Physics and Perception

Bertrand Russell

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V.—DISCUSSIONS.

PHYSICS AND PERCEPTION.

My purpose in what follows is to deal with certain questions raised by Mr. C. A. Strong in his article on my theory of the external world (Mind, July, 1922). Mr. Strong finds difficulties in certain points of my theory of perception, which are really points in my theory of physics. The main purpose of this whole outlook is, in my view, to fit our perceptions into a physical context, and to show how they might, with sufficient knowledge, become part of physics. I think the questions at issue between Mr. Strong and me all concern matter rather than anything psychological; I shall therefore begin by treating the topics to be discussed purely from the point of view of physics.

Mr. Strong is surprised (p. 309) that I should suppose particulars which are members of different pieces of matter to exist all at the same place, in case the place is one reached by light from all these different pieces of matter. He is also surprised (p. 310) that objects are "apparently everywhere except in the place where we see and feel them," and that "a multitude of events, all happening after 12 o'clock, should be the constituents of an event happening at 12 o'clock". He points out, as though it were a consequence I had not observed, that "the object, as physical science conceives it, is not correctly defined as *the system* of all the perspectives . . . but is rather their mathematical limit" (p. 311).

It is true that I maintain these propositions (to which I shall return presently), and it is true that they are somewhat curious. But the curiousness is that of modern physics, for which I am not responsible. (I wish I were.) A piece of matter, according to modern physics, has two aspects, one gravitational, the other electromagnetic. (If Weyl is right, as seems highly probable, these two aspects can be reduced to one, but it is not necessary for me to assume that this reduction is possible.) Both these consist of a field, extending theoretically throughout space-time. The gravitational field consists in a certain distortion of space-time, making it everywhere more or less non-Euclidean, but particularly so in a certain neighbourhood, the neighbourhood in which we say the matter is. The electromagnetic field consists of "something" (the physicist cannot say more) which satisfies Maxwell's equations, and is likewise theoretically diffused throughout space-time, though with more intensity in a certain neighbourhood. As Einstein puts it:—

"Da nach unseren heutigen Auffassungen auch die Elementarteilchen der Materie ihrem Wesen nach nichts anderes sind als Verdichtungen des elektromagnetischen Feldes, so kennt unser heutiges Weltbild zwei begrifflich volkommen von einander getrennte, wenn auch kausal aneinander gebundene Realitäten, nämlich Gravitationsaether und elektromagnetisches Feld oder—wie man sie auch nennen könnte—Raum und Materie."[1]

In every little region of space-time, according to this view, there are two things to be considered: first, the metrical structure of the neighbourhood, which represents gravitation, and can be analysed into a number of superposed gravitational fields, each with a centre, which may be taken to be an electron; secondly, the electromagnetic field, which is similarly analysable. There are thus a number of things happening everywhere always. What we call one element of matter—say an electron—is represented by a certain selection of the things that happen throughout space-time, or at any rate throughout a large region. We cannot speak in any accurate sense of the "history" of a piece of matter, because the time-order of events is to a certain extent arbitrary and dependent upon the reference-body. Each piece of matter has, however, a "proper time," which is that indicated by clocks that share its motion; from its own point of view, this proper time may be used to define its history. But it must be understood that a piece of matter is only a convenient grouping of occurrences which extend throughout space-time; it is these occurrences, not matter, that physics accepts as ultimate.

These occurrences are ordered in a four-dimensional continuum called space-time, each "point" of space-time containing many such occurrences. There are no accurate large-scale metrical relations between occurrences in different regions, but within one small neighbourhood there are approximate metrical relations, which are more or less non-Euclidean according to the gravitational field in the neighbourhood. All ultimate physical relations are embodied in differential equations, and only hold accurately in the infinitesimal. There are no longer such things as straight lines of finite length, or time-intervals that are not very short. That is to say, all relations between distant occurrences, even those that we have been accustomed to regard as purely geometrical or chronological, proceed by propagation through the intervening region, like light.

It is into a physical world of this description that we have to fit our theory of perception.

Before proceeding to this task, there is a philosophical question to be disposed of. Mr. Strong says:—

"Mr. Russell accepts the phenomenalistic principle—as we may call the denial of the legitimacy of transcendence—and his theory is, in the main, the application of it to perception. Does he

[1] *Aether und Relativitätstheorie.* Rede gehalten am 5 Mai, 1920. Berlin, Julius Springer, 1920, p. 14.

adhere to it strictly everywhere? The question is pertinent; for a principle which you can disregard when it becomes inconvenient is perhaps only a prejudice."

I have never called myself a phenomenalist, but I have no doubt sometimes expressed myself as though this were my view. In fact, however, I am not a phenomenalist. For practical purposes, I accept the truth of physics, and depart from phenomenalism so far as may be necessary for upholding the truth of physics. I do not, of course, hold that physics is certainly true, but only that it has a better chance of being true than philosophy has. Having accepted the truth of physics, I try to discover the minimum of assumptions required for its truth, and to come as near to phenomenalism as I can. But I do not in the least accept the phenomenalist philosophy as necessarily right, nor do I think that its supporters always realise what a radical destruction of ordinary beliefs it involves.

I will try to make more explicit my attitude on this question. Mr. Strong says: "You cannot go beyond your own sensations, or the world as they present it to you" (p. 312). This position is not maintained by Mr. Strong on idealist grounds, but on grounds of straightforward theory of knowledge. His position is, I suppose, that we have immediate knowledge of our sensations, but that this knowledge is not of a sort to enable us to infer anything other than our own sensations. I should reply: (1) that we do not immediately know either our own sensations or anything else; (2) that no atomic fact can ever be demonstratively inferred from any other atomic fact; (3) that, assuming the validity of induction and analogy (on the lines set forth by Mr. Keynes), it is possible to make *probable* inferences from one fact to another; (4) that in such inferences the inferred probable fact need not lie within our experience or anyone else's; (5) that philosophy has immensely overestimated the importance of knowledge, which is merely a physical relation between physical occurrences, or at any rate does not differ from this in any important way.

(1) The question of "immediate knowledge" can only be treated historically; it only arises when we have reached the stage of Cartesian doubt. At this stage, we find ourselves with a system of beliefs as a going concern, and it strikes us that some of these beliefs are inferred, while others are not. It is difficult to define what is meant by an "inferred" belief. When, for example, you see a tree beginning to fall, you expect to hear a crash; but this belief as to the future has not been inferred by a logical process. It is, however, derivative from what you see; if your belief were challenged, you could allege what you see in justification. All beliefs which are in this wide sense derivative come within the scope of Cartesian doubt, unless some logical process can be found by which they might have been inferred from beliefs which are in no sense derivative, or at any rate not derivative from other beliefs. When we start to look for beliefs which are not derivative, we find,

at first sight, three kinds: perceptions, memories, and logical principles. To begin with the last: I suspect that logical principles are really always propositions about symbols, and are derivative from perceptions of geometrical (or quasi-geometrical) relations among symbols. That is to say: a logical principle never asserts that this can be inferred from that, but only that this symbol and that have the same meaning; the assertion that two symbols have the same meaning is based upon a relation between their forms.[1] It would take me too far from my theme to dwell further upon this question. As for memories, it might be thought odd to include them among non-derivative beliefs. I do not think one would naturally include them in the usual case, where what is remembered was perceived when it happened; but when, as sometimes occurs, we remember something which we did not notice at the time, it seems as if the remembrance had the same claim to be regarded as non-derivative as a perception would have. This brings us to perceptions. Mr. Strong does not discuss my analysis of perception, which, so far as I know, is orthodox; I assume that he does not seriously disagree with it. Thus a perception consists of two parts: (*a*) a core of sensation; (*b*) images and beliefs called up by the sensation through the influence of past experience. The second part is derivative in an important sense, and must certainly be included within the scope of Cartesian doubt, since the beliefs which enter into it are sometimes erroneous. The first part does not consist of beliefs, but is a mere occurrence, of just that kind with which physics is concerned and by which its theories are empirically tested. Moreover, it is a difficult question of theory to discover the sensational core in a perception, and it is highly doubtful whether it can be distinguished from the accompanying images except by assuming an external world with which it is correlated in a way that images do not exactly share. We are thus left with nothing immediate except the core of sensation, which is not knowledge, and is not itself immédiately *known*.

(2) That no atomic fact can ever be demonstratively inferred from any other atomic fact is again a view which I have taken from Mr. Wittgenstein; it obviously hangs together with his view of logical principles. It is of course a view at least as old as Hume. I do not see how anyone can dispute it who examines the nature of logical inference. I do not mean to deny that from the fact that two propositions are true we can infer that each of them severally is true, nor do I mean to deny the validity of Barbara. But such methods do not give *new* knowledge, and I do not see how the methods of demonstrative logic can ever do so. An "atomic" fact is one which does not consist of two or more facts; it is implied that it is not general.

(3) On the question of probable inferences by induction and analogy, I am prepared for the moment to accept what Mr. Keynes

[1] I have adopted this view from Mr. Wittgenstein. See his forthcoming work on *Philosophical Logic* (Kegan Paul).

has to say, though I have no doubt that he has only laid down the broad lines of a theory which can be amplified. I assume in practice the validity of inductive methods as they occur in science, although I do not think that the results obtained are certain. In many cases they are far from certainty, but no other method will lead to results having a higher degree of probability. It is because of the absence of certainty that it is desirable to organise and interpret science in the way involving fewest assumptions; this is the reason why, in practice, I approach as near to phenomenalism as I can without destroying the whole edifice of science. What is involved is not an absolute philosophical principle, but a method of securing a higher degree of probability.

(4) When inferences are made by analogy and induction, what is inferred need not lie within our experience or anyone else's. It is clear, in the first place, that we only employ such inferences when what we infer does not lie within our present or remembered experience. We therefore habitually assume that they are valid as applied to future experience. If so, what can prevent them from being valid as applied to something not experienced at all? There is of course the idealist position, according to which it is logically impossible for anything to exist without being experienced; but this, I gather, is not Mr. Strong's position. His view is merely that there can be no valid inductive inference from what has been experienced to what will never be experienced. I do not know how he would justify this view. Of course it is true that what is not experienced cannot be directly verified, but it can form part of a body of hypothesis of which other parts can be verified. It is natural to suppose that Neptune existed before it was discovered. If you see your cat running away with a fish which was in your larder, it is natural to suppose that the cat has been in the larder. Such inferences transcend experience in the way which Mr. Strong regards as illegitimate; but I do not see in what respect they fail to conform to the canons of inductive inference.

(5) Since Kant—perhaps since Hume—philosophy seems to me to have overemphasised the importance of knowledge, and the difference between what we know and what we do not know. Perhaps the trouble goes further back, to the Cartesian emphasis on the difference between mind and matter. It is clear that Mr. Strong regards our visual perceptions as something very different from what physics treats as light, and that his reluctance to infer beyond experience is bound up with this belief that, if there is anything beyond experience, it must be very unlike sensations. I believe this to be a mistake. To my mind, the world is full of particulars of the sort dealt with by physics, and some of these particulars (namely those in places where, as we say, there is a brain) have peculiar effects which are called "being known" or "being experienced". I think that particulars (of which there are many in one "point" of space-time) can be collected into sets of such a sort that two neighbouring members of the same set differ very little.

I think that when I see (say) a penny, what I perceive is one member of the system which is the momentary penny, and that it is that member which is situated (according to one meaning of "situation") in a certain part of my brain. I think that, very near this part of the brain, there are closely similar unperceived particulars which are other members of the momentary penny; there is no solution of continuity in passing from what I perceive to the outside particulars dealt with by physics.

If this view is correct, a mental occurrence is to be called a "perception" when it has a certain kind of relation, based upon certain differential laws of change (those of perspective, to the first order), to a number of other occurrences all linked with each other in the same way. Common sense imagines that there is a "thing" which "causes" all these occurrences, but that is an unnecessary hypothesis, which is avoided by defining the "thing" as the group of these occurrences. Thus a "perception" is a member of a thing occurring in a place where there are mnemic effects, and it is these mnemic effects which give rise to what is called knowledge of the thing.

Why should such a theory be thought probable? (I do not claim that it is more than probable.) (*a*) Because it harmonises physics, the physiology of the sense-organs, and psychology. (*b*) Because it fits perception into that correlation by differential equations to which all physical laws have been reduced, and avoids treating perception as a case of action at a distance; in other words, because it is in harmony with the principle of continuity, which, though not logically necessary, has been found increasingly fruitful in science. (Continuity here is not to be understood in a strict sense, but only in an approximate sense. In its strict sense it is incompatible with the theory of quanta, and very likely false.)

My view of the relation of what we perceive to physics is the same as that of Dr. Whitehead, who first persuaded me to adopt it. I do not think that he would agree with my psychology in other respects, but in this matter the view set forth in his two books is practically the same as that which I am advocating. He holds, as I do, that colours and sounds and secondary qualities generally should not be extruded from the physical world. The habit of shutting them out he calls the "bifurcation of nature". But he still allows a bifurcation between nature and mind, perhaps only because he deliberately excludes mind from his theme. I wish to include nature and mind in one single system, in a science which will be very like modern physics, though not at all like the materialistic billiard-ball physics of the past. A great deal of work will be required to show in detail how the data of sense are to be fitted into physics. What Dr. Whitehead and I have done so far is only a small part of the necessary work, which probably neither he nor I will be able to complete. I only want, as yet, to recommend the general point of view as a possible one, which deserves to be worked out.

I will end with a discussion of some special points raised by Mr. Strong. On page 309, he objects to the theory that, at a place from which a number of objects are visible, perspectives of all of them exist, on the ground that there is need of a lens to separate out the different light-rays. He admits, however, that "the perspectives can by analysis be separated out," and that is quite enough for me. You cannot by analysis separate out what was not there. He argues that, apart from something like a lens, "what exists there is only a synthesis of effects, and not anything like the stars from which the rays proceeded". Up to a point, this is true, and it accounts for the vagueness of perception. I emphasised this point in *Analysis of Mind*, pp. 135-136.

Space comes next. "What first strikes one in this theory is the curious reversal of the spatial position of objects which it seems to involve—objects being apparently everywhere except in the place where we see and feel them" (Strong, p. 310). As I explained in my book on the *External World* (which, however, laid too little stress on relativity), we have to start with a private space-time for each percipient, and generally for each piece of matter. The correlation of these with the constructed public space-time is a long piece of work, but obviously feasible. The "place where a particular is" is ambiguous; it may mean the place where it is in its perspective, or the place where it is in the system which is the physical thing of which it is a member, or the place where this thing is in public space-time. When these distinctions are borne in mind, Mr. Strong's paradox disappears. The quotation from Einstein with which I began shows that my view is in harmony with modern physics.

Mr. Strong next objects to the similar paradox as regards time, namely that the moment in public time to which an occurrence is to be assigned is earlier than the various moments in public time to which are to be assigned the particulars which are members (or "appearances") of the occurrence at various places. The answer here is essentially the same as before. Public time is a convention, which may be fixed in many equally legitimate ways; it may happen that A is before B according to one legitimate convention, while according to another B is before A. All this is already in the special theory of relativity. One need not therefore treat the time-order of events with any undue respect.

One more point: "We may draw one inference from these paradoxes, and that is that the object, as physical science conceives it, is not correctly defined as the *system* of all the perspectives (even of the 'regular' ones, *i.e.*, those undistorted by the interviewing medium), but is rather their mathematical limit" (Strong, p. 311). I myself suggested this view in my book on the *External World*, but rejected it for the reason that there is no limit to which the appearances approach. For this reason, in *Analysis of Mind* (pp. 106-107), I defined a piece of matter as that set of appearances to which the set approximates which consists of a

given appearance together with all those others which would exist if the given appearance were regular. This is a limiting *set*, not a limiting single appearance, and it exists when the limiting single appearance does not. The device is essentially the same as that of defining an irrational number as a certain class of rationals. It must be understood that a "piece of matter" is not anything real, but merely some constructed object having properties which enable us to state shortly facts or laws concerning a whole set of particulars that are real. In defining a piece of matter, therefore, we are to be guided solely by convenience.

There are other points in Mr. Strong's article which I should like to deal with, but I think what I should have to say about them can be inferred by the reader from what I have already said. I will, therefore, close this discussion, which is already long enough, by expressing gratitude to Mr. Strong for having brought into prominence so many important points.

BERTRAND RUSSELL.