of natural unit Θ -duration) and τ (when $s = 0$, and the "sub-phase" is supposed to shrink into a mere *phase-particle*.)

(4) *The Specious Present*. This brings me to the question of the Specious Present. What I have to say about this is in principle the same as what I said in Vol. II, Part I, of the *Examination* (Pp. 281-288). But on the one hand it becomes considerably clearer when stated in terms of 2-dimensional Time, and on the other hand it provides a concrete illustration of the abstract account of the latter given above.

It is evident that the 2-dimensional diagram at the top of P. 285 in my account of the specious present in the *EMcP* would have to be replaced by a 3-dimensional diagram. For we have now to represent *two* temporal dimensions (instead of one only, as in the *EMcP*), and in addition (as there) the magnitude which I called "degree of presentedness." The modifications needed will be understood without difficulty, if the reader will refer back to the diagram given above in expounding the general theory of 2-dimensional Time.

Suppose now that the lines PQ , Πq , $P'Q'$, etc., in that diagram represent the Θ -durations of T -successive *specious presents*. Then we should have to represent *degree of presentedness* by distances along a *third* axis, sticking out at right-angles from the plane of the paper. We must regard each of these lines as the base of a right-angled triangle, e.g.,



turned about PQ so that QR is normal to the plane of the paper. The length QR then represents the *maximal* degree of presentedness, viz., that of the *latest* end of the content of a specious present. The degree of presentedness tails off to zero at the *earliest* end of that content. For the details of this I refer the reader to the account in the *Examination*.

It may be of interest, however, to add a diagram representing in terms of this theory the hearing of the sound of a short word, e.g.,

(B) **PRECOGNITION.** This topic forms the theme of a part of Professor Ducasse's paper and of the whole of Professor Flew's. I will begin with Professor Ducasse's "Theory Theta." Although this is put forward primarily to deal with the problem of non-inferential precognition, it is a *general* theory of time, and therefore highly relevant to the topics which we have been discussing above.

(1) *Professor Ducasse's "Theory Theta."* The theory falls into three divisions, viz., (i) Inter-relations of *physical events*, (ii) Inter-relations of *experiences*, and (iii) Relations *between* experiences and physical events. I will take these points in turn.

(i) *Inter-relations of Physical Events.* (a) Purely physical events, which do not overlap each other, form a 1-dimensional *quasi-temporal* series ordered by an *irreducibly triadic relation*, which I will call "chronical betweenness." By saying that this relation is *irreducibly triadic* we mean that the statement that the physical event *Y* is chronically between the physical events *X* and *Z* is *not* analysable into the statement that *either X* is earlier than *Y* and *Y* earlier than *Z*, or *X* is later than *Y* and *Y* later than *Z*. In the sequence of purely physical events there is no asymmetrical dyadic relation, such as earlier-and-later, which would give an intrinsic direction to it.

(b) If we take any *two* terms *U* and *U'* in such a series, we can subdivide all the remaining terms in it into two mutually exclusive and collectively exhaustive sub-classes, as follows, viz., (α) those which are on *the same side* of *U'* as *U* is, and (β) those which are on *the opposite side* of *U'* to *U* (i.e., those of which it is true that *U'* lies between them and *U*.)

(c) Let us now consider a term *X*, which is on the *same side* of *U'* as *U* is. Then there are two mutually exclusive and collectively exhaustive possibilities, viz., (α) that *X* is between *U* and *U'* (i.e., that *X* is "chronically nearer" to *U'* than *U* is), or (β) that *U* is between *X* and *U'* (i.e., that *X* is "chronically further" from *U'* than *U* is.) These are the two possibilities which Professor Ducasse would formulate respectively as: "*X* is *past* to *U* from *U'*" and "*X* is *future* to *U* from *U'*." So these two statements may be defined as follows:—

(α) "*X* is *past* to *U* from *U'*" means the same as "*X* is chronically on the *same side* of *U'* as *U*, and is chronically nearer to *U'* than is *U*." I will denote this by $\Pi (X, U; U')$.

(β) "*X* is *future* to *U* from *U'*" means the same as "*X* is chronically on the *same side* of *U'* as *U*, and is chronically further from *U'* than is *U*." I will denote this by $\Phi (X, U; U')$.

I think it is wiser to keep to the symbols and their definitions, and *not* to use the phrases "past to . . . from" and "future to . . . from,"

when talking of the *quasi-temporal* inter-relations of purely physical events. For these phrases inevitably have associations which may mislead us.

It should be noted that, although the two relationships $\Pi (X,U;U')$ and $\Phi (X,U;U')$ are *mutually exclusive*, they are *not* collectively exhaustive. Both of them presuppose that X is on the *same* side of U' as U is. Obviously there remains the possibility that X should be on the *opposite* side of U' to U . In that case obviously X and U' would be on the *same* side of U , and X would be *further* from U than is U' . So we should have $\Phi (X,U';U)$. So it would seem that for any three non-overlapping purely physical events the three mutually exclusive and collectively exhaustive possibilities would be $\Pi (X,U;U')$, $\Phi (X,U;U')$, and $\Phi (X,U';U)$.

It is also worth noting that $\Pi (X,U;U')$ is equivalent to $\Phi (U,X;U')$. For to say that X is *nearer* to U' than is U , is obviously equivalent to saying that U is *further* from U' than is X .

I hope that the above is a complete and correct formal statement of Professor Ducasse's account of the *quasi-temporal* order of purely physical events. It may be remarked that it is precisely analogous to the *intrinsic spatial* order of points on a straight line. There is no *intrinsic* "sense" in the order of points on a line. When we ascribe one to it, we do so either by reference to our right and left hands, or by imagining something traversing it and so occupying certain points earlier and others later.

(ii) *Inter-relations of Experiences*. I am not at all sure that I fully understand Professor Ducasse's account of the temporal order of *experiences*. I think it is plainly concerned primarily with the experiences which together make up the mental history of some *one* conscious individual. Again, I think it is concerned *both* (a) with what almost everyone would call "experiences," e.g., feeling a twinge of toothache, or the popping up into consciousness of a name which one had been trying to recall, *and* (b) with what some (but not all) philosophers would refuse to call "experiences," but would prefer to describe as the "immediate objects" of certain experiences, and would call "sense-data," "mental images," and so on. I shall cover both cases here by saying that " x is *present to* P ," if and only if x is either (a) an experience, or (b) a sense-datum or a mental image or (in general) a "prehensum"; and P is either (a) having that experience, or (b) sensing that sensum or imaging that image or (in general) "prehending" that prehensum.

On these assumptions, I think that this part of the theory certainly includes the following propositions:—

(a) What is "present to" a person at any given moment consists of a certain set of *sub-phases*. All of these are then present to him with *some*

degree or other of what Professor Ducasse calls "liveness," and each different one of them is present to him with a *different* degree of liveness. One and only one of them (which may itself be internally complex) is then present to him with the *maximal* degree of liveness. I propose to call them "degree-of-liveness sub-phases."

(b) It follows that what is present to a person at any given moment constitutes a *finite segment*, in respect of the different degrees of liveness, from maximal to minimal, with which each different sub-phase is then present to him. Let us call this a "degree-of-liveness segment."

(c) What is present to a person at any moment is not merely *in fact* a degree-of-liveness segment. It is *presented to him as* such a segment, and he can formulate these facts about it if he inspects it and reflects on his findings.

(d) That sub-phase of what is present to a person at any moment which is then present to him with *maximal* degree of liveness, is at that moment *strictly present*. All the other sub-phases in the segment are then *strictly past*. The *degree* of pastness of each is correlated *conversely* with its degree of liveness.

(e) Every degree-of-liveness sub-phase of the segment which is present to a person at any moment may be called "*speciously present*." This serves to contrast them all with sub-phases which have been present to the person, but are no longer so, and with others which have never been present to him but may be so later.

Now it will be noted that all the propositions which I have ascribed above with some confidence to Professor Ducasse have involved the phrase "at any one moment." I find it impossible to state the theory (or indeed *any* account of "specious presentness") without introducing that phrase, or some equivalent of it. But it is plain that the theory would be hopelessly inadequate unless it also referred to a *plurality of successive specious presents*, and it is at this point that I feel very uncertain as to Professor Ducasse's meaning.

It seems to me that at least the following statements would need to be added, but I am not sure which (if any) of them Professor Ducasse would accept:—

(a) If a sub-phase is present to a person with the *maximal* degree of liveness at the moment t_1 , then (α) at *no* moment before t_1 was it present to him at all; (β) at each successive moment *after* t_1 (up to and including a certain moment t_2) it will be present to him with a lesser and lesser degree of liveness, and at t_2 with *minimal* degree; and (γ) after t_2 it will never be present to him again.

(b) If a sub-phase is present to a person with a degree of liveness *less than the maximal* at the moment t , then there is a moment t_1 (*earlier*

than t) such that it was present to him with *maximal* degree of liveness at t_1 . (My account of specious presentness in the *Examination*, and the amended account given above in terms of 2-dimensional Time, was intended to represent, by means of the series of triangles with partly overlapping bases, this irreducible feature of continuous transition.)

Now one reason why I am doubtful whether I have fully understood Professor Ducasse's theory is this. On the one hand, he appears to make such statements as the following. To call any phase "strictly present" means simply and solely that it is present to one with the maximal degree of liveness. To call any phase "strictly past" means simply and solely that it is either (a) present to one with less than the maximal degree of liveness, or (b) not present to one, but capable of being attended to only by "recalling" it in memory. Again, he says explicitly that statements about the specious present should *not* be made in *temporal* terms, as if we knew *independently* what such terms mean. And the reason given is that liveness and its degrees are the experiential basis, not only of our notion of *presentness*, but also of our notion of *earlier* and *later*, and therefore of our notion of Time. On the other hand, he does speak of events "popping into" the specious present, in the direction from maximal to minimal degree of liveness.

Now I agree that the ordered degrees of liveness with which a number of different sub-phases are present to a person at any given moment of his life may well be *one* essential factor in the experiential basis of our notions of past and present, of earlier and later, and so of Time. But surely a no less essential factor is the experience (a) of what *was just lately* present to one with maximal liveness being *now* present to one with lesser liveness, (b) of something which *just lately* was not present to one at all being *now* present to one with *maximal* liveness, and (c) of what *was just lately* present to one with minimal liveness being *now no longer* present to one at all. So far as I can see, none of these features in our experience can be described except in such temporal terms as "at a given moment," "now," "just lately," "no longer," etc. If so, it would seem impossible to admit that our notion of Time can be defined or described, completely and without circularity, simply in terms of maximal and lesser degrees of presentness to an experient. Perhaps Professor Ducasse has no intention of denying this. But my impression is that his explicit statements are (and are probably intended to be) equivalent to denying it. If so, I cannot see how the theory can be adequate to the facts of our experience.

(iii) *Relations of Experiences to Physical Events*. I doubt if I fully understand Professor Ducasse's account of the relation between the triadic *quasi-temporal* order of purely physical events and the dyadic

genuinely temporal order of experiences. It is this which plays an essential part in his theory of Precognition. The following points seem to be certain:—

(a) To say of a *physical* event that it is "present" at a certain moment means simply and solely that it is then the object of a *perception* which is *strictly present* to some percipient. (b) It is logically possible that one and the same physical event should be *perceived* on several different occasions (either by the same person or by different persons), and therefore, by definition, that it should be *present* on as many different occasions. (c) In a genuine case of precognising non-inferentially a physical event *e*, what would happen would be this. (*α*) The person who is said to "precognise" *e* in fact *perceives* it, but in an *abnormal* way, viz., *not* by means of sensations (which Professor Ducasse describes as "vivid images caused at the time through the functioning of the sense-organs"). (*β*) *Later* he, or someone else, perceives *e* in the *normal* way, i.e., by means of sensations.

It seems to me that these three statements would at any rate need the following qualifications. (a) I think that the first of them would need to be amplified somewhat as follows. A physical event *e* is present at any moment, if *either* (*α*) it is then the object of a perception which is strictly present to a percipient, *or* (*β*) it is *contemporary with* another physical event *e'*, of which this is true. Unfortunately Professor Ducasse has given no account of "simultaneity" between purely physical events.

(b) As Professor Ducasse recognises, it would be necessary (in view of the finite velocity of light and sound and of the transmission of nervous impulses) to modify the original statement somewhat as follows. A physical event, perceived normally by the senses, which would be called "present" in accordance with the proposed definition, would always in fact be *earlier* (and in some cases very much earlier) than the strictly present sense-perception of it. It would therefore be really *past* at the moment when, if the definition were taken as it stands, it would be called "present."

(c) Professor Ducasse admits the possibility that a physical event which is non-sensuously "perceived" by one person may *later* be perceived normally by *another* person. This presupposes some temporal correlation between the mental histories of *different* persons. Obviously there is such a correlation. But all Professor Ducasse's statements about the temporal interrelations of experiences have been confined to those which fall within the mental history of a single person.

The above are comments on matters of detail. The two following are more general. (a) What are we to understand by the kind of *non-sensuous* perception of a physical event, which Professor Ducasse postulates

in contrast with ordinary sense-perception? He seems to assume that there is some generally admitted definition or description of the genus or determinable "perception," which leaves the two possibilities "sensuous" and "non-sensuous" open as specifications of it. But, if so, what is it?

(b) At an earlier stage of his essay, in dealing with the alleged "fatalistic objection" to the possibility of non-inferential precognition, Professor Ducasse asserts (quite rightly) that many of the instances of "veridical precognition" are not instances of *cognition* (i.e. knowledge) of the future event which will in course of time verify or refute the "precognition." I do not see how this fits in with the later suggestion that in cases of veridical precognition one and the same physical event is twice perceived, first non-sensuously and later sensuously, by the same person or by different persons.

(2) *Professor Flew's Comments.* I pass now to Professor Flew's essay. This is concerned with my treatment of three *prima facie* objections which I alleged that many people feel in connexion with the very notion of veridical non-inferential foreseeing. I called these the "Epistemological," the "Causal," and the "Fatalistic" objections.

(i) *The Epistemological Objection.* As Professor Flew agrees in the main with my statement of this and with my answer to it, I will comment only on the following point. I stressed an alleged analogy between ostensible *foreseeing* and ostensible *remembering* of incidents, persons, and things. In doing so, I said that a present *image* is involved in ostensible remembering, and I asserted or implied that one would be involved in ostensible foreseeing also. Professor Flew points out that in *The Mind and its Place in Nature* I had denied that a present image is necessarily involved in ostensible remembering, and he speculates on the cause of my "backsliding."

In point of fact I have never seen occasion to alter the opinion on this point which I expressed in *MPN*. The explanation of my apparent "backsliding" in "The Philosophical Implications of Precognition" is this. I was thinking exclusively of the *sporadic* cases on record at the time in the books and papers which I mentioned. The experimental work in connexion with card-guessing, which has since become the most important evidence for "precognition," was not then available. Now images (in a wide sense which includes the *quasi-sensa* of dreams and waking hallucinations) *are* involved in most of the sporadic ostensibly precognitive experiences, and images *are* involved in many experiences of ostensibly remembering events, persons, or things. In the paper in question I was concerned to stress the resemblance to memory, and to undermine the common assumption that veridical non-inferential precognition, if it occurs, *must* be of the nature of *perception*.

It is plain that the card-guessing results bear very little resemblance to experiences of ostensible remembering, and do not fit at all into the framework of my Aristotelian paper. This is of considerable importance in reference to Professor Flew's essay, for it is evident that the empirical data which he has in mind are correlations between a sequence of guess-values and a sequence of target-values, as in a card-guessing experiment.

Here the only relevant property of any guess is that it is a guess that the target-card bears such and such a one of a small number of known alternative symbols, e.g., a cross, where the alternatives are known to the guesser to be, e.g., a cross, a square, a circle, a wavy line, or a triangle. No-one would say here of *any particular* correct guess that it is "at least a very remarkable coincidence." One would say this only of the *proportion* of correct guesses in a long sequence of guesses. And one would say it only if that proportion were to differ (either by excess or defect) from "the proportion most probable on the hypothesis of chance-coincidence" by several times the "standard deviation" for such a sequence on that hypothesis. At a certain point, which would differ from person to person, one would be inclined to say: "This excess (or defect) is altogether too great to be reasonably regarded as a mere freak of chance."

Now the kind of case which I had in mind was different. Here a person is presented on a certain one occasion with an image or a *quasi-sensum* of a very detailed and elaborate kind, and no obvious cause can be suggested for the occurrence of that experience in him at that time. Not too long afterwards there happens an event in the external world, which could not normally have been expected by him at the time, and it corresponds in a remarkable way in its details with the experience in question. One is inclined to say of *any particular* pair of events of this kind that it constitutes "at least a very remarkable coincidence." And if the singularity and unexpectedness of the later event, and the degree of detailed correspondence between it and the earlier experience, surpass a certain point (which again would differ from person to person), one would be inclined to say: "This correspondence is altogether too peculiar and too detailed to be reasonably regarded as a mere freak of chance."

Since Professor Flew confines himself to evidence of the *first* kind, I shall do so too. I will only remark that I think it would be very difficult, in the case of some of his arguments, to adapt them to evidence of the *second* kind, which is what I had in mind in writing the paper on which he is commenting.

(ii) *The Causal Objection.* I think that Professor Flew's formulation of the alleged objection is essentially correct, but I will re-state it in my own way. A certain person *P* makes a sequence of guesses as to which one of a small number of known alternative symbols will be on the face of

the *next* card which is about to be turned up in a certain experiment. Here a "hit" is a case where the symbol guessed is the *same* as that on the face of the card next turned up after the guess has been made. What is said to be "too great to be a mere chance coincidence" is the deviation (positive or negative) between the actual proportion of hits in the whole sequence and what is called "the most probable proportion of hits, on the hypothesis of chance coincidence, in such a sequence."

Now (a) by *definition* we are *not* to count the results as evidence for *foreseeing*, if the difference between the actual and the most probable proportion of hits *can* be dismissed as "merely a remarkable coincidence." But (b) to deny that it is a mere coincidence is to allege that there is *some causal connexion* between (α) some at least of the events described as "a guess that the next card will have such and such a symbol on its face" and (β) some of the events described as "the turning up, immediately after the making of that guess, of a card with such and such a symbol (the same or different) on its face." Let us call these respectively a "G-event" and its "A-correlate." Now (c) it is contrary to the notion of causation that the A-correlate to a G-event should be a factor in *causing* the latter. For the A-correlate does not *begin* until after the G-event in question has *ceased*. Therefore (d) the only kinds of causal connexion that are possible are the following. Either (α) a G-event is a cause factor in a *causal ancestor* of its A-correlate; or (β) a G-event and its A-correlate are effect factors respectively in an earlier and in a later *causal descendant* of some *common causal ancestor*; or (γ) the G-event was determined by the result of an *inference* as to the nature of its forthcoming A-correlate, either made somehow by the guesser himself or made by someone else and somehow imparted by him to the guesser. But (e) all these alternatives are so many ways of accounting for the difference between the actual and the most probable proportion of hits in the sequence *without* supposing that any of the guesses is an instance of veridical non-inferential precognising. (f) It would seem, therefore, that my *definition* of "foreseeing" rules out the possibility of any of the guesses counting as instances of *foreseeing* the nature of the target-card, no matter how great may be the deviation between the actual proportion of hits and the proportion which would be most probable on the hypothesis of chance-coincidence.

In the above reasoning it is the *second* step to which Professor Flew takes exception, viz., the transition from *denying* that so great a divergence can be a mere chance coincidence to *asserting* that there must be some causal connexion between some at least of the G-events and their A-correlates. Professor Flew's alternative, as I understand it, may be stated as follows:—

(a) It is a fact, with regard to certain persons P_1, P_2, \dots, P_n , who have been investigated, that on all or most occasions when any of them has made a long sequence of guesses under certain assigned conditions, the proportion of hits has differed significantly from the most probable proportion on the hypothesis of chance-coincidence. (b) It is reasonable to believe that the guess made by such a person on any occasion is causally determined, and it is reasonable to believe that the immediately subsequent turning up of a card with such and such a symbol on its face by the experimenter is causally determined. But in a properly designed experiment there is *no* reason to believe that there is *any causal connexion*, direct or indirect, between the former event and the latter, whether the guess be a hit or a miss.

Notwithstanding this complete lack of causal connexion, the following expectations might reasonably be entertained and the following enquiries might reasonably be undertaken, according to Professor Flew. (a) It would be reasonable to expect, with regard to any of these persons, that, if he were to make a further sequence of guesses under similar conditions, the proportion of hits would *still* diverge significantly and in the same direction from the most probable proportion on the hypothesis of chance-coincidence. (b) It would be sensible to compare these persons with each other, and to contrast them with others who (under apparently similar conditions) consistently make scores which do *not* differ significantly from what might be expected on the hypothesis of chance-coincidence, in order to discover some characteristic ψ , common and peculiar to the former. If such a characteristic were found, it would be reasonable to put forward, and to test by further experiment and observation, following wider generalisation:—Most (if not all) persons having the characteristic ψ , and few (if any) who lack it, would, if making a sequence of guesses under conditions C , score a proportion of hits significantly different from the most probable proportion on the hypothesis of chance-coincidence. (c) Even if only the narrower extrapolation, labelled (a) above, were available, the following statement would be true. Suppose that an observer O accepted this extrapolation. Suppose he knew that on a certain occasion one of these persons P_r had guessed that the next card to be turned up would be of such and such a kind. Then O would be justified in conjecturing, with *greater* conviction than would otherwise have been warranted, that such a card *would* be turned up next (if P_r 's previous performance had shown a significant *positive* deviation), or that such a card would *not* be turned up next (if P_r 's previous performance had shown a significant *negative* deviation).

Now, as regards the logic of the question, I find myself largely in agreement with Professor Flew here. Let us assume that the cards are

properly randomised, that the experiments are properly conducted so as to eliminate all possibility of fraud, sensory leakages, and so on. Then the conclusion which can legitimately be drawn from a successful series of such experiments can be accurately stated in the following rather complex sentence: "In view of the actual results obtained with the subject *S*, it is *extremely unlikely* that the probability of his assigning any particular one of the alternative symbols to the next card to be turned up is the *same* no matter whether *that* symbol or any of the alternatives to it will *in fact* be on the face of that card." Now this can be expressed loosely by saying that, in view of the actual results, it is extremely unlikely that the nature of a guess is not "to some extent *influenced by*" the nature of the symbol borne by the card which will be turned up immediately afterwards. The latter expression, taken as it stands, does suggest some kind of *causal connexion* between G-events and their A-correlates. But we must remember that it is simply a shorthand translation of the longer and more complex sentence given above. Now that sentence certainly does not *explicitly* contain any reference to causation. It is couched in terms of "probability," and of variation or non-variation in "probability" according as the data are of one form or another.

That is the *prima facie* case in favour of Professor Flew's contention. I think it is strong, but not absolutely conclusive. Both "probability" and "causation" are extremely obscure and ambiguous notions, and one cannot be quite sure that sentences which explicitly mention only the former may not implicitly refer to the latter.

Without entering into that question, we can ask ourselves the following one:— Under what circumstances would one's initial impression, that the deviation of the actual proportion of hits from the proportion which would be most probable on the hypothesis of chance-coincidence is too great to be a chance-coincidence, be *strengthened*? And under what circumstances would it be *weakened*? It seems to me plain that it would be strengthened or weakened according as the answers to such questions as the following were affirmative or negative. Are the results as a whole repeatable, (a) in the sense that there are generally a few subjects who can produce them, and (b) that each such subject can go on producing them over a longish period when the known conditions are kept as constant as possible? Do they vary concomitantly with certain variations in the conditions? Is there a certain "statistical pattern," which manifests itself in each successive sequence of guesses made by the same subject with the same agent and the same experimenter under the same experimental conditions?

Before concluding this sub-section I would add the following remark.

If we consider in detail how card-guessing experiments are designed and conducted, it seems that in most of those which are said to provide evidence for "precognitive telepathy or clairvoyance" there is no *necessity* to postulate *foreknowledge* at all. The results could in fact be interpreted *causally*, and the causation would involve no *temporal* difficulties or paradoxes, though it would be in other respects extremely peculiar.

This is not the place to develop the point in detail, and the following general hint must suffice. Suppose we assume that the bodily action which the "telepathic agent" performs, on receipt of his cue from the experimenter that the subject has made a guess, is *completely determined causally*. Then a complete causal ancestor of that action must have already existed *immediately before* the subject made his guess. Now it is this action, together with the experimental set-up, which determines *what symbol* the agent will perceive on the receipt by him of his cue from the experimenter. Suppose we assume that the bodily action which the subject performs, in writing down such and such a symbol as his guess about the card which the agent is about to look at, is also *completely determined causally*. Then all that we need to assume in order to account causally for (say) a significant *predominance of hits* is this. We must suppose that that causal ancestor of the *agent's* future action of selecting and looking at such and such a card, which immediately precedes the *subject's* present action of writing down his guess as to the nature of that card, contains a factor which influences the subject to write down as his guess the symbol which it is already determined that the agent will perceive. This would be a very odd kind of causal law, but its oddity would arise from its unfamiliarity and not from its involving causal influence from future to present or later to earlier.

(iii) *The Fatalistic Objection.* I will begin by re-stating the objection. Suppose that at t_1 a person A veridically foresees an event which in due course happens at t_3 ; and suppose that an essential factor in causally pre-determining that event was a voluntary decision, made at a certain *intermediate* moment t_2 either by A himself or by another person B . (The following would be an example. Mr. Jones correctly foresees at t_1 that Mr. Smith will be killed in an aeroplane-crash at t_3 ; and a necessary precondition of this happening was that Mr. Smith decided at t_2 to travel by a certain plane and not by another plane or by boat.)

Then the argument runs as follows. (a) Since the event was correctly foreseen at t_1 , it must have been completely predetermined causally by that time. (b) It must therefore have been completely predetermined causally at the later moment t_2 . (c) Therefore the voluntary decision made by B at t_2 , which was an essential factor in the causal ancestor at t_2

of the event in question, must have been completely pre-determined causally at least as early at t_1 . (d) We must therefore draw the following conclusion in such a case. *Either* (α) B 's voluntary decision at t_2 was *not* (as we had assumed it to be) a causally necessary precondition of the occurrence of the foreseen event. *Or* (β) that decision was completely predetermined causally at least as early at t_1 .

Now both alternatives are highly *distasteful to the feelings* of many, though that is of course no reason for holding that neither of them could be *true*. The first alternative is in the worse position. We have as good reason for holding that Mr. Smith, in my example, would not have travelled by the plane in question, unless he had decided at an intermediate moment to do so, as we have for almost any empirical belief as to the consequences of unfulfilled conditions.

In commenting on this "objection," as I originally stated it, Professor Flew makes the following points:—

(a) He says that he rejects the assumption that freedom to decide between alternatives entails that voluntary decisions are not completely predetermined causally. If the reader will look at my re-statement above, he will see that that assumption is *not* involved. All that I allege is that many people find it distasteful to think that their voluntary decisions are completely predetermined causally long before they come to be made. I think that they would do this, even though they admitted that such decisions would still be *free* in a number of important senses, which were mentioned by Locke and have been repeated *ad nauseam* by other philosophers since his time.

(b) The premiss in this "objection" which I myself questioned was that which comes first in the above re-statement, viz., that, if an event were correctly foreseen at a certain moment, it must have been completely predetermined causally at that moment. I do not know what attitude Professor Flew would himself take towards this premiss. But he does argue that to *reject* it (as I was inclined to do) is inconsistent with certain things which I said or implied about veridical foreseeing.

The point is this. (a) My definition of "veridical foreseeing" entails that an event would *not* be said to have been "veridically foreseen" *unless* there were some kind of *causal connexion* between the experience of "foreseeing" and that event "foreseen." For, otherwise, according to what I said, the correspondence between the two would be no more than a remarkable coincidence. (b) My account of an event being "completely predetermined causally at t " was this. There is a set of facts about the states and dispositions at t of the various things and persons then existing, which, together with the laws of nature, logically entail that an event of precisely that kind would happen at the time and place

at which that event did happen or will happen. (c) Now suppose that there occurs at *t* an experience answering to my definition of "veridical foreseeing." Then *it* must be included among "the states and dispositions at *t* of the various things and persons then existing." And among the "laws of nature" we must include, not only the laws of normal physics and psychology, but also those causal laws (whatever they may be), in accordance with which experiences of veridically foreseeing and the events veridically foreseen are (according to my definition) causally interconnected. (d) But, when that is done, it is not open to me (consistently with my definitions) to question that, if an event is veridically foreseen at *t*, it must be causally predetermined at *t*.

I accept this criticism of Professor Flew's on my consistency, at any rate to the following extent. I certainly did forget to include the experience of veridically foreseeing *itself* among the states and dispositions of the various things and persons existing at the time when it occurred. And I certainly failed to notice that my assertion that there must be some kind of *causal* connexion between an earlier experience and a corresponding later event, if the former is to count as a "veridical foreseeing" of the latter, implies that among the laws of nature there are laws concerning *just that kind* of causal connexion. But it does not seem to me to follow, even when these factors are taken into account, that the event foreseen at *t* must be *completely* predetermined causally at *t*. It might at that moment be causally predetermined *only within certain limits*.

This has a bearing on the last point which I will consider under this head. Consider the following modification of our previous example. Mr. Smith, hearing of the experience in which Mr. Jones ostensibly foresees his death in the crash of a certain plane (or, alternatively, having such an experience himself), cancels his booking and thus saves his life, though that plane does crash and all the passengers on it are killed. It is sometimes said that in such cases the occurrence of a *veridical* foreseeing causes voluntary action to be taken which prevents it from being fulfilled.

About all such cases it seems to me that Professor Ducasse is right. In so far as it is known that a claim has been made to foresee that a certain kind of event will happen at such and such a time and place, and in so far as that knowledge leads to action which *prevents* an event of *exactly* that kind from happening there and then, the claim *as it stands* is mistaken. But, if the claim were re-stated in a *conditional* form, or in a *less determinate* categorical form, there may be no reason against and good reasons for admitting it.

(VII) *The Psycho-Physical Individual*

Under this heading I shall consider comments on what I have written on the nature of the human mind and on the relation between the mental and the bodily aspects of a human being. These topics are the subject of the whole of Mr. Kneale's essay and of parts of the essays of Professor Ducasse and Professor Patterson.

I will begin by expressing my complete agreement with Mr. Kneale's criticisms of the views which he ascribes to a certain influential group of contemporary philosophers in England and U.S.A. I fully accept his conclusion that "all talk of minds presupposes the occurrence of experiences," in a sense in which statements about experiences are *not* reducible to statements about behaviour and tendencies to behave.

As to the persons, mentioned by Mr. Kneale, who profess to be unable to understand what is intended by familiar technical terms, like "experience," "sensation," "event," etc., unless these are used in certain special senses in which they occur in popular speech and writing, I can only say this. I have always given them the credit of not being in fact such fools as they would need to be if their professions of impotence could be taken literally. It is, after all, a very common device, in philosophic and other controversies among well-bred disputants, to use: "I don't *understand* what Mr. X means" as a polite euphemism for: "I understand quite well what Mr. X means, but I think it such obvious rubbish that I shall not waste time in refuting it." By this device two advantages are gained. One gracefully pretends to take upon oneself the blame for stupidity, whilst in fact imputing it to one's opponent. And one avoids the labour of controverting him in detail.

There is one other quite minor point in Mr. Kneale's paper which I will dispose of at once. It concerns my use of the word "know." (i) I agree, of course, that this is primarily a *dispositional* word, though it connotes *inter alia* a disposition to have certain experiences, and not only to speak, write, or otherwise behave in certain ways. (ii) But it certainly is sometimes used in a predominantly occurrent sense. Cf., e.g., the following sentences:— "When that black thunder-cloud blew up during my afternoon's walk, I *knew* that I was in for a wetting" and "All the time he was talking to me I *knew* his thoughts were elsewhere." (iii) Certainly the word "know," like many other cognition-words, such as "see," "remember," etc., is not *merely* descriptive of the experience which the subject is thought to be having at the time or of his supposed disposition. To say that a person "knows" so-and-so, evinces a belief on the part of the speaker that so-and-so is the case. And similar remarks apply *mutatis mutandis* to the statement that a person is "seeing" so-

and-so or that he is "remembering" so-and-so. None of this is precisely news to me. I have insisted on it almost *ad nauseam* in my writings, though probably more in later than in earlier ones.

Passing now to matters of detail, I will take in turn the following matters:— (A) Professor Patterson's comments on certain points in my account of McTaggart's doctrine of the Self and of Self-knowledge, (B) Mr. Kneale's discussion on Epiphenomenalism, and (C) Professor Ducasse's proposed amendments to my "Compound Theory" of the human individual.

(A) THE SELF AND SELF-KNOWLEDGE ACCORDING TO MCTAGGART. On this topic I will make the following comments.

(1) We must begin by reminding ourselves that McTaggart's doctrine of the self is very peculiar, and that it must be taken together with his doctrine of time. In judging it one has to bear in mind, and try to harmonise, the following facts. (i) For him a self, as it really is, is a *timeless* existent; and what appears to it and to others as its successive experiences are really *timeless* existents, ordered in a *non-temporal* series, by a certain asymmetrical dyadic relation of "containing" and "contained in." Thus a self and its experiences, though of course not literally extended, have a property which is in important respects formally analogous to spatial extension. (ii) On the other hand, his argument to show that every self is directly acquainted with itself is based on alleged facts about our everyday experience, as it appears to us under the partly misleading form of a *temporal* sequence of *dated* and *fleeting* mental events. (iii) For McTaggart a self, as it really is, is a non-temporal *whole*, of which its apparently successive total states are, in reality, *parts*, in a sense formally analogous to that in which, e.g., the representation of the Queen's head on an English postage-stamp is a part of the whole design on the front of the stamp. (iv) According to him there is a certain part of each such whole which stands to that whole in the *cognitive* relation of prehension to prehended object.

(2) Since neither Professor Patterson nor I can accept this account of the self, I need say no more about it. But Professor Patterson suggests, as an alternative, that a self may be "present in" the events which make up its history, "as the whole is in the part."

I find two difficulties in this. (i) In what sense is *any* whole *ever* present in any part of it? In what sense, e.g., is the whole design on the front of a penny stamp present in the representation of the Queen's head, which forms a part of it? (ii) I understand that Professor Patterson agrees with me in denying that a self's experiences are *parts* of it. In that case a self cannot stand to its experiences in *exactly* that relation (whatever it may be) which is described by saying that a whole is "present

in" each part of it. At best there might be some kind of important *analogy* between the two relations. But what exactly is the analogy?

(3) I drew a distinction between "perception" and "prehension," and substituted the latter term throughout for the former as used by McTaggart. Professor Patterson objects to this.

Now there is no doubt about the following facts. (i) McTaggart intended to use "perception" for what Lord Russell called "acquaintance with particulars." (ii) He held that, whenever a person is acquainted with a particular, it *presents itself to him* as characterised in certain ways, e.g., as red, as squeaky, as having the emotional tone of anger, and so on. (iii) He regarded it as an almost intolerable paradox to suppose that a particular could *present itself*, to a person who is acquainted with it, as having any character which does *not* in fact characterise it at the time. (iv) "Perception" is commonly used as a general name for such experiences as "seeing," "touching," "hearing," etc.; and what we ostensibly see, touch, hear, etc., is either of the nature of *bodies* (e.g., tables or bells or lamps) or of the nature of *physical events* (e.g., flashes of lightning, peals of thunder, etc.).

Now, everyone admits that the experience of ostensibly seeing, e.g., a cow, *involves* as an essential factor being acquainted with a particular which sensibly presents itself as having a certain characteristic shape, size, arrangement of colours, and so on. And this holds *mutatis mutandis* of the other species of perception. So "perception," in the ordinary sense, certainly *involves* "perception" in McTaggart's sense. But it seems equally plain that it involves something *more* and something *different*. In the first place, a person would not claim to be "seeing a cow" unless he took himself to be in the presence of something which has many specific characteristics, e.g., being an animal that gives milk, which the particular which he is acquainted with at the moment is certainly *not* sensibly presenting itself to him as having. Secondly, no one finds any difficulty or paradox in a *perceived* object being *perceived as having* a characteristic which it does not in fact have at the time. There is, e.g., no paradox in the fact that a stick, which is *in fact straight*, is *seen as bent* when half in air and half in water.

For these reasons I hold to my view that McTaggart used "perception" in an unusual sense; that this usage is liable to mislead through the associations of the ordinary usage; and that it is best therefore to substitute some technical term, such as "prehension" for what he obviously had in mind.

Professor Patterson says that what I call "perception" is "an unholy amalgam of prehension and judgment." As to "unholy," hard words break no bones. As to "judgment," a rather similar point is raised by

Professor Blanshard in his essay, and I may as well deal with it here. Professor Blanshard is inclined to assert that a rudimentary kind of non-demonstrative *inference* is involved in sense-perception. The reason that he gives is that there certainly are *data*, e.g., the visual appearance of the object; that there certainly is a kind of *transition* from them; and that what is reached by that transition must be a *judgment*, since it can significantly be said to be "true" or "false."

Now I think that this question largely turns on the usage of certain words. I should not talk of "judgment" unless a person had before his mind a proposition with a subject and a predicate. I should not talk of "inference" unless that person (a) saw or thought he saw a certain *logical relation* of entailment or of probabilification between that proposition and another proposition, and (b) *in virtue of this*, and of his full or partial conviction of the truth of the former proposition, took up an attitude of full or partial belief in the latter. Professor Blanshard (and, I think, Professor Patterson too) would wish to use both "judgment" and "inference" much more widely than this. Both the wider and the narrower usages have certain advantages and certain disadvantages; but, so long as each party realises how he is using his terms and how the other party is using the same terms, there is no occasion for controversy between them.

Before leaving this topic, I wish to emphasise that the reasons stated above for distinguishing prehension from perception are quite independent of whether or not the object prehended is ever identical with, or a part of, the object perceived.

(4) In terms of this distinction it seems to me that a person's awareness of *himself* must be much more like "perception," in my sense of the word, than like "perception" in McTaggart's sense, i.e., *prehension*. For, assuming the reality of time, a self is something with a long *history*, consisting of its successive experiences, and it is something with an elaborately organised *system of dispositions*. That being so, I should suppose that self-consciousness would resemble *perception* (i) in involving non-discursive awareness of certain particulars as having certain psychological qualities and standing in certain psychological relations to each other, and (ii) in including another kind of cognition (based on the former) which so far resembles judgment as to be significantly describable as "true" or "false," "veridical" or "delusive." But I should not consider the analogy to *sense-perception* to be at all close in any other respect. One's simultaneous non-discursive awareness of one's own experiences is obviously extremely *unlike* sensation, and *reminiscence* obviously plays an all-important part in one's consciousness of one's self.

(5) Lastly, I think it is misleading to say, as Professor Patterson does, that to hold a "bundle-theory" of the self is equivalent to "denying that there is any self at all." A bundle-theory would claim to admit all the facts which we summarise by saying that certain simultaneous and successive experiences "belong to a certain self," and that certain others "belong to a certain other self." What it then professes to do is to give a satisfactory account of these facts wholly in terms of *direct inter-relations between experiences*. Ordinary language does undoubtedly suggest a quite different view, viz., that the experiences which "belong to a certain self" *derive* their characteristic inter-relations from a common relationship in which they all stand to something which is *not* an experience or a group of experiences. Let us call this a "Pure Ego." Then a bundle-theory *does* "deny that there is any self at all," in the sense of a *Pure Ego*; and in so doing it *does* go against the suggestions of ordinary language. But, if it does not ignore any relevant introspectable fact, and if it does give a satisfactory account of all the relevant introspectable facts, it cannot fairly be charged with "denying that there is a self at all." At worst it denies a certain *theory of the self*, which is so embodied in the language in which we speak of mental facts that we have a difficulty in separating it from them and in realising that it is a theory.

(B) EPIPHENOMENALISM. I think that Epiphenomenalism, (in the sense in which Mr. Kneale takes it, is equivalent to what T. H. Huxley called the "conscious automaton theory." It may be summed up in the following three propositions:—

(1) An experience is not a state or modification of any substance, if "substance" be understood to mean a particular existent of a peculiar kind, *other than* a set of intimately inter-related events, which *has* qualities, states, and dispositions, but *is not* a quality or a state or a disposition of anything. (2) The *complete* immediate cause of any experience is a simultaneous *bodily* event in the brain or nervous system of some one living organism. (3) No experience is a cause-factor in the total cause of any *bodily* event. (It is unnecessary to add that no experience is a cause-factor in the total cause of any *mental* event, for that follows immediately from Proposition (2) above.) So far as I can see, these three propositions are logically independent of each other.

This is not the place for me to consider at length the arguments which might be adduced for and against these propositions. As regards the second and the third of them, I will content myself with the following remarks. Both are in *prima facie* conflict with notorious facts. This is admitted by all intelligent epiphenomenalists, and there are certain well known opening moves in the game of trying to reconcile these propo-

sitions with the facts. I would summarise my impression of the whole controversy as follows. I do not think that there is any adequate empirical evidence for either of these two propositions. As to Proposition (2) the utmost that can be said is this. There is fair, but far from conclusive, empirical evidence for holding that a *necessary* condition for the occurrence of any experience is a simultaneous event in the brain or nervous system of some living organism. And there is no *strong* empirical evidence against this, though some fairly well established phenomena of trance-mediumship seem difficult to reconcile with it. Proposition (3) is in a still weaker position. There is such strong *prima facie* evidence against it that it is an extreme paradox. It could be accepted only on a *priori* grounds. And it can be defended empirically only by making liberal drafts on the unobserved and the unobservable; by drawing a distinction between *de facto* invariable accompaniment and causal conditioning; and by holding that only the *former* relation holds between a brain-event and its mental correlate, whilst the latter holds between certain bodily events.

As to Proposition (1) and its logical relationship to the other two propositions, I would make the following remarks. So far as I can see, Proposition (2) is quite consistent with there being *non-causal* relations of the most intimate and peculiar kind, which hold between all or some of the mental correlates of events in *one and the same* brain, and do not hold between any of the mental correlates of events in *different* brains.

Suppose we grant that it is intelligible to talk of a mental event which is not a modification or state of any kind of *substratum*. Then Proposition (2) is compatible with the view that the mental correlates of events in a single highly organised brain constitute a single *mental* system, highly organised in its own characteristic way. That is what a mind would be, on such a view. It might fairly be described as an "empirical substance," except for the following important defect. An essential part of the notion of an empirical substance is that *dispositional* properties of specific kinds can be ascribed to it. Now, if Proposition (2) be assumed, dispositional properties could be ascribed to a mind only by courtesy. Strictly speaking, they would all belong to the *brain*, with which that mind is correlated in the way described.

I will now consider two closely inter-connected objections which Mr. Kneale puts forward. They concern, not so much the *possibility* of a view of minds which takes the notion of an experience or mental event as primary and self-sufficient, as the *consistency* of that view with certain statements which I have made about (i) what I call "prehension" or

acquaintance with particulars, and (ii) introspection. I will take these two points in turn.

(1) The first seems to come to this. I talk in many places as if the statement that X is prehending so-and-so (e.g., is aware of a squeaky noise) consists in a subject S_X (X 's "ego") standing in a certain asymmetrical dyadic relation of "prehending" (in this case auditorily sensing) to a certain particular (in this case an auditory sensible of a squeaky kind). Now, it is said, Proposition (1) explicitly rules out such an entity as S_X , and therefore is incompatible with this account of prehension.

No doubt that it is true. But the following modification of the above account of prehension would remove the inconsistency. Let us say that the asymmetrical dyadic relation is not that of prehending, but that of *being-a-prehension-of*; and let us say that this relation relates, not a "subject" but a *mental event*, to a certain particular. The proposition that X is prehending so-and-so, now consists of a conjunction of the following two propositions:— (i) In that system of organised experiences which is X 's mind there is a certain experience e , and (ii) e has to so-and-so the relation of prehension to prehensum.

(2) The point about introspection seems to be this. I alleged that introspection is comparable, not to *sensation* (which I regard as a species of prehension), but to *sense-perception* (which I consider to involve sensation, but to involve also something fundamentally different). But the appropriate object of sense-perception is a *body*. Now, according to epiphenomenalism, a mind differs from a body in the absolutely fundamental respect that it is *not a substance*, whilst a body *is*.

To this objection I would answer as follows. (i) The *only* analogy which I wanted to draw between introspection and sense-perception was this. In both there is *prima facie* a prehensive factor. In the former this is an immediate *non-sensuous* awareness of some contemporary experience or complex of experiences; in the latter it is an immediate awareness of some *sensible*. In both there is certainly another factor, based upon the former, but carrying the mind beyond what is being prehended at the moment. The "something more," which is "accepted" introspectively in the one case and perceptually in the other, is of extremely different character in the two cases. But that is irrelevant for the present purpose.

(ii) The prehensive factor in introspection could be treated, consistently with a "bundle-theory" of the mind, on the lines indicated above for sensation in my answer to Mr. Kneale's first objection.

(iii) Epiphenomenalists do no doubt regard a *body* as a "substance"

in the *empirical* sense; but they are not, as such, committed to holding that it involves a "substance" in the *metaphysical* sense of a substratum. Now, as I have argued above, they are not precluded from holding that a human mind is at any rate a "half-blown" substance in the *empirical* sense. (It could not be a "full-blown" empirical substance for them, because they deny it to have any dispositional properties.) So I do not think that the statement that for epiphenomalists the objects of sense-perception are substances, whilst the objects of self-awareness are *not*, will bear the weight which Mr. Kneale attaches to it.

(C) THE "COMPOUND" THEORY. I fully agree with Professor Ducasse as to the four defects which he enumerates in the theory of a "psycho-genic" factor, as put forward by me on various occasions. I welcome his attempt to substitute something on the same lines, but more definite and therefore more susceptible to experimental confirmation or invalidation. I will confine myself to the following comments on his proposals.

(1) The theory is a form of substantival dualism. As that type of theory is unfashionable at the moment, and as it has been held in forms which are almost certainly untenable and can easily be made to appear ridiculous by anyone who has a happy turn for phrase-making, I would like to say explicitly that I see no objection *in principle* to substantival dualism.

According to Professor Ducasse's form of the theory, a living human being from his conception to his death is composed of two *substances* intimately interconnected. One of these is purely *physical*, viz., a brain; the other is purely *psychical*, but it is *not* at first a *mind*. It is provided from the first with certain *aptitudes*, i.e., dispositions to acquire certain dispositions; and it is not unless and until it has acquired a number of these dispositions, and they have become organised, that it becomes a *mind*. Until then it can be called only a "psychical germ."

(2) If we ask what exactly is meant by calling such a germ a "*psychical*" substance, we are told that such a substance may be defined as one that has *some* "*psycho-psychical*" dispositions, and *no* "*physico-physical*" dispositions. If we consider Professor Ducasse's definitions of these terms, and of their congeners "psycho-physical" and "physico-psychical" dispositions, we are referred back finally to "psychical" and "physical" as applied to what may be called "*stimulus-events*" and "*reaction-events*." I think that Professor Ducasse might fairly say that the meaning of "psychical" and of "physical," as applied to events, can be made quite plain (except to the hopelessly stupid or the artfully naive) by instances and counter-instances. E.g., a twinge of toothache, as actually felt, is certainly a *psychical* event (whether or not it be in some sense *also* physical). And an electrical disturbance in a certain part of the

brain is certainly a *physical* event (whether or not it be in some sense *also* psychical). A general, and perfectly well understood, name for a psychical event is an "experience," as that word is used in books by psychologists and epistemologists. And physical events may be quite satisfactorily indicated for the present purpose by saying that they are the kind of events which are discussed in books on physics and chemistry and physiology.

That being understood, we can define a "purely *psychical* substance" as follows. It would be a substance which (a) has *some* dispositions which both need an experience to stimulate them, and when thus stimulated react (if at all) by producing an experience; and (b) has *no* dispositions which both need a physical event to stimulate them, and when thus stimulated react (if at all) by producing a physical event. A "purely *physical* substance" could be defined *mutatis mutandis* in a similar way, viz., by substituting "no" for "some" in (a), and "some" for "no" in (b). It is evident from the definitions that no substance could be both "purely psychical" and "purely physical," in the senses defined. But it would be a question of fact whether there are any purely psychical or any purely physical substances. And, even if there were, it would be another question of fact whether there are or are not also substances which possess all four kinds of disposition, and might be called "psycho-physical substances."

(3) I think that there are some obscurities in Professor Ducasse's general account of dispositions, on which the above definitions depend. We are told that to say *S* has a disposition *D* is to say: "In circumstances of the kind *C* the occurrence of an event of the kind *E* causes *S* to respond in the manner *R*." One would like to be told rather more about *where* the stimulus-event is supposed to happen, and *where* the reaction-event is supposed to happen. It seems plain that the stimulus-event and the reaction-event, in the case of a psycho-physical or a physico-psychical disposition, would have to be in *different* substances (and therefore could not both be in the substance *S*) on Professor Ducasse's theory of the human individual. For in such a case one would be in his brain, and the other in his psychical germ (or, at a later stage, his mind).

Suppose, e.g., that there is telepathic interaction between embodied human minds, and that this is not mediated by their brains. Then it would involve *psycho-psychical* dispositions, for which the stimulus-event is in one psychical substance and the reaction-event in another. In view of all this, one is inclined to ask: What is the criterion for attributing a disposition *D* to a certain one substance *S*? Suppose that a stimulus-event *E* in *S*, under the kind of circumstances *C*, gives rise to a reaction-event of the kind *R* in a different substance *S'*. Are we to

assign the disposition always to *S* (the seat of the stimulus-event), or always to *S'* (the seat of the reaction-event), or in some cases to one and in others to the other? Or are we to assign two complementary dispositions, *D* and *D'*, one to *S* and the other to *S'*?

(4) Lastly, I am not altogether happy about Professor Ducasse's attempt to explain in terms of his theory the case of a man, hitherto of an amiable and benevolent disposition, who becomes violent and morose after an injury to his brain.

As I understand it, the explanation proposed comes to this. A man, who predominantly often responds in a *friendly* way to external stimuli which might be responded to either in a friendly or an unfriendly manner, may, when certain transitory internal conditions are fulfilled (e.g., when he is very tired or in a state of great anxiety), respond in an *unfriendly* way to similar stimuli. This shows that he has a disposition to respond in a predominantly unfriendly way, *in addition to* his disposition to respond in a predominantly friendly way, to such stimuli. The prevailing *internal condition* of a benevolent man is such that his disposition to a friendly response generally passes into action, whilst his co-existing disposition to an unfriendly response generally fails to do so. In the case of a morose man we have only to substitute "unfriendly" for "friendly" and conversely, in the last sentence.

Suppose now that a man, who has hitherto been benevolent, becomes morose after suffering a brain-injury; or that one, who has hitherto been morose, becomes benevolent after an operation on his frontal lobes. Then, according to Professor Ducasse, we must say, not that the *dispositions* of his mind have been changed, but that the *internal conditions* necessary for the functioning of a certain one disposition have been suspended, whilst those necessary for the functioning of a co-existent disposition of the opposite kind have been fulfilled.

Now my original difficulty with ordinary dualism was that the facts about changes of character and temperament, after brain-injuries or operations, made it very difficult in principle to know what dispositions to ascribe to a man's *mind*, as such. I do not find this difficulty much lightened by being told that we may and must ascribe to a man's mind all kinds of *opposite* dispositions, and ascribe the predominance of some over others in his habitual reaction to his fellows to the prevalence of this or that background condition of his body or his mind or both.

(VIII) *Sense-perception and Matter*

Beginning with *PPR* (1914) and ending (for the present) with "Berkeley's Denial of Material Substance" (*Philosophical Review*, 1954), I have treated the subject of sense-perception on nine main occasions. The

various essays were written in different contexts, and each usually without reference to its predecessors. It will be obvious to anyone who may take the trouble to collate them that the definitions of such terms as "*sensibile*," "sense-datum," "sensus," etc., which are stated or implied in some of them, are not consistent with those which are stated or implied in others. This seems to me to be a matter of very little interest except to the minute historians of minor philosophers, and I shall not waste time in discussing it. I now think that the least unsatisfactory treatment is to be found in the two latest, viz., "Elementary Reflexions on Sense-perception" (*Philosophy*, 1952) and the already mentioned "Berkeley's Denial of Material Substance." I would, however, warn possible readers that the word "sensus" is used in the former, and the words "*sensibile*" and "sense-datum" are *not*, whilst the *opposite* is true of the latter article.

The contributions of Professors Price, Marc-Wogau, and Yolton are wholly concerned with certain aspects of this question, and I shall now consider them in turn.

(A) PROFESSOR PRICE'S PAPER. Professor Price is concerned mainly with the following two questions:— (1) Does the "act-object" analysis apply to *all* sensations, or do some at least of them require instead the "internal accusative" analysis? (2) Can the so-called "Sensus Theory" deal satisfactorily with certain experiences which a person would commonly express by saying that an object *O* "looks" or "sounds" so-and-so to him?

(1) *Range of Applicability of the "Act-Object" Analysis.* I begin by accepting practically all that Professor Price says in Sections I and II of his paper. All that I need say is that in writings later than *The Mind and its Place in Nature* I have recognised and tried to deal with some of the points which I had hitherto overlooked.

In "Berkeley's Denial of Material Substance," e.g., I admit that the "internal accusative" analysis is quite plausible in regard to vague, peripheral, and unusual *visual* sensations, whilst the "act-object" analysis seems obviously appropriate to the visual sensations which are an essential factor in ostensibly seeing a body of definite outline in the middle of the field of view. Again, although I nowhere deal with sensations of *smell*, I have considered rather fully (in "Normal Cognition, Clairvoyance, and Telepathy" and in later writings) the phenomenological likenesses and unlikenesses between (a) intra-somatic and extra-somatic ostensible perceptions; and (b) among the latter, between ostensible seeing, hearing, and touching. Lastly, in dealing with "touch," I have considered temperature-sensations (both of radiant heat and

those associated with ostensibly touching a hot surface) and what I call "dynamic experiences."

All this, however, serves only to underline my substantial agreement with Professor Price's conclusion in Section II. The distinction between sensations to which the "act-object" analysis seems appropriate, and those to which the "internal accusative" analysis seems appropriate, has to be made *within* sensations of the same sense, and not just between the sensations of certain senses and those of others. And there are always marginal cases, where it seems arbitrary to say that the one type of analysis is more or less applicable than the other.

Professor Price's doctrine, as I understand it, comes to this. When a sensation of *any* kind is occurring, there is always a sensation of a *total sense-field* of the corresponding kind. Let us call such an experience an "*integral sensation*," of the visual kind, of the auditory kind, and so on. The "act-object" analysis applies to *integral* sensations of *every* kind. To have an integral sensation of any kind always consists in sensing a total sense-field of that kind, or (to put it otherwise) in having such a total sense-field *sensibly presented* to one.

But the total sense-field presented in an integral sensation may take three alternative forms. (a) It may not be appreciably differentiated at all. An example would be the total visual field of a person who is gazing up into a cloudless sky. (b) It may be differentiated into several sharply localised and bounded items, standing out from a relatively undifferentiated background. An example would be if the blue sky, in the former example, had a number of small fairly definite flecks of white cloud scattered about it. (c) It may be differentiated, but not in that particular way. An example might be the visual field of a person looking into an iridescent mist.

In cases (a) and (c) we should *not* be inclined to say that the integral sensation is differentiated into a number of constituent sensations, each with its own object. But in case (b) we *are* inclined to say this; and then we apply the "act-object" analysis, not only to the integral sensation, but also to each of these constituent sensations. In cases (a) and (c) we may say that there is *no sensum* at all, but only a *sense-field*, undifferentiated in the one case and differentiated in the other. Or, if we like, we can say that in each of these cases there is a *single sensum*, undifferentiated in the one case and differentiated in the other.

It must be admitted, however, that there are sensory experiences which it is natural to express in language which seems to imply the "*internal-accusative*" analysis, and which it would be extremely strained and unnatural to express in terms which imply the "act-object" analysis. The most obvious examples are certain *organic* sensory experiences,

such as one naturally expresses by saying: "I feel tired," "I feel sick," and so on.

Professor Price deals with these as follows. In such cases a person is having an integral organic sensation of an organic sense-field, which is very little differentiated, and is certainly not differentiated into outstanding localised organic sensa, such as an ache here and a tickle there. He perceptually accepts this organic sense-field as a state of *his own body*. Now on many occasions a person regards his own body as part of *himself*. On other occasions he regards his body, not as a part of himself, but as one thing in the material world, to which he stands in certain uniquely intimate relations. A person tends to use phrases like "I feel tired" when (a) the integral organic sensation, by which he is perceiving his own body, is not differentiated into a number of constituent sensations with localised organic sensa as their objects; and (b) he is taking his body, so perceived, as a part of himself.

Professor Price suggests that there are occasions (rare for most of us, but not uncommon in the lives of certain poets and nature-mystics) when something analogous to condition (b) is fulfilled even in the case of *visual* sensation. The percipient takes, not only his body, as intrasomatically perceived, but also the total object of his visual perception, to be included in himself. He is most likely to do this when his visual field is not differentiated into a number of outstanding strongly localised and bounded sensa. In such cases he tends to describe his visual experience in terms suggestive of the "internal accusative" analysis. (The reader will find a good example in Byron's *Childe Harold*, Canto III, Stanza 72.)

I will now make a few comments on Professor Price's doctrine:—

(i) I wonder why Professor Price is so sure that the "act-object" analysis applies to all *integral* sensations. Is he really sure of this, or would he be content to say that it is *not obviously inapplicable* to any such sensation? For my part, I would not be prepared to go further than this. The cases where the "act-object" analysis seems most obviously applicable are those *partial* sensations which occur as essential factors in ostensible perceptions of bodies with sharp outlines, or of physical events (such as a flash of forked lightning) which are presented as definitely localised and shaped. It is difficult to believe that there is not something significant in the high positive correlation between these two features in a sensation. Yet the latter feature is conspicuously *absent* in most cases of an integral sensation of a total sense-field.

(ii) We must, no doubt, distinguish between (a) the question of what kind of analysis is applicable to a sensation, and (b) the question whether the sensible which it presents (when the "act-object" analysis

seems plainly applicable) could conceivably exist except as a sense-datum to one particular person on one particular occasion. But the two are closely inter-connected. For the "act-object" analysis leaves the latter question open, whilst the "internal-accusative" analysis entails a negative answer to it. It is therefore natural, in cases where the independent existence of the presented sensible not only seems possible but is commonly taken for granted, to accept the "act-object" analysis. It is no less natural, in cases where the existence of the sensible independently of the sensation seems (for whatever reason) incredible, to favour the "internal-accusative" analysis.

Now it is illuminating, in this connexion, to compare and contrast in this respect (a) the visual sense-datum involved in ostensibly seeing a near-by familiar body of sharp outline in the middle of the field of view, and (b) a vivid and detailed "visual image" (recognised by the experient to be such) of such a body. *Intrinsically* the sensation and the image-experience are very much alike. It would seem equally plausible *prima facie* to apply the "act-object" analysis in both cases, and to say that in the one a sensible is prehended and in the other an imaginable is prehended. But the *sense-datum* is uncritically and unhesitatingly taken to exist independently of being sensibly presented on this particular occasion to this particular person, and to have *other* sensible qualities (e.g., hardness, smoothness, coldness, etc.) which it is not sensibly presenting at the time. For the *sense-datum* is uncritically and unhesitatingly taken to be a part of the surface of the body which the experient is ostensibly seeing, in and through his visual sensation. In view of all this we have no hesitation, if the question is raised, in applying the "act-object" analysis here. But in the case of the *visual image* (recognised as such) all this is lacking. The person who is aware of it does not take it to be part of a body, which he is perceiving in and through imaging it; he does not take it to have any other qualities beside those which it is now presenting to him; and (perhaps because of this) most of us find it hard to conceive of a mental image existing except as imaged by some one person on some one occasion. One is therefore strongly inclined *on reflexion* to apply the "internal-accusative" analysis to all image-experiences, though *prima facie* many of them seem to demand the "act-object" analysis.

(iii) In view of this, might there not be something to be said for the following opinion, which I take to have been held, e.g., by Stout and by Prichard? The "internal-accusative" analysis applies alike to *all* sensations, integral or partial. The so-called "act-object analysis" is not really an *analysis*. It is a statement, not about the *internal constitution* of any sensation, but about the part which certain sensations play in certain

ostensible perceptions. No doubt there are intrinsic dissimilarities between those sensations which are, and those which are not, fitted to play this part. But these intrinsic dissimilarities concern the "content" or "*quale*" of the two kinds of sensation, and not the kinds of analysis applicable to the one and to the other.

(2) "*Appearing so-and-so*" and the "*Sensum Theory*." The question which Professor Price discusses under this head may be stated as follows. Does every proposition of the form: *That body appears ϕ to S* entail a proposition of the form: *S is sensing a certain sensible as characterised by ϕ* ? Here "appearing ϕ " is used as a general name to cover "looking ϕ ," "feeling ϕ ," "sounding ϕ ," etc., though most of the cases discussed by Professor Price are in fact instances of *looking so-and-so*. It is to be clearly understood that "appearing ϕ " is used here in such a way that to appear ϕ neither *entails* nor *excludes* being in fact ϕ .

I think that Professor Price tacitly makes the following two assumptions. (i) That any quality which a sensible is *sensed as having* must *in fact belong to it*, and the sensible must have that quality in the *very same determinate form* as that in which it is sensed as having it. (ii) That *in no case* is the sensible which a person senses identical with any part of the surface of the body which he ostensibly perceives through sensing it. Nor *a fortiori* is it identical with any part of any *other body*. I will call the first assumption the "Assumption of Inerrancy," and the second the "Assumption of Non-corporeality."

But I do not think that either of these assumptions is essential to Professor Price's argument here. What he is concerned to do is to call our attention to a series of cases of the following kind. In *all* of them one would say: "That body appears ϕ to S." But, as one goes along the series, one would be increasingly disinclined to admit that S *is* (or even in some cases that he *could be*) sensing a sensible as ϕ .

I agree with Professor Price that no serious difficulty for the general principle under discussion arises until we come to cases where the mode of appearing is appropriate to one sense (e.g., sight), whilst the characteristic which the perceived body is said to appear to have is appropriate to another sense (e.g., touch). An obvious example is when a person, looking at a block of ice or at a picture of a field covered with snow, says: "It looks cold."

I should be inclined to treat such cases as follows. When a person, who has often both seen and felt such things as snow and ice, merely looks at such an object, the sensible which he visually senses is no doubt subtly modified in a characteristic way through the excitement of traces left by his past associated tactual experiences. But I see no reason to think that this modification consists in the sensible literally having the

quality of *sensible coldness*, in addition to such qualities as hue, brightness, distribution of light and shade, etc. I should suppose that it is *these latter qualities* which are modified in a subtle and characteristic way. But we have no simple adjective available to name such characteristic modifications of hue, brightness, distribution of light and shade, etc., and none for the *visual gestalt-quality* in which they are integrated. So we express its presence by using the *verbally* paradoxical adjective "cold-looking."

The following analogy may be helpful. When a person says that something looks cream-coloured, no one supposes that the *sensible* which he senses is made of cream. We know that he uses this phrase simply because there is no simple adjective, like "red" or "purple," available to name the hue in question. Why should we not treat such phrases as "cold-looking," "hard-looking," etc., on similar lines *mutatis mutandis*?

The next critical point in Professor Price's series is where the characteristic, which a perceived object is said to appear to have, is or involves something of such a nature that it cannot be literally present to *any* sense. Examples are: "He looks angry," "He looks ill," etc. To be angry, e.g., is to be feeling certain emotions, to be disposed to speak and act in certain ways, and so on. These are plainly not *sensible* qualities of *any* kind.

The analysis which Professor Price offers of *O appears ϕ to S*, in such cases, seems to come to the following. (a) The sensation, in and through which *S* is perceiving *O*, is a sensing by him of a *sensible* which he senses as having a *sensible* quality ψ , of a certain characteristic kind. (b) In *S*'s past experience objects which have presented that kind of appearance have generally or always been found to have the *non-sensible* characteristic ϕ .

I think that this is a plausible account of the circumstances under which such experiences happen, but that it is hardly an adequate account of the experiences themselves. I would suggest that when a creature (man or animal of the higher kind), whose past experiences have been of the kind described under (b), has a sensation of the kind described under (a), he feels certain characteristic emotions (e.g., apprehension) towards *O*, and that dispositions in him to react in certain ways are stirred and certain feelings arise in connexion with this. These emotions and feelings blend with the purely cognitive factor in the experience. I suggest that the experience which a *person* might express by saying "*O* looks angry to me," and which a *dog* might have but could not express in words, is this blended state of cognition, emotion, and feeling. And I would suggest a similar account of other such experiences.

(B) PROFESSOR MARC-WOGAU'S PAPER. Professor Marc-Wogau confines his discussion to *visual* perception of *bodies*, and I shall here follow his lead. I think that the question which he is primarily concerned to discuss may be stated as follows:—

Let us grant, for the sake of argument, that whenever a person is ostensibly seeing a certain body, e.g., a cricket-ball, he is directly apprehending a certain particular existent, which sensibly presents itself to him as having certain qualities, e.g., brownness, roundness, convexity, etc. Let us also grant, for the sake of argument, that such a directly apprehended particular must *have* any characteristic which it sensibly presents itself as having, and that it must have it in *the precise determinate form* in which it presents itself as having it. It has been alleged that, even when the ostensible perception is veridical and non-hallucinatory (e.g., when there really is a cricket-ball in front of the percipient, and when his ostensible perception really is evoked by the stimulus of light coming to his eyes from it) the particular which he directly apprehends is *never* identical with the body which he sees or with any part of it, and *a fortiori* is never identical with any *other* body or any part of one. I will call this doctrine "the non-corporeality of visual sensibilia." It is this doctrine which Professor Marc-Wogau is concerned to discuss. His thesis is that, whether it be true or false, the reasons which have been alleged for it are inconclusive or positively fallacious.

I shall not waste time in discussing whether the non-corporeality of visual sensibilia becomes *analytic*, if we substitute the phrase "visual sensum" for the phrase "particular which a person directly apprehends when he ostensibly sees a body." That depends on how certain philosophers may have defined a certain technical term. But a person must *already* have persuaded himself that such particulars are never bodies or parts of bodies, *before* he would make that property a part of his definition of the technical term "visual sensum," by which he proposes to call them.

Professor Marc-Wogau is obviously right when he says that we need to be clear as to what we mean by "body" and by "part of a body" before we can appraise the doctrine in question. Suppose, e.g., you allow that the name "body" may be given to any complex of sensibilia inter-related in a certain characteristic intimate way, and that the phrase "being a part of a body" may be applied to the relationship of being a constituent of such a complex. Then some of the arguments which have been used in support of the non-corporeality of visual sensibilia cease to be relevant.

In this connexion Professor Marc-Wogau makes some highly pertinent comments on statements which I have made in various places

about "the common-sense view of bodies." The essential point is that I seem to suggest that certain beliefs about the nature of a body are implicit in the language and behaviour of plain men; that these can be formulated, and constitute an essential part of the connotation of the word "body" and the phrase "part of a body," as plain men understand them; and that it can be seen on reflexion that the sensible which a person senses when he sees a body *never* answers to the conditions required for being a "body" or a "part of a body," in the popularly accepted sense of those words. Professor Marc-Wogau says that I seem to ascribe a *philosophic theory* to plain men; and that it is very doubtful whether they have one, and whether (if they have) I have formulated it correctly. He remarks that sometimes I seem to make it an *objection* to a philosophic theory about bodies and sense-perception that it would "shock common-sense," and at other times advise common-sense to "go out and hang itself" like Judas Iscariot.

What I would now say about this is the following. (1) I still think that ordinary language and practice about bodies and parts of bodies do at least *strongly suggest* that the words "body" and "part of a body" connote certain characteristic properties, for most men at most times, in the entities to which they are applied. I would hardly describe these beliefs as constituting a "philosophic theory." But that phrase is no doubt highly elastic, and I do not wish to dispute about words.

(2) At any rate I would admit (and indeed assert) this. When a professional philosopher, who has reflected on these topics and has been led thereby to draw distinctions which are not recognised and not needed in our ordinary practical dealings with bodies, tries to formulate what he takes to be suggested by ordinary language and practice, the result of his efforts will always be open to the following objections. (i) The beliefs which he ascribes to the plain man are likely to be severally much clearer and collectively much more coherent than those which plain men hold at most times. (ii) It will be of very little use to enquire of supposedly representative plain men whether they do or do not hold the beliefs ascribed to them. The witness will not understand your questions, or see the point of your putting them to him, until you have got him to see distinctions which he would not have noticed if left to himself. At that stage he has ceased to be a "representative plain man," and his conditioning at your hands has probably biased him in favour of an affirmative or of a negative answer to your questions.

(3) In view of this, it is perhaps presumptuous for a philosopher to describe his formulation of what he takes to be connoted by "body" and by "part of a body" as "the common-sense belief." The important thing is that he should formulate it clearly, and that it should not *obviously*

conflict with common-sense notions, vague, incoherent, and half-baked as these may be. I think that the account which I have offered fulfils these modest conditions.

(4) Lastly, I do *not* think that the fact that a philosophic theory (e.g., the Leibnizian theory that what we ostensibly perceive as a body is in fact a certain kind of collection of very low-grade minds) would "shock common-sense," is by itself any good reason for doubting it. As one who is, in his non-professional hours, the plainest of plain men, I would like to do what I can for "poor dear common-sense." But I refuse to regard that thing of rags and tatters as an oracle. And, as a professional philosopher, I should not hang my head or feel wistful, if I should find myself obliged (as a result of recognising distinctions which the plain man ignores, and of viewing synoptically facts which he views only severally, and of some of which he is entirely ignorant) to "shock common-sense" quite severely.

I will now pass from these generalities to Professor Marc-Wogau's criticism of certain specific arguments, which have been put forward in support of the non-corporeality of visual sensibilia. Before doing so I will make two explanatory remarks.

(1) The word "see" is used in ordinary speech in a looser and in a stricter sense. In the looser sense one would talk of seeing a *body* (e.g., a certain cricket-ball). In the stricter sense one would say that, from any one position at any one moment, one sees only *a certain part of the external surface* of the body which one is seeing in the looser sense. Now the only question worth discussing here is whether or not the visual *sensible* which a person senses, when he "sees" (in the looser sense) a certain body, is or is not ever identical with that part of the external surface of that body which he is then "seeing" in the strict sense.

(2) Suppose I were looking at a body whose surface is variegated in colour, e.g., a geographical globe with the representation of the U.S.A. facing me, and with the various States represented by adjoined patches of various colours. Then what I understand by the statement that the *sensible* which I am visually sensing is identical with the part of the surface of the globe which I am strictly seeing, is this. The total coloured particular which I am visually sensing is a part of a certain complete coloured surface of spherical shape (the *rest* of which I am *not* visually sensing), in precisely the way in which the representation of the State of Nebraska (which I *am* visually sensing) is a part of the representation of the U.S.A. (which I also *am* visually sensing). I assume that what Professor Marc-Wogau questions is the cogency of various arguments which have been adduced to show that such propositions as this are *never true*.

Of the three specific arguments which Professor Marc-Wogau considers, I need not dilate upon what he calls the "Argument from Delusive Perceptual Situations." As he says, I have discussed that argument pretty fully in *Mind*, Vol. LVI, Pp. 104–107, and have admitted it to be *inconclusive*. I have not altered my opinion. But I continue to think that the argument has *some* weight. The other two arguments are (1) an argument from *continuity*, and (2) an argument based on the *finite velocity of light*. I will now take these in turn.

(1) *Argument from Continuity*. This argument is based on the alleged continuity in the series of sensibilia visually sensed by a person when the same part of the same body (e.g., the whole of the top of a penny) is seen from a series of positions at various distances from it and at various angles to the normal through its centre. Professor Marc-Wogau complained, quite justifiably, that those who use this argument have generally ignored the fact of "phenomenal constancy," which has been established by experimental psychologists. I alleged that this merely shifts the point of application of the argument to the dividing line between the sub-class of this series of sensibilia within which phenomenal constancy holds and the sub-class for which it breaks down.

To this he answers that there is no reason why *all* the sensibilia on the one side of this line should not be identical with the top of the penny, whilst *none* of those on the other side of it are so. (It may be remarked that, strictly speaking, there would be, on the view in question, only *one* sensible on the former side of the dividing line, though it would answer to a number of different descriptions, each of the form "the *sensible* sensed by *X* from position *P*.") Professor Marc-Wogau suggests that the difference between the two sub-groups of sensibilia might be compared with the difference between a set of trees which could be called a "wood" and a set which could be called a "grove."

This analogy seems to me to be faulty. (i) Since one of the sub-groups would, as I have pointed out, contain *only one* member, it would be analogous to a *single tree* and neither to a wood nor a grove. (ii) When we say that a "wood" and a "grove" melt imperceptibly into each other, *all* that we mean is that there is a rather indefinite range of size and density within which either *name* is equally applicable to a collection of trees. But what is involved in the present case is not a question of the applicability of one name or another. It is the *factual* difference between (a) being identical with a certain part of the surface of a certain body, and (b) not being a part of the surface of any body. This is a difference of kind. If it occurs at all within the series of sensibilia in question, it must separate them at a certain definite point into two

sub-classes of radically different kinds, one of which contains only one member, and the other an indefinite plurality of members.

What seems to me so paradoxical is to suppose that such a series of visual sensations, evoked under conditions which vary continuously, should fall into two sub-classes, having objects of such radically different ontological status.

(2) *Argument from the finite velocity of Light.* I have dealt very fully with this argument in *Scientific Thought*, and again (with special reference to Professor Marc-Wogau) in *Mind*, Vol. LVI, Pp. 120–124. All that I wish to add here is the following.

Professor Marc-Wogau says that we need not consider the argument except in cases where the body seen is so near to the percipient's body that the time-interval between the emission or reflection of light from the former and its arrival at the latter is extremely small compared with the duration of the percipient's specious present. The reason which he gives is that, in the case of a very distant body, e.g., the sun, it is obvious, from a mere comparison of the characteristics which the sensible is sensed as having with those which the body is known to have, that the former cannot be identical with a part of the surface of the latter. The argument in question is therefore *unnecessary* in such cases.

That is no doubt true. But the fact that the argument is not *needed* in the case of very distant bodies does not affect its *validity* in such cases. Nor do I think that Professor Marc-Wogau would claim that it does. But, if that be admitted, the question of continuity comes in. The case where the body seen is near to the percipient's body cannot fairly be considered in isolation from cases where it is very remote. We see bodies which are at all sorts of distances, from close at hand to many millions of miles away. The external causal conditions of the visual perceptions are, so far as we know, precisely similar in kind in all these cases. Is it really credible that there is a certain range of distance, on one side of which the immediate objects of visual sensations are parts of the surfaces of bodies emitting light to the percipient's eye, and on the other side of which they are of a wholly different nature?

To sum up. I think that *all* the arguments for the non-corporeality of visual sensibilia rest on considerations of continuity. In view of the continuity in the external conditions of our visual sensations, I find it very hard to believe that *some* of the visual sensibilia which we sense *are* parts of the surfaces of the bodies which we see, and that others are *not* parts of the surface of *any* body, if to be a "body" and to be "a part of the surface of a body" be understood in the simple literal way which I have tried to state and illustrate. Now I also find it very hard to believe that *all* the visual sensibilia which we sense *are* parts of the

surfaces of the bodies which we see, if "body" and "part of the surface of a body" are understood in that way. Therefore I am strongly inclined to think that *none* of them are. I admit that neither severally nor collectively are the arguments conclusive. What I may call Professor Marc-Wogau's "half-and-half" theory is logically possible; but it is the kind of theory of which I can only say: "If it should be true, I'll eat my hat!"

I will conclude, however, by adding this remark. I have been mainly concerned to work out, for good or ill, theories which presuppose the non-corporeality of visual and other sensibilia. But I have *always* recognised that there are other alternative views of sense-perception which an intelligent and instructed philosopher might take. Moreover, as I have grown older I have realised more and more that the plausibility of that presupposition rests on certain assumptions, e.g., the analysis of ostensible perception into sensation *plus* perceptual acceptance founded upon it, the "act-object" analysis of sensation, and the assumption of inerrancy. And I realise that I was formerly inclined to take all these too much for granted.

(C) PROFESSOR YOLTON'S PAPER. I shall discuss Professor Yolton's paper under the following three headings, viz., (1) the three "alternative ontologies" which he mentions, (2) the notion of "ontological construction," and (3) the alleged "phenomenalist" and "dualist" strands in my writings.

(1) "*Phenomenalism*," "*Phenomenalist Realism*," and "*Dualist Realism*." Professor Yolton defines the first two of these theories in terms of what he calls "sense-qualities." I think he must mean what I should call "sense-qualified occurrents," and I shall so interpret his statements. On that interpretation one might say, e.g., that Berkeley held a form of "Phenomenalism," and that Lord Russell has propounded in some of his works a form of "Phenomenal Realism."

I note that Professor Yolton includes among "sensible qualities" the property of being made of oak. Surely that involves having certain *causal* or *dispositional* properties, which are certainly not *sense-given*, in the way in which, e.g., redness and coldness are. However that may be, it seems to me certain that causal and dispositional properties are an essential element in the notion of a body. But "phenomenalists" and "phenomenal realists" might wish to give an account of such properties as inertia, elasticity, gravitational mass, etc., which "dualist realists" would not accept, and *vice versa*.

I take "Dualist Realism" to involve at least the following propositions. (i) That there certainly or probably are "bodies," in the sense of more or less persistent substances within closed surfaces, e.g., spheres, cubes, etc., which have (a) non-dispositional "extensible qualities"

diffused over their surfaces or throughout their volumes, (b) dispositional properties, e.g., inertial mass, elasticity, etc., and (c) positions in a spatial order of at least three dimensions. (2) That when a person ostensibly sees or touches a body, he is immediately aware of certain sense-qualified occurrents, but that these are *in no case* bodies or parts of the surfaces of bodies, in the literal way in which one such sense-qualified occurrent often is a spatial part of another. They are particulars of a radically different kind. And it may be doubted whether any of them ever exists except as the objective factor in a particular sensation had by a particular individual on a particular occasion.

Now, if this be what is implied by "dualist realism," it seems plain that there is *prima facie* another alternative, which might be called "direct realism." This would accept (1) and reject (2) in the above summary. I think that the phrase "to reject (2)" covers two very different attitudes, which might be called "pre-critical" and "post-critical." At the *pre-critical* stage no clear distinction is drawn between the notion of what I have called "sense-qualified occurrents" and the notion of bodies or parts of the surfaces of bodies. Speaking in terms of a conceptual distinction which is not clearly recognised by the persons concerned, we, who have recognised that distinction, can say that the perceiver simply *takes for granted* (at any rate in perceptual situations which are not regarded by him as decidedly abnormal) that the particular which he is visually or tactually sensing is literally a part of the surface of the body which he is ostensibly seeing or touching. At the *post-critical* stage we have philosophers, who *have* recognised the distinction in question, and are well aware of the arguments against identifying the particulars sensed with parts of the surfaces of bodies seen or touched, but *nevertheless* hold that the former *are* identical with the latter, at any rate in normal perceptual situations.

I suppose that Reid would be an adherent of what I should call "post-critical direct realism." I agree with Professor Yolton and Professor Marc-Wogau that it is dangerous, and perhaps almost a contradiction in terms, to ascribe any philosophical theory to plain men. Yet I am inclined to think that the language and behaviour of plain men, and of philosophers in their non-professional hours, implies or suggests what I should call "pre-critical direct realism."

When *all* the relevant facts, viz., those of physics, physiology, anatomy, and psychology, are taken into account, I think that *direct* realism is very difficult to maintain. I do not doubt that, with enough ingenuity and a good deal of special pleading, it could be saved from downright refutation. But I confess that I do not think that it is worth such intellectual acrobatics. On the other hand, I do not wish to depart

further from pre-critical direct realism than I am forced to do by a fair synoptic consideration of all the relevant facts. That is not because I regard any proposition as sacrosanct, merely because it seems to me to be implied or strongly suggested by common language and everyday behaviour. At most I should say that there is a *prima facie* case for treating such propositions seriously, and seeing how far and in what sense they can be maintained in face of relevant facts which were unknown when ordinary language was formed, and which are seldom or never viewed synoptically even by those to whom they are known.

I think that a form of *dualist* realism can be stated, which fulfils these various conditions on the whole better than any alternative theory known to me. I have in the main tried to work out such a theory. I have never doubted that other types of theory, better in some ways but perhaps not so good in others, can be coherently formulated and plausibly defended. I think, however, that most of them demand more boldness and speculative originality than I have ever possessed, and that it was better for me to stick to my last.

(2) "*Ontological Construction.*" Professor Yolton defines this as an attempt to derive physical objects from instantaneous punctiform event-particles. He rightly contrasts it with what he calls "linguistic construction," which he defines as an attempt to derive, from the phenomenal world, meaningful concepts to apply to the physical world.

I am sure that Professor Yolton exaggerates the significance of what I have said about ontological construction, in this sense, in relation to my account of the physical world. I suggest that this is because he ignores the very special context in which it occurs, and the very special purpose which I had in mind.

So far as I am aware, *all* that I have said on this topic will be found on Pp. 587-608 of *The Mind and its Place in Nature*. It occurs in a chapter devoted to the nature of the *unity of a mind*. The context is a discussion of the question whether it is possible to take the notion of *mental event* as primary, and to regard a *mind* as a certain kind of complex composed of appropriately inter-related mental events. In order to elucidate this I threw out for discussion the question whether it is possible to take the notion of *physical event* as primary, and to regard a *body* as a certain kind of complex composed of such events.

I did not want to devote much space to the development of what was a side-issue of a side-issue. I therefore stated the case in terms of literally punctiform instantaneous qualified event-particles, and talked as if I thought that *these* might be actually existent particulars, and as if bodies might be *literally composed of these*. But I never seriously believed this to be a possible view. Professor Yolton has taken all this too

seriously and with too little reference to its peculiar context. I do not think that it would be profitable to pursue the question further here.

(3) *The "phenomenalist" and the "dualist" strands.* The above misunderstanding (as I think it) does not, however, affect the validity of what I take to be Professor Yolton's main criticism. This, if I am not mistaken, comes to the following. I make great use of the notion of logical construction, and in particular of Whitehead's Method of Extensive Abstraction. But there is a standing ambiguity as to what I supposed to be accomplished by this. Sometimes I write as if I held that statements in which physical-object words and phrases occur can be replaced, without loss or gain of meaning, by more complicated statements which are about nothing but sense-data, sensible qualities, and sensible relations. At other times I write as if I held that physical objects are particular existents of a certain kind, and sense-data (or the sensations in which they are the objective factor) are particular existents of a radically different kind; that there are intimate *causal* relations between certain of the former and certain of the latter; and that the logical constructions serve only to define, in terms of sensible qualities and relations, the concepts in terms of which we have to think of physical objects, their qualities, and their relations. We may call these respectively the "phenomenalist strand" and the "dualist strand."

I should not be much ashamed of this, if each tendency occurred only in different writings, and if those writings differed considerably in date and in main purpose. But I must admit that statements exemplifying each tendency are to be found in one and the same book, e.g., *Scientific Thought*.

I believe that my main intention at the time was to expound and defend a form of *dualism*, viz., what I call in *Scientific Thought* the "Critical Scientific Theory." This may fairly be described as an attempt to refurbish (in the light of criticisms made by later philosophers, and with the help of tools provided by later logicians) the much decried "theory of Representative Perception" or "Causal Theory of Perception," which goes back through Locke to Descartes. Much water (and still more hot air) has passed through the bridges of philosophy since I wrote. But I still think such a theory defensible, and I have not met with any alternative which seems to me less unsatisfactory in view of *all* the relevant facts. The philosophy of the physical world and of our perception of it becomes analogous in certain respects (though profoundly dissimilar in certain others) to the making and testing of a far-reaching *scientific hypothesis*. I am well aware that (to parody St. Paul) all this is "to the Phenomenalists a stumbling-block and to the Wittgersnappers foolishness." But I have always thought that Vienna

contributed more notably to culture by its *Schnitzel* than by its *Kreis*, and *ich kann nicht anders*.

There is one further remark that I would make here. Professor Yolton speaks of the "Principle of Isomorphism," and says that it is essential to my way of philosophising about sense-perception and the world of bodies and physical events. I am inclined to think that what I had in mind was something less determinate and more flexible than what Professor Yolton understands by that principle.

What I would say is this. There is no reason *a priori* why the locus of those physical events which are the rather remote causal ancestors of the various groups of inter-connected sensations by which various persons ostensibly perceive a certain body, should *resemble* at all closely the *percepta* of those persons. There is also no reason *a priori* why it should *not* do so. Antecedently, then, we are free to postulate as much or as little resemblance as we choose between (a) the qualities and inter-relations of the hypothetical system of physical things and events, and (b) the qualities and relations which the objects that we ostensibly perceive present themselves to us in sense-perception as having.

The latter is certainly the only source from which we can derive the *empirical* concepts, in terms of which we think of the physical system as a whole, and of its detailed contents and their varying states and mutual relations. Undoubtedly our thought of these involves also concepts which I regard as *non-empirical*, in that they are not and could not possibly be *sense-given*, in the way in which, e.g., the concepts of colour, of shape, of position, of motion, etc., are so. Examples of such non-empirical concepts, essential to the thought of a system of physical things and events, are the notions of substance, of cause, of disposition, of potentiality and actuality, and so on. But nothing concrete can be thought of *wholly* in terms of categories; there must be a non-categorical "filling" and specification, and this can come only from what is *sense-given*.

If this be allowed, I am willing to leave to experts to decide (i) in what *respects* it is necessary to postulate isomorphism, if we are to form any workable and fruitful conception of the actual physical world; and (ii) to what *degree* it is necessary, and within what limits it is permissible, to postulate isomorphism in those respects.

(IX) *Moral Philosophy*

Under this heading come the papers by Professors Frankena, Hede-
nius, and Kuhn; Mr. Hare's paper; and one section of Professor Blans-
hard's.

(A) PROFESSOR FRANKENA'S QUESTIONS. In order to formulate the questions which Professor Frankena puts to me, I will begin by introducing the phrase "moral sentence in the indicative." This is to denote a sentence in the indicative mood, in which the grammatical subject is a name or a description of a person, an action, an experience, or a disposition (or of a class of such), and the grammatical predicate is some word like "ought" or "ought not," "right" or "wrong," "good" or "evil," used in its *specifically moral* sense. It would not be difficult to show by instances and counter-instances what I have in mind.

In terms of this phraseology, I think that what Professor Frankena asks me may be summarised as follows:—Have I any decided opinion, and, if so, why do I hold it, on the following interconnected questions? (1) Do moral sentences in the indicative express *judgments* or not? (2) If *not*, what does the utterance of such a sentence express? (3) If *so*, do words such as "ought" and "ought not," etc., when used in their specifically moral sense, stand for predicates of a *certain peculiar kind*, which has been described as "non-natural?" (4) If such words stand for predicates which are "*natural*," what account should be given of the "natural" characteristics for which typical words of this kind stand?

Now a short answer, and a true one so far as it goes, to Professor Frankena's questions would be: No! I have no decided opinion on any of these points. But I could say the same about almost any philosophical question. The reasons which incline one to or against a certain opinion on any one philosophical question are always highly complex, and they are always bound up with the reasons which incline one to or against certain opinions on many other philosophical questions. Here, as elsewhere in philosophy, I have tried to clear up the questions and to indicate logical connexions between certain answers to some of them and certain answers to others. These are necessary preliminaries to any attempt to come to a reasoned decision about them. But it does not follow that it is sufficient to enable a person to do this. So far as I am concerned, I find myself now inclined to favour one kind of alternative and now another, but never to come down decisively in favour of any. At most I feel fairly confident that some proposed answers to some of the questions are *inadequate* by themselves.

I will now try to be a little more concrete. Let us give the name "predicative" to all theories which hold that moral sentences in the indicative express *judgments*, in which a *moral attribute* is ascribed to a person or action or experience or disposition. I will begin by mentioning and dismissing one general argument against all predicative theories, which has been thought by many intelligent contemporaries to be conclusive.

It is alleged that a sentence can express a synthetic judgment, if and only if one can conceive and describe some kind of possible perceptual situation or introspectable situation which, if realised, would tend to confirm it or to invalidate it. Now consider such a sentence as, e.g., "Acts of promise-breaking tend as such to be morally wrong." If this expresses a judgment at all, the judgment is certainly not analytic. But, it is said, one cannot suggest any possible perceptual or introspectable situation which, if realised, would tend to confirm or to invalidate what it expresses. So it is concluded that it cannot express a judgment. And a similar argument is applied to all moral sentences in the indicative.

This argument leaves me wholly unmoved. The account of synthetic judgments, which is its main premiss, is obviously a generalisation based *exclusively* on a review of *non-moral* indicatives, and in particular of statements about *physical* and *psychological* phenomena. Now there are admittedly whole classes of sentences in the indicative which seem *prima facie* to express synthetic judgments, and which are plainly *not* of that kind. Moral indicatives are important instances of them. If you first exclude all such sentences from your purview, in making your generalisation about the conditions under which alone a sentence can express a synthetic judgment, and then use that generalisation to show that such sentences cannot express synthetic judgments, you are simply begging the question. For the only legitimate ground for excluding these from your purview, and nevertheless holding that your generalisation covers *all* sentences which express synthetic judgments, would be a *prior* conviction that *these* sentences do not express synthetic judgments.

Dismissing this kind of argument as circular, I would next remark that there are two general principles to which I should appeal in preferring one type of theory to another. They sound rather platitudinous when stated baldly; but, in default of anything better, they are not to be despised. (1) Other things being equal, a theory is to be preferred if it does not have to postulate anything of a kind which is not already admitted as a fact and found to be readily intelligible. (2) Other things being equal, a theory is to be preferred if it does not have to suppose that all men are *fundamentally mistaken* on certain matters with which the whole race is and has always been constantly concerned. Unfortunately these two principles sometimes point in opposite directions.

On the *second* principle, taken by itself, I should be strongly inclined *prima facie* to prefer an ethical theory of the *predicative* kind to one of the non-predicative kind. The normal use of uttering a sentence in the indicative is undoubtedly to *convey information* (true or false). The fact that our moral utterances are commonly couched in the indicative

mood strongly suggests that most men at most times take for granted that they are making and expressing and conveying to others *moral judgments* on such occasions. If they are in fact doing nothing of the kind, but are only e.g., evincing or evoking certain emotions, issuing certain admonitions or commands, etc., their mode of expression seems to betray a fundamental misapprehension of their situation.

On the *first* principle, taken by itself, I should be inclined *prima facie* to favour an ethical theory which holds that moral concepts are *empirical*, in the sense that they are derived from data presented in sense-perception or introspection, in the familiar ways in which, e.g., the concepts *red* or *angry* are derived, and the concepts *mermaid* or *hot-tempered* are derived. On the same principle I should be inclined *prima facie* to favour a theory which makes universal propositions of the form: *Anything that had the non-moral character N would have the moral character M* to be either (a) empirical generalisations, or (b) analytic propositions.

Now, in formulating the two principles I have prefixed to each the conditional clause "other things being equal." The basic requirement of a philosophic theory is that it shall do justice to *all* the facts characteristic of the region with which it deals (including, of course, "higher-order" facts about the inter-relations of the "lower-order" facts), and that it shall neither ignore nor distort any of them. When this fundamental condition of inclusiveness and non-distortion is taken into account, I think that the two principles point in opposite directions.

I have tried to show, in various papers quoted by Professor Frankena, that it is doubtful whether any *predicative* theory can do justice to the facts unless it admits (a) that the concepts of moral attributes are *non-empirical*, and (b) that there are universal propositions, connecting certain non-moral attributes with certain moral ones, which are *synthetic* and yet *necessary*. Now, as I have said above, the second principle would incline one to favour *predicative* theories, whilst the first principle would incline one to favour theories which do not involve either *non-empirical concepts* or *synthetic a priori judgments*.

It is plain that philosophers of two different kinds, who might agree in accepting my argument up to this point, would here diverge from each other. (1) Some are quite convinced that there *can* be no non-empirical concepts and no synthetic *a priori* judgments. They will have to accept some form of non-predicative theory, and make the best of it. (2) Others (including myself) have no such convictions. They will be in a freer position. They are not *obliged* at the next move to accept any form of non-predicative theory, but they are equally not obliged at this stage to *reject* all forms of it. They can view that type of theory sympa-

thetically as a praiseworthy attempt to do without non-empirical concepts and synthetic *a priori* judgments in an important region of human experience. They may even offer a helping hand, as I have tried to do in certain of the writings quoted by Professor Frankena.

Those who feel obliged to accept some form of non-predicative theory will be most usefully occupied in the following tasks. (i) In trying to account plausibly, in terms of their theory, for the main outstanding facts which seem *prima facie* to demand a theory of the *predicative* type. (ii) In trying to adduce facts which seem to fit better into a *non-predicative* type of theory than into any of the predicative type. One such fact, e.g., is that the state of mind (whatever it may be) which is expressed by uttering sincerely and wittingly such a sentence as "That act would be wrong," always tends to evoke a reaction *against* doing the act in question. It might be alleged that this seems to be a *necessary* proposition, and not a mere empirical generalisation about human nature. Now it might be argued that, if what such a sentence expresses is a *judgment*, one will have to hold either (a) that the psychological proposition in question is merely an empirical generalisation, or (b) that it is a *necessary synthetic proposition* known *a priori*. The former alternative seems unplausible; and the latter is one to be avoided, if possible, in accordance with my first Principle. Now it might fairly be alleged that, on some forms of the non-predicative theory, the proposition in question would be *analytic*. That would certainly be a point in favour of such forms of non-predicative theory.

Whether the non-predicativists have succeeded in these tasks or not, I think that there is no doubt that, in the course of their very strenuous efforts to perform them, they have made some valuable contributions to moral philosophy. At the time when I wrote *FTET* moral philosophy in England and the U.S.A. might fairly be described as dormant and apparently moribund. Since then, partly owing to the writings of certain predicativists (like Prichard and Ross) and partly owing to those of certain non-predicativists (like Professor Stevenson and Mr. Hare), it has become one of the liveliest branches of philosophy. *Plurimi pertransibunt et multiplex erit scientia.*

There is one other topic, closely connected with those which I have discussed above, on which I will briefly comment. That is the phrase "non-natural characteristic." As a student at Cambridge I was brought up to believe that it is a fundamentally important proposition of ethics that moral attributes belong to a peculiar category called "non-natural," and that there is something called "the naturalistic fallacy," which most moralists had committed who had written before the light dawned in 1903. When I became Professor of Moral Philosophy, and had to write

a course of lectures on ethics, I was unable to discover any intelligible and tenable account of the meaning of this distinction between "natural" and "non-natural" attributes. It also seemed to me that, unless "fallacy" be used in the improper and question-begging sense of "mistaken opinion," instead of in its proper sense of "invalid bit of reasoning," there was nothing which can be described as "the naturalistic fallacy."

I do not propose to traverse again now this much trodden ground, but I will state briefly and dogmatically the conclusion which it seems fair to draw. *If* words like "morally good (or evil)," "morally right (or wrong)," etc., stand for characteristics, *then* the characteristics for which they stand differ from non-moral ones in being *dependent on the latter* in a way in which no *non-moral* characteristic appears to be dependent on others. No doubt some *non-moral* characteristics are *necessarily* dependent on others, e.g., to have a shape entails having a size. But none of these cases of necessary connexion between non-moral characteristics seems to be at all like the connexion between being a breach of promise and being morally wrong, which we express by saying that being a breach of promise necessarily contributes towards making an act morally wrong.

Now a non-predicativist might accept all this, and simply use it as water for his own mill. He might proceed to argue, in accordance with my first Principle, that any ethical theory which can avoid postulating characteristics of such an odd kind as moral ones would have to be, if there were such, is to be preferred (other things being equal) to one which has to postulate them. Suppose he could then explain in detail, in terms of a certain form of non-predicative theory, how it comes about that moral adjectives *seem* to stand for characteristics of this peculiar kind. Then I think that there would be a fairly strong *prima facie* case for preferring his form of the non-predicative theory to any form of predicative theory known to me.

Now non-predicativists have attempted such detailed explanations. I am impressed, if not completely convinced, by their efforts up to date; and I am inclined to think, at the moment of writing, that it is likely that the truth lies somewhere in that direction rather than on predicative lines. I could not be more definite if Professor Frankena (that kindest of men) were to hold a pistol to my head, which I cannot imagine him doing.

(B) MORAL PHILOSOPHY AND MORAL PRACTICE. The main topic which Mr. Hare discusses is the bearing or lack of bearing of moral philosophy on moral practice. As regards the historical part of his essay I would make the following comments.

Mr. Hare rightly mentions Moore and Prichard as the two most influential English moral philosophers at the time when I was young and for many years afterwards. Each held that the moral concepts which he took as fundamental are not only unanalysable, but also of a unique and peculiar kind. Now anyone who takes such a view must, if he would be consistent, hold that any proposition, in which the subject is described in purely *non-moral* terms and the predicate is or involves one of these moral notions, must be *synthetic*. Mr. Hare thinks that this commits such a philosopher to the particular epistemological view, called by Sidgwick "aesthetic intuitionism." This view he ascribes to Moore and to Prichard, and he thinks that for those who hold it moral philosophy can give no guidance to those who seek to know what they ought to do in various types of situation.

Now I do not think that a person who holds the Moore-Prichard type of theory as to the nature of moral concepts is necessarily committed to aesthetic intuitionism. The latter view may be stated roughly as follows. The only way to discover what is morally good or morally obligatory (as the case may be) in a particular situation is to put oneself actually or imaginatively into that situation, and to note what kind of value-judgment or deontic judgment one then makes. Now I do not doubt that it would be a *necessary preliminary* to giving practical guidance to others that one should oneself often have done what the aesthetic intuitionist has in mind. It would also be a *necessary preliminary* that other men should have done the like, and should have recorded the moral judgments which they then made. But at that stage there are the following two conceivable developments.

(1) Suppose a person admits (as Sidgwick certainly did, and as I imagine both Moore and Prichard would do) the possibility of necessary *synthetic* universal propositions, which can be seen to be true *ex vi terminorum*. Then it is conceivable that one might arrive by "intuitive induction" at a number of synthetic *a priori* axioms, stating necessary connexions between certain non-moral and certain specifically moral characteristics. This alternative would no doubt be rejected unhesitatingly by Mr. Hare and by most of his English and American contemporaries. But in a *historical* account it must be remembered that it has been held by many eminent and influential moral philosophers.

(2) Even if this alternative be rejected, there remains the theoretical possibility of *inductive generalisations*, of a high order of generality and reliability, similar in content to the alleged synthetic *a priori* axioms of the rejected view.

Now such a set of moral axioms, or of well established moral inductive generalisations, *might* be capable of elaborate deductive develop-

ment, and *might* be found to entail consequences which no one could have foreseen. These consequences, together with factual information about the situation in which a particular person is placed, and about the probable consequences of this, that, or the other alternative action, *might* enable a moral philosopher to provide him with valuable (though never infallible) guidance as to how he morally ought to act.

The legitimate source of scepticism here is of course the very general conviction that none of these "mights" is in fact realised. The first alternative would involve admitting that there are *synthetic* necessary propositions knowable *a priori*, and this is very commonly held to be an exploded superstition. The second of them, though it might be admitted to be theoretically possible, seems not in fact to be true. *Either* (a) there are no well established inductive generalisations in morals; *or* (b) if there are, they do not (like, e.g., the laws of motion and the law of gravitation) form a system capable of elaborate deductive development and detailed application.

Passing from the historical to the other parts of Mr. Hare's essay, I agree that many young persons take up the study of philosophy because they are morally perplexed and hope that moral philosophy will give them practical guidance. But I think that this attitude covers a number of different troubles and demands, and I propose to distinguish some of them.

(1) A person may have been brought up to accept as *unconditional* a number of general moral principles, as to how one ought or ought not to act in any instance of certain frequently recurring types of situation. It may be that each of these maxims, considered in isolation on its merits, still seems to him on reflexion to be obviously true. But he may become aware, either in his own life or in the lives of others, of situations in which several of these principles are relevant and it is impossible to act in accordance with one without acting against another.

Moral philosophy could help here, if it could carry out the following programme. (i) Indicate a certain more general principle, which seems on its merits to be at least as obviously true as any of the more special ones. (ii) Show that, in acting on each of the more concrete principles in the relevant kinds of situation, one will *generally* (though not invariably) be acting in accordance with this more general one. (iii) Show that, in the exceptional situations, where several of the more concrete principles are relevant but it is impossible to act in accordance with all of them, this more general principle provides a satisfactory answer to the question how one ought to act. (iv) Suggest the causes which may have made the more concrete maxims seem to be true in their *unconditional* form, when really they are true only in the majority of situations in

which they are relevant. This is the kind of programme which, e.g., Utilitarianism claims to carry out; and it has allayed, or at any rate mitigated this kind of perplexity in many highly intelligent and conscientious persons, such as J. S. Mill and Sidgwick.

(2) A great many conscientious plain men and several very eminent moral philosophers, e.g., Plato, Butler, and Sidgwick, seem to hold the following conviction. *All* moral maxims are subject to a certain implicit condition. When this is made explicit, any acceptable moral maxim would take the form: "In situations of the kind *S* one ought always to behave in the way *W*, if and only if such behaviour would not be in the end and on the whole detrimental to one's own interests." Now the difficulty is that there are kinds of behaviour which seem to many of these very persons to be morally obligatory or to be morally forbidden even in situations where the condition just mentioned seems *prima facie* not to be fulfilled.

If such a person appealed to moral philosophy in his perplexity, its first move should be to clear up the many ambiguities in the phrase "one's own interest." Is this supposed to be confined to one's own *happiness or unhappiness*; or is it to be extended to cover the improvement or worsening of one's own character, intellect, and personality? If the latter, is it to be confined to improvement or deterioration in *non-moral* respects, or is it to be extended to cover *specifically moral* improvement or deterioration also?

So much might fairly be regarded as within the range of *moral* philosophy. But what might be demanded is an assurance that behaviour, which we all agree to be morally obligatory, but which often *seems to be to all appearances* detrimental to the agent's long-term "interest" (however that may be interpreted), can never *really* be so, and therefore is no exception to the general principle in question. Now it seems to me that any attempt to show this would fall outside the realm of *specifically moral* philosophy, since it would turn on the nature and destiny of the human individual and the organisation of the rest of the universe. Philosophy has traditionally been held to be closely concerned with such questions, but the prevalent view among professional philosophers in England and America at the present time is that that is an elementary mistake.

(3) What troubles many intelligent and conscientious persons nowadays is something still more fundamental. There is a certain view of the nature and destiny of man, which seems to have the whole weight of biology and experimental psychology behind it, viz., a "behaviourist" or "epiphenomenalist" view, which I will call for short "scientific materialism." To many people it seems that, if this view be true, the notion of moral obligation must be a mere figment, which arose somehow in the

days of men's ignorance of their nature and destiny, and now survives precariously like a vestigial organ. When they contemplate the scientific evidence they cannot help accepting the materialist account of human nature. When they are engaged in co-operating or competing with their fellow-men they cannot help thinking that they have moral obligations. When they try to bring together these two convictions into one focus it seems impossible to reconcile them. They naturally, and I think quite legitimately, appeal to professional philosophers to help them.

Now philosophers might seek, and in fact have sought, to do this in various ways. One is to try to show that, when the scientific materialist view of human nature and the notion of moral obligation are both properly understood, there is no incompatibility between accepting the former and continuing to hold that men are subject to moral obligations. This type of solution will be helpful, only if it can succeed without having to give such an account of moral obligation as seems to the intelligent and conscientious non-philosopher to distort it or eviscerate it or altogether to dissolve it. Another way would be to admit the conflict, but to deny the adequacy and the ultimate coherency of the scientific materialist account of human nature, whilst granting its plausibility and usefulness in the limited context in which it has arisen. I think that the first type of answer might fairly be said to fall within *moral* philosophy, and the second only within *philosophy* in a wider sense.

It would take me too far afield to attempt to discuss adequately the "test" for rightness or wrongness, which Mr. Hare very tentatively puts forward at the end of his essay. I will consider only the following point. Mr. Hare says that *A* will be inclined to judge it to be *wrong* for him to treat *B* in a certain way, if, on imagining himself to be in a similar situation as *patient* instead of agent, he finds that he would *dislike* to be treated in that way. What is not clear to me is what Mr. Hare takes to be the relevance of this "dislike" on *A*'s part.

It seems to me that all that is *logically* relevant is that *A* should judge that it would be *wrong* for another to treat him as he is proposing to treat *B*. Whether he would *dislike* or *like* being treated in that way seems logically irrelevant.

Perhaps Mr. Hare wishes to assert only the *psychological* proposition that *A* will be inclined to judge that it would be wrong for another to treat him as he is proposing to treat *B*, if and only if he would *dislike* to be treated in that way. If so, I think it is a very doubtful generalisation. Perhaps, then, what Mr. Hare wishes to assert is only the following. *A* needs to be convinced that he would dislike to be treated in the way in question, *not* in order to judge that such action by another towards him would be wrong, *nor* in order to judge (in accordance with Mr. Hare's

principle) that such action by him towards *B* would be wrong, but in order that the latter conviction should have *any practical effect* on his conduct towards *B*. If that is what Mr. Hare means, I think it is a rash generalisation about human motivation.

I am inclined to think that the only relevance of *A*'s *disliking* the experience which he would have if he were to be treated as he is thinking of treating *B* is this. (i) An *important*, though neither a necessary nor a sufficient reason for thinking that it would be *wrong* to treat *B* in a certain way, is that *B* would dislike to be so treated. (ii) An *important*, and perhaps indispensable, way for *A* to gain a *vivid* and *practically effective* belief that *B* would dislike a certain experience is that *A* should imagine himself to be having a similar experience in similar circumstances, and should find the idea strongly distasteful. The vivid and practically effective belief thus gained is not, of course, infallible. It seems to me likely, e.g., that many soldiers do not find the experience of hand-to-hand fighting as horrible as I feel that it must be when I try to imagine myself in their situation. But, though not infallible, it is a most valuable corrective to a common tendency to perform, without any concrete realisation of the consequences, actions which will produce, in those affected by them, experiences which the latter would intensely dislike.

(C) "OUGHT" AND "CAN." The relations between the former and the latter of these notions form the main topic of Professor Hedenius's paper. I would like at the outset to make the following general remark. The treatment of the whole subject in my lecture "Determinism, Indeterminism, and Libertarianism" is extremely condensed and somewhat dogmatic. It omits much that should be included in any adequate discussion; the points raised are not sufficiently developed; and objections and counter-arguments are not considered. Such defects are inevitable when a vast and intricate subject has to be handled in the course of an hour's lecture.

Professor Hedenius draws a distinction between acts which are morally *obligatory* and acts which are morally *imputable* to the agent. He argues that a conceivable act, which it is impossible or inevitable for an agent to do, may nevertheless be morally obligatory. But he holds that, for an act to be morally imputable, it must be at any rate what I have called "conditionally substitutable." I am inclined to think that any difference between us on this matter depends mainly on different usages of certain terms, which undoubtedly are used sometimes in a wider and sometimes in a narrower sense. I will now proceed to develop this suggestion.

Consider the statement that *A* is under an obligation to do *X* at *t*. Does this entail (1, 1) that it is *not impossible* for him to do *X* at *t*? And

does it entail (1, 2) that it is *not inevitable* for him to do *X* at *t*? Next consider the statement that *A* is under an obligation *not* to do *Y* at *t*. Does this entail (2, 1) that it is *not inevitable* for him to do *Y* at *t*? And does it entail (2, 2) that it is *not impossible* for him to do *Y* at *t*?

I think it is easy to show that (2, 1) can be reduced to the form of (1, 1), and (2, 2) to the form of (1, 2). In order to do this one need only note that an obligation *not* to do *Y* is equivalent (subject to two conditions which I will state in a moment) to an obligation to do *something-other-than-Y*. The two conditions are these. (i) It is to be understood that "to do something other than *Y*" includes, as one alternative, refraining from all positive relevant action, e.g., just *not* answering a question. (ii) It is also to be remembered that to be under an obligation to behave in *one-or-another* of several alternative ways does *not* entail being under an obligation to behave in *any particular one* of those ways. Subject to these explanations, I propose to confine the discussion to questions (1, 1) and (1, 2).

Professor Hedenius is undoubtedly right in saying that we often use expressions which seem to imply that the alleged entailment in (1, 1) does *not* hold. Here are some examples. "He ought to have lectured from 9 to 10 A.M. yesterday; but it was impossible, since he was then undergoing an operation." "He ought to be lecturing now; but it is impossible, since he is now stricken with aphasia." "He ought to begin to lecture at 6 P.M. in London this evening; but that will be impossible, since it is now 5 P.M. and the train in which he is travelling from Cambridge is held up by a derailment at Bishop's Stortford."

I am very doubtful, however, whether these expressions in fact show that the entailment alleged in (1, 1) does not hold. I suggest that in each of them "ought" is used in a certain *conditional* sense; that the condition is regarded as obvious and as nearly always fulfilled; and therefore is not explicitly stated. I would expand my first example as follows:—"If and only if he had been able (as he normally would have been) to lecture from 9 to 10 A.M. yesterday, he *would have been* under an obligation to do so. But (owing to the exceptional circumstances of undergoing an operation at the time) it was then impossible for him to do so, and therefore he was *not* in fact under an obligation to do so." The other two examples can be treated on similar lines.

It should be noted that the collapse of a categorical obligation, through the impossibility of performing the relevant action, very often imposes on the agent a categorical obligation to perform a certain *other* action, which is in his power. The lecturer in the delayed train, e.g., ought, if he can, to send a telegram to the person in charge of the arrangements for his intended lecture in London.

Let us now consider the alleged entailment (1, 2), i.e., that if *A* is under an obligation to do *X* at *t*, it follows that it is *not inevitable* for him to do *X* at *t*. Can we think of a relevant and obvious counter-instance?

The first point to notice is this. An action, such as answering (truly or falsely) a question, returning or withholding a borrowed article, etc., has to be considered in two aspects, viz., in reference to the person affected by it and in reference to the person doing it. In respect of the *patient* the important question is: Does the action *in fact* treat him as he has a right to be treated in the situation? In respect of the *agent* the important question is: Is the action done from the *intention* (*inter alia*) of treating the patient as he has a right to be treated in the situation? An action of the former kind may be called "right-securing," and one of the latter kind "right-intending."

Now I think it is certain that we often use "obligation" and "obligatory" in such a way that an action which the agent is under an obligation to do is one that is right-securing, whether or not it be right-intending. If we use our terms in that way, it is obvious that an action which the agent could not help doing may be obligatory upon him. (It is equally obvious that one which he could not possibly do might be obligatory on him.)

But I think it is no less certain that we often use "obligation" and "obligatory" in such a way that an action which the agent is under an obligation to do must be right-intending. Now it seems to be that an action, which the agent *could not help* doing, might indeed be *in accordance with* an intention on his part to treat the patient as he has a right to be treated in the situation. But one could hardly say that such an action was done *from that intention* (*inter alia*). So I do not think that an action which the agent could not help doing could be called "obligatory," if that word is used (as it often is) to connote right-intending and not merely right-securing.

Professor Hedenius says, quite correctly, that we can talk of a man being *forced* to do his duty in a certain manner, e.g., forced to repay money that he owes. I doubt, however, whether this is relevant to the issue. In the first place, "duty" is here used in the first of the two senses which I have just distinguished. What we mean is that *A* is forced to do an act which in fact treats *B* as he has a right to be treated. And, secondly, to say that *A* was forced to do *X* is not generally equivalent to saying that it was *inevitable* for him to do *X*. What it generally means is that *A* would have preferred antecedently not to do *X*, but that he was in a situation where it was practically certain that the consequences to him of not doing it would be extremely unpleasant. It was open to him to

refrain from doing *X* and to put up with the unpleasant consequences. So his doing of *X* was not inevitable.

Very likely I used "obligable" in my lecture in roughly the sense in which Professor Hedenius uses "morally imputable." Let us assume this for the sake of argument, and use the latter phrase in the rest of the discussion. I understand that Professor Hedenius is inclined to agree, up to a certain point, with my account of the conditions which must be fulfilled if it is to be morally imputable to *A* that he behaved in the way *W* in a certain situation *S*. He agrees with me up to the point that *A*'s behaving in the way *W* would not be morally imputable unless it were, *in a certain sense*, "determined by *A*'s ego or self." Now I offered a certain analysis of this latter condition, and said that it seemed to me self-evident that it could not be fulfilled. Professor Hedenius offers an alternative analysis, which would not be open to that objection.

If I understand him aright, the essential features in his account are as follows. We have at the back of our minds a reference to a certain large class of persons (e.g., contemporary middle-class Englishmen above the age of puberty); and we have the thought of a certain type of personality as *normal* in that class in respect to the nature and strength and organisation of a number of important conative-emotional dispositions (e.g., desire for food and drink, desire for money, sexual desire, tendency to react with hostility when thwarted, and so on). The agent is assumed to be a member of such a class of persons. We regard a bit of behaviour on the part of a member of such a class as "determined by his ego or self," when and only when the following conditions are fulfilled. (1) The stimulus must be of a kind to which (i) all members of the class are quite often subjected, and (ii) in response to which most of them on most occasions would behave in a certain way *Z*. (2) The individual in question *A* behaved, when so stimulated, in a markedly different way *W*.

Now I think that the distinctions which Professor Hedenius draws are important in reference to the degree of *merit or demerit* which we ascribe to a person in respect of a bit of intentional behaviour. We do not get morally excited when a person behaves rightly under circumstances which frequently occur in the lives of all of us, and in which most of us generally do act rightly. Nor do we get morally excited when a person behaves wrongly under circumstances which are highly exceptional, and in which one suspects that most such persons would act wrongly and is very doubtful whether one would have acted rightly oneself.

It seems to me that all this comes fairly easily under the sense of "ought" and "ought not" which I described in paragraph (ii) of the Section entitled, '*Various Senses of "Obligable"*' in my lecture. I said of

this that "a clear-headed Determinist should hold either that this is the only sense, or that, if there is another sense, in which obligability entails *categorical* substitutability, it has no application." But I added that I am inclined to think that we often use "ought" and "ought not" in another sense, and that in this other sense they entail categorical substitutability. I think that this is most obvious when one makes judgments about *oneself*, of the form "I ought to have done so-and-so" (which I did not do), or "I ought not to have done so-and-so" (which I did). I cannot help thinking that a reference to what the average middle-class Englishman above the age of puberty would or would not generally do, when subjected to the stimulus to which I was subjected, would serve only as a rough measure of the degree of my delinquency, and not at all as an analysis of my conviction that *I*, under the very circumstances in which I in fact failed to do my duty, *could* instead have done it.

(D) THE "EXISTENTIAL" ACCOUNT OF HUMAN PERSONALITY. I understand Professor Kuhn to be using "existence" throughout nearly the whole of his essay in a certain technical sense, *viz.*, to denote the peculiar kind of being which he holds to be characteristic of a *person*, and to be revealed to each of us by the reflexive awareness which is an essential factor in personality. If I understand Professor Kuhn aright, what he takes such reflexive awareness to reveal to each person may be described as follows. What a person now *is* is what he has *made himself*, through the reaction of himself as active, spontaneous, and selective, upon himself as passive and malleable. Furthermore, he, as he *now is*, is actively engaged in determining and generating himself *as he will become*, by a further process of selection and action. This interest in, and self-direction towards, the *future* is particularly characteristic of a person. Moreover, each person has a unique and fundamental concern for *himself*, and this is alleged to be an essential condition of "the absolute validity of moral obligation and moral claims in a person."

I am willing to accept much of this, if I am allowed to interpret it as follows, and to put certain qualifications upon it. In the first place, it is certainly characteristic of human beings (as contrasted with other animals, and especially with certain insects) to be born with extremely few and comparatively unimportant *first-order* dispositions. They are born, instead, with what Professor Ducasse calls "aptitudes," *i.e.*, dispositions to *acquire* dispositions and to *organise* those which they acquire. In so far as statements to the effect that a person is not "an entity fixed and bound by its own whatness" are interpreted in this way, I think that they are true and important.

On the other hand, we must not overlook the fact that what a person can make of himself, even under the most favourable conditions, is lim-

ited by his innate endowments. It is true that no one knows even approximately what are his own or another person's ultimate limitations. It is true too that it is generally undesirable for a person to dwell on this topic in his own case, or for his neighbours to express a confident and narrow view about it. Lastly, it is true that experience shows that a person, who seems *prima facie* to be hopelessly handicapped, physically or intellectually or morally, sometimes does (if he seriously takes himself in hand, and if others give him understanding help) achieve a development of personality which seems well nigh miraculous. But I see no reason to believe that the possibilities are in fact unlimited in any case, or that the limits in each particular case are not fixed by the innate constitution of the individual.

Allowing that there is an important sense in which it is true that each of us is continually making and re-making himself, we must not exaggerate the part played by the *deliberate action* of the individual himself in this process. In the case of most of us it is but fitfully and for short periods that one "takes oneself in hand" and sets out to make oneself a person of such and such a kind. In the main each man's personality is moulded for him in early life by the pressures of family, of school, of business, by the newspapers, the wireless, and the films. These influences are (after occasional struggles, which leave their scars in all, and mar the personalities and wreck the lives of some) generally assimilated fairly thoroughly, though of course *in modo recipientis*. Thereafter the reactions of most men of a given social group in normal situations are almost automatic. Doubtless the power to make a hard deliberate choice, which one realises will profoundly modify one's life and personality, remains latent in everyone. If faced with a crisis, some few of us might make such a choice. But I suspect that in most men that power has become so repressed and overlaid and atrophied in middle life that the chance of its being exercised, if a crisis should face one, is negligible.

"Existentialism," as presented by Professor Kuhn, seems to me to be an account of human nature derived from contemplating men of forceful and original character, making hard (and for themselves and those near and dear to them, at any rate) far-reaching decisions. It is certainly most important not to neglect this heroic side of human nature, and not to forget that it can and does show itself in what we might be tempted to regard as very ordinary men and women in very humdrum circumstances. But that should not make us ignore the dim and petty background (against which these cases shine forth by their rarity), summed up in the epitaph which might so fittingly commemorate most of us:—

Too bad for heaven, too good for hell;
So where he's gone I cannot tell.

Professor Kuhn's main criticism on what I have written about human personality is that I have treated a person and his doings and sufferings as if they were exactly like a physical thing and what happens to it, and have treated voluntary action as if it were exactly like physical causation. I must admit that there is much truth in this, as regards my published works. I can, however, assure Professor Kuhn that I am not, and have never been, a "physicalist" (as I understand that word) about human nature. I regard the differences between men and any non-human animals of whom we have knowledge, as quite fundamental, however they may have arisen in the course of evolution. And I consider that causation, as it shows itself in rational cognition, deliberation, voluntary decision, and considered action, has certain unique peculiarities as contrasted with either purely physical causation or psychological causation at the non-rational level.

The only other matter on which I will comment is this. Professor Kuhn twits me with some *obiter dicta*, which occur towards the end of *The Mind and its Place in Nature*, to the effect that the human race might possibly escape disaster by applying psychology and genetics to "deliberately altering the emotional constitution of mankind, and deliberately constructing more reasonable forms of social organisation." He asks me what I think about that now.

My answer is as follows. It seems to me even more likely now than it did then that, unless opportunities for organised scientific research should be destroyed in the near future, the *knowledge* and the *power* will be available to determine the kind of individuals who shall be born (or incubated), and to mould their nature at will after birth. Such knowledge or power *could* be used on a large scale at any moment only by that person or that group who then have control in a given society. They *would* be used only in so far as those in control knew of them and desired to use them, and the *ends* for which they would in that case be used would depend on the wishes and ideals of the controllers. Given all this, the scheme would be *effective* only in so far as those in control could apply it on a large scale by consent or through inadvertence, or impose it by fraud or by force or by propaganda on the rest of the society.

Plainly that would give an unprecedented power for good or for ill to those who are in a position to use it. Beyond that platitude there is little that I can say except to add the following supplementary platitudes.

(1) There is little likelihood that the scientists, who had the knowledge, would be any more than the tools, or at best the willing technical advisers, of those who had the power to apply it. (2) Even if, by some strange chance, the relevant scientists should also be in effective control, that would be no guarantee that a good use would be made of the power.

There is no reason to think that the ideals of psychologists and geneticists, as such, in regard to human nature and society, would be better (as distinct from more practicable) than those of trades-unionists, businessmen, lawyers, soldiers, or professional politicians. Nor is there any reason to think that psychologists and geneticists, as such, would be any less susceptible than other men to the corruptions of power. (3) I am inclined to believe that there is a rather strong *negative* correlation between the qualities which help a man to get and to keep power in a highly organized industrial society of the modern type (whether capitalist, social-democratic, or communist), and the qualities which tend to endow a man with high ideals of human personality and human society. I should therefore think it much more likely that the powers in question, if used at all, would be *misused* than that they would be applied to good ends. (4) On the other hand, it seems to me plainer than ever that, *unless* the emotional make-up of the average citizen throughout the world *be* profoundly modified in certain ways in the fairly near future, the chance of humanity escaping a large-scale disaster is very slender.

Existing societies are composed of persons whose emotional reactions are largely infantile or anachronistic, i.e., adapted to situations utterly different from those with which men are now faced. They are wholly dependent for their livelihood on a complex and delicate web of economic conditions, which no individual understands. They are now brought into ever closer and more irritating contact with each other, through the development of means of quick communication and the inordinate growth of population, and their emotions are continually played upon by wireless propaganda. All the conditions for an explosion are thus given. And now such persons and societies, whom a sensible parent would hesitate to trust with a popgun, are provided with atomic and hydrogen bombs, and with rockets to convey them. So there is every prospect that the explosion, when it comes, will be shatteringly destructive.

These seem to me to be reasonably probable inferences from fairly plain empirical facts, and I do not think that their plausibility is much affected by whether one holds a "physicalist" or an "existentialist" view of the nature of human personality.

• • • • •

Philosophy is essentially a middle-aged man's game, though certain philosophers (notably Plato and Kant) have put up their best performances when they were well past middle life. Those of us who are not Platos or Kants are well advised to retire gracefully before they have too obviously lost their grip. Medical science would almost have made the

world safe for senility, if physics had not made it unsafe for everybody; and there are far too many old clowns arthritically going through their hoops, to the embarrassment of the spectators:—

From *X*'s eyes the streams of dotage flow,
And *Y* expires a driveller and a show.

My younger colleagues would have no difficulty in substituting appropriate constants for the variables in these lines. Moreover, though philosophies are never refuted, they rapidly go out of fashion, and the kind of philosophy which I have practised has become antiquated without having yet acquired the interest of a collector's piece:—

New forms arise, and different views engage,
Superfluous lags the veteran on the stage.

So this veteran now definitely makes his last bow as a professional performer, though he may occasionally make a graceful appearance "by request" at a matinee for charity.

Cambridge, 14 December, 1956


 A handwritten signature in black ink, appearing to read "C. D. Broad". The signature is written in a cursive, somewhat stylized hand. Below the signature, there are two parallel diagonal lines that extend from the bottom left towards the right, underlining the signature.

BIBLIOGRAPHY OF THE WRITINGS OF

C. D. BROAD

TO THE END OF JULY 1959

Compiled by

C. LEWY

ACKNOWLEDGEMENT

I am grateful to Professor Broad for looking through a draft of this bibliography, and drawing my attention to three items which I had out as well as to ten items which were published unsigned or under a pseudonym.

C. Lewy

CAMBRIDGE
August 1959

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