Albert Einstein

REPLY TO CRITICISMS

REMARKS CONCERNING THE ESSAYS BROUGHT TOGETHER IN THIS CO-OPERATIVE VOLUME*

BY WAY of introduction I must remark that it was not easy for me to do justice to the task of expressing myself concerning the essays contained in this volume. The reason lies in the fact that the essays refer to entirely too many subjects, which, at the present state of our knowledge, are only loosely connected with each other. I first attempted to discuss the essays individually. However, I abandoned this procedure because nothing even approximately homogeneous resulted, so that the reading of it could hardly have been either useful or enjoyable. I finally decided, therefore, to order these remarks, as far as possible, according to topical considerations.

Furthermore, after some vain efforts, I discovered that the mentality which underlies a few of the essays differs so radically from my own, that I am incapable of saying anything useful about them. This is not to be interpreted that I regard those essays—insofar as their content is at all meaningful to me—less highly than I do those which lie closer to my own ways of thinking, to which [latter] I dedicate the following remarks.

To begin with I refer to the essays of Wolfgang Pauli and Max Born. They describe the content of my work concerning quanta and statistics in general in their inner consistency and in their participation in the evolution of physics during the last half century. It is meritorious that they have done this: For only those who have successfully wrestled with the problematic situations of their own age can have a deep insight into those situations; unlike the later historian, who finds it difficult to make abstractions from those concepts and views which appear to his generation as established, or even as self-evident. Both authors

* Translated (from the German typescript) by Paul Arthur Schilpp.

deprecate the fact that I reject the basic idea of contemporary statistical quantum theory, insofar as I do not believe that this fundamental concept will provide a useful basis for the whole of physics. More of this later.

I now come to what is probably the most interesting subject which absolutely must be discussed in connection with the detailed arguments of my highly esteemed colleagues Born, Pauli, Heitler, Bohr, and Margenau. They are all firmly convinced that the riddle of the double nature of all corpuscles (corpuscular and undulatory character) has in essence found its final solution in the statistical quantum theory. On the strength of the successes of this theory they consider it proved that a theoretically complete description of a system can, in essence, involve only statistical assertions concerning the measurable quantities of this system. They are apparently all of the opinion that Heisenberg's indeterminacy-relation (the correctness of which is, from my own point of view, rightfully regarded as finally demonstrated) is essentially prejudicial in favor of the character of all thinkable reasonable physical theories in the mentioned sense. In what follows I wish to adduce reasons which keep me from falling in line with the opinion of almost all contemporary theoretical physicists. I am, in fact, firmly convinced that the essentially statistical character of contemporary quantum theory is solely to be ascribed to the fact that this [theory] operates with an incomplete description of physical systems.

Above all, however, the reader should be convinced that I fully recognize the very important progress which the statistical quantum theory has brought to theoretical physics. In the field of *mechanical* problems—i.e., wherever it is possible to consider the interaction of structures and of their parts with sufficient accuracy by postulating a potential energy between material points—[this theory] even now presents a system which, in its closed character, correctly describes the empirical relations between statable phenomena as they were theoretically to be expected. This theory is until now the only one which unites the corpuscular and undulatory dual character of matter in a logically satisfactory fashion; and the (testable) relations, which are contained in it, are, within the natural limits fixed by the indeterminacy-relation, *complete*. The formal relations which are given in this theory—i.e., its entire mathematical formalism—will probably have to be contained, in the form of logical inferences, in every useful future theory.

What does not satisfy me in that theory, from the standpoint of principle, is its attitude towards that which appears to me to be the programmatic aim of all physics: the complete description of any (individual) real situation (as it supposedly exists irrespective of any act of observation or substantiation). Whenever the positivistically inclined modern physicist hears such a formulation his reaction is that of a pitying smile. He says to himself: "there we have the naked formulation of a metaphysical prejudice, empty of content, a prejudice, moreover, the conquest of which constitutes the major epistemological achievement of physicists within the last quarter-century. Has any man ever perceived a 'real physical situation'? How is it possible that a reasonable person could today still believe that he can refute our essential knowledge and understanding by drawing up such a bloodless ghost?" Patience! The above laconic characterization was not meant to convince anyone; it was merely to indicate the point of view around which the following elementary considerations freely group themselves. In doing this I shall proceed as follows: I shall first of all show in simple special cases what seems essential to me, and then I shall make a few remarks about some more general ideas which are involved.

We consider as a physical system, in the first instance, a radioactive atom of definite average decay time, which is practically exactly localized at a point of the co-ordinate system. The radioactive process consists in the emission of a (comparatively light) particle. For the sake of simplicity we neglect the motion of the residual atom after the disintegration-process. Then it is possible for us, following Gamow, to replace the rest of the atom by a space of atomic order of magnitude, surrounded by a closed potential energy barrier which, at a time $t = 0$, encloses the particle to be emitted. The radioactive process thus schematized is then, as is well known, to be described—in the sense of elementary quantum mechanics—by a ψ-function in three dimensions, which at the time $t = 0$ is different from zero only inside of the barrier, but which, for positive times, expands into the outer space. This ψ-function yields the probability that the particle, at some chosen instant, is actually in a chosen part of space (i.e., is actually found there by a measurement of position). On the other hand, the ψ-function does not imply any assertion *concerning the time instant of the disintegration* of the radioactive atom.

Now we raise the question: Can this theoretical description be taken as the *complete* description of the disintegration of a single individual atom? The immediately plausible answer is: No. For one is, first of all, inclined to assume that the individual atom decays at a definite time; however, such a definite time-value is not implied in the description by the ψ-function. If, therefore, the individual atom has a definite disintegration-time, then as regards the individual atom its description by means of the ψ-function must be interpreted as an incomplete description. In this case the ψ-function is to be taken as the description, not of a singular system, but of an ideal ensemble of systems. In this case one is driven to the conviction that a complete description of a single system should, after all, be possible; but for such complete description there is no room in the conceptual world of statistical quantum theory.

To this the quantum theorist will reply: This consideration stands and falls with the assertion that there actually is such a thing as a definite time of disintegration of the individual atom (an instant of time existing independently of any observation). But this assertion is, from my point of view, not merely arbitrary but actually meaningless. The assertion of the existence of a definite time-instant for the disintegration makes sense only if I can in principle determine this time-instant empirically. Such an assertion, however, (which, finally, leads to the attempt to prove the existence of the particle outside of the force barrier), involves a definite disturbance of the system in which we are interested; so that the result of the determination does not permit a conclusion concerning the status of the undisturbed system. The supposition, therefore, that a radioactive atom has a definite disintegration-time is not justified by anything whatsoever; it is, therefore, not demonstrated either that the ψ-function can not be conceived as a complete description of the individual system. The entire alleged difficulty proceeds from the fact that one postulates something not observable as "real." (This the answer of the quantum theorist.)

What I dislike in this kind of argumentation is the basic positivistic attitude, which from my point of view is untenable, and which seems to me to come to the same thing as Berkeley's principle, *esse est percipi*. "Being" is always something which is mentally constructed by us, that is, something which we freely posit (in the logical sense). The justification of such constructs does not lie in their derivation from what is given by the senses. Such a type of derivation (in the sense of logical deducibility) is nowhere to be had, not even in the domain of pre-scientific thinking. The justification of the constructs, which represent "reality" for us, lies alone in their quality of making intelligible what is sensorily given (the vague character of this expression is here forced upon me by my striving for brevity). Applied to the specifically chosen example this consideration tells us the following:

One may not merely ask: "Does a definite time instant for the transformation of a single atom exist?" but rather: "Is it, within the framework of our theoretical total construction, reasonable to posit the existence of a definite point of time for the transformation of a single atom?" One may not even ask what this assertion *means*. One can only ask whether such a proposition, within the framework of the chosen conceptual system—with a view to its ability to grasp theoretically what is empirically given—is reasonable or not.

Insofar, then, as a quantum-theoretician takes the position that the description by means of a ψ-function refers only to an ideal systematic totality but in no wise to the individual system, he may calmly assume a definite point of time for the transformation. But, if he represents the assumption that his description by way of the ψ-function is to be taken as the *complete* description of the individual system, then he must reject the postulation of a specific decay-time. He can justifiably point to the fact that a determination of the instant of disintegration is not possible on an isolated system, but would require disturbances of such a character that they must not be neglected in the critical examination of the situation. It would, for example, not be possible to conclude from the empirical statement that the transformation has already taken place, that this would have been the case if the disturbances of the system had not taken place.

As far as I know, it was E. Schrödinger who first called attention to a modification of this consideration, which shows an interpretation of this type to be impracticable. Rather than considering a system which comprises only a radioactive atom (and its process of transformation), one considers a system which includes also the means for ascertaining the radioactive transformation—for example, a Geiger-counter with automatic registration-mechanism. Let this latter include a registration-strip, moved by a clockwork, upon which a mark is made by tripping the counter. True, from the point of view of quantum mechanics this total system is very complex and its configuration space is of very high dimension. But there is in principle no objection to treating this entire system from the standpoint of quantum mechanics. Here too the theory determines the probability of each configuration of all its co-ordinates for every time instant. If one considers all configurations of the coordinates, for a time large compared with the average decay-time of the radioactive atom, there will be (at most) *one* such registration-mark on the paper strip. To each co-ordinate-configuration corresponds a definite position of the mark on the paper strip. But, inasmuch as the theory yields only the relative probability of the thinkable co-ordinate-configurations, it also offers only relative probabilities for the positions of the mark on the paperstrip, but no definite location for this mark.

In this consideration the location of the mark on the strip plays the rôle played in the original consideration by the time of the disintegration. The reason for the introduction of the system supplemented by the registration-mechanism lies in the following. The location of the mark on the registration-strip is a fact which belongs entirely within the sphere of macroscopic concepts, in contradistinction to the instant of disintegration of a single atom. If we attempt [to work with] the interpretation that the quantum-theoretical description is to be understood as a complete description of the individual system, we are forced to the interpretation that the location of the mark on the strip is nothing which belongs to the system *per se*, but that the existence of that location is essentially dependent upon the carrying out of an observation made on the registration-strip. Such an interpretation is certainly by no means absurd from a purely logical standpoint; yet there is hardly likely to be anyone who would be inclined to consider it seriously. For, in the macroscopic sphere it simply is considered certain that one must adhere to the program of a realistic description in space and time; whereas in the sphere of microscopic situations one is more readily inclined to give up, or at least to modify, this program.

This discussion was only to bring out the following. One arrives at very implausible theoretical conceptions, if one attempts to maintain the thesis that the statistical quantum theory is in principle capable of producing a complete description of an individual physical system. On the other hand, those difficulties of theoretical interpretation disappear, if one views the quantum-mechanical description as the description of ensembles of systems.

I reached this conclusion as the result of quite different types of considerations. I am convinced that everyone who will take the trouble to carry through such reflections conscientiously will find himself finally driven to this interpretation of quantum-theoretical description (the ψ-function is to be understood as the description not of a single system but of an ensemble of systems).

Roughly stated the conclusion is this: Within the framework of statistical quantum theory there is no such thing as a complete description of the individual system. More cautiously it might be put as follows: The attempt to conceive the quantum-theoretical description as the complete description of the individual systems leads to unnatural theoretical interpretations, which become immediately unnecessary if one accepts the interpretation that the description refers to ensembles of systems and not to individual systems. In that case the whole "egg-walking" performed in order to avoid the "physically real" becomes superfluous. There exists, however, a simple psychological reason for the fact that this most nearly obvious interpretation is being shunned. For if the statistical quantum theory does not pretend to describe the individual system (and its development in time) completely, it appears unavoidable to look elsewhere for a complete description of the individual system; in doing so it would be clear from the very beginning that the elements of such a description are not contained within the conceptual scheme of the statistical quantum theory. With this one would admit that, in principle, this scheme could not serve as the basis of theoretical physics. Assuming the success of efforts to accomplish a complete physical description, the statistical quantum theory would, within the framework of future physics, take an approximately analogous position to the statistical mechanics within the framework of classical mechanics. I am rather firmly convinced that the development of theoretical physics will be of this type; but the path will be lengthy and difficult.

I now imagine a quantum theoretician who may even admit that the quantum-theoretical description refers to ensembles of systems and not to individual systems, but who, nevertheless, clings to the idea that the type of description of the statistical quantum theory will, in its essential features, be retained in the future. He may argue as follows: True, I admit that the quantum-theoretical description is an incomplete description of the individual system. I even admit that a complete theoretical description is, in principle, thinkable. But I consider it proven that the search for such a complete description would be aimless. For the lawfulness of nature is thus constituted that the laws can be completely and suitably formulated within the framework of our incomplete description.

To this I can only reply as follows: Your point of view—taken as theoretical possibility—is incontestable. For me, however, the expectation that the adequate formulation of the universal laws involves the use of *all* conceptual elements which are necessary for a complete description, is more natural. It is furthermore not at all surprising that, by using an incomplete description, (in the main) only statistical statements can be obtained out of such description. If it should be possible to move forward to a complete description, it is likely that the laws would represent relations among all the conceptual elements of this description which, *per se*, have nothing to do with statistics.

A few more remarks of a general nature concerning concepts and [also] concerning the insinuation that a concept—for example that of the real—is something metaphysical (and therefore to be rejected). A basic conceptual distinction, which is a necessary prerequisite of scientific and pre-scientific thinking, is the distinction between "sense-impressions" (and the recollection of such) on the one hand and mere ideas on the other. There is no such thing as a conceptual definition of this distinction (aside from circular definitions, i.e., of such as make a hidden use of the object to be defined). Nor can it be maintained that at the base of this distinction there is a type of evidence, such as underlies, for example, the distinction between red and blue. Yet, one needs this distinction in order to be able to overcome solipsism. Solution: we shall make use of this distinction unconcerned with the reproach that, in doing so, we are guilty of the metaphysical "original sin." We regard the distinction as a category which we use in order that we might the better find our way in the world of immediate sensations. The "sense" and the justification of this distinction lies simply in this achievement. But this is only a first step. We represent the sense-impressions as conditioned by an "objective" and by a "subjective" factor. For this conceptual distinction there also is no logical-philosophical justification. But if we reject it, we cannot escape solipsism. It is also the presupposition of every kind of physical thinking. Here too, the only justification lies in its usefulness. We are here concerned with "categories" or schemes of thought, the selection of which is, in principle, entirely open to us and whose qualification can only be judged by the degree to which its use contributes to making the totality of the contents of consciousness "intelligible." The above mentioned "objective factor" is the totality of such concepts and conceptual relations as are thought of as independent of experience, viz., of perceptions. So long as we move within the thus programmatically fixed sphere of thought we are thinking physically. Insofar as physical thinking justifies itself, in the more than once indicated sense, by its ability to grasp experiences intellectually, we regard it as "knowledge of the real."

After what has been said, the "real" in physics is to be taken as a type of program, to which we are, however, not forced to cling *a priori*. No one is likely to be inclined to attempt to give up this program within the realm of the "macroscopic" (location of the mark on the paperstrip "real"). But the "macroscopic" and the "microscopic" are so inter-related that it appears impracticable to give up this program in the "microscopic" alone. Nor can I see any occasion anywhere within the observable facts of the quantum-field for doing so, unless, indeed, one clings *a priori* to the thesis that the description of nature by the statistical scheme of quantum-mechanics is final.

The theoretical attitude here advocated is distinct from that of Kant only by the fact that we do not conceive of the "categories" as unalterable (conditioned by the nature of the understanding) but as (in the logical sense) free conventions. They appear to be *a priori* only insofar as thinking without the positing of categories and of concepts in general would be as impossible as is breathing in a vacuum.

From these meager remarks one will see that to me it must seem a mistake to permit theoretical description to be directly dependent upon acts of empirical assertions, as it seems to me to be intended [for example] in Bohr's principle of complementarity, the sharp formulation of which, moreover, I have been unable to achieve despite much effort which I have expended on it. From my point of view [such] statements or measurements can occur only as special instances, viz., parts, of physical description, to which I cannot ascribe any exceptional position above the rest.

The above mentioned essays by Bohr and Pauli contain a historical appreciation of my efforts in the area of physical statistics and quanta and, in addition, an accusation which is brought forward in the friendliest of fashion. In briefest formulation this latter runs as follows: "Rigid adherence to classical theory." This accusation demands either a defense or the confession of guilt. The one or the other is, however, being rendered much more difficult because it is by no means immediately clear what is meant by "classical theory." Newton's theory deserves the name of a classical theory. It has nevertheless been abandoned since Maxwell and Hertz have shown that the idea of forces at a distance has to be relinquished and that one cannot manage without the idea of continuous "fields." The opinion that continuous fields are to be viewed as the only acceptable basic concepts, which must also [be assumed to] underlie the theory of the material particles, soon won out. Now this conception became, so to speak, "classical;" but a proper, and in principle complete, *theory* has not grown out of it. Maxwell's theory of the electric field remained a torso, because it was unable to set up laws for the behavior of electric density, without which there can, of course, be no such thing as an electro-magnetic field. Analogously the general theory of relativity furnished then a field theory of gravitation, but no theory of the field-creating masses. (These remarks presuppose it as self-evident that a field-theory may not contain any singularities, i.e., any positions or parts in space in which the field-laws are not valid.)

Consequently there is, strictly speaking, today no such thing as a classical field-theory; one can, therefore, also not rigidly adhere to it. Nevertheless, field-theory does exist as a program: "Continuous functions in the four-dimensional [continuum] as basic concepts of the theory." Rigid adherence to this program can rightfully be asserted of me. The deeper ground for this lies in the following: The theory of gravitation showed me that the non-linearity of these equations results in the fact that this theory yields interactions among structures (localized things) at all. But the theoretical search for non-linear equations is hopeless (because of too great variety of possibilities), if one does not use the general principle of relativity (invarivelocity of a particle or its x-co-ordinates) to the individual (not eliminable) magnitudes. In this case, which has always existed in physics, we have to limt ourselves to ascribing objective meaning to the general laws of the theory, i.e., we have to demand that these laws are valid for every description of the system which is recognized as justified by the group. It is, therefore, not true that "objectivity" presupposes a group-characteristic, but that the group-characteristic forces a refinement of the concept of objectivity. The positing of group characteristics is heuristically so important for theory, because this characteristic always considerably limits the variety of the mathematically meaningful laws.

Now there follows a claim that the group-characteristics determine that the laws must have the form of differential equations; I can not at all see this. Then Margenau insists that the laws expressed by way of the differential equations (especially the partial ones) are "least specific." Upon what does he base this contention? If they could be proved to be correct, it is true that the attempt to ground physics upon differential equations would then turn out to be hopeless. We are, however, far from being able to judge whether differential laws of the type to be considered have any solutions at all which are everywhere singularity-free; and, if so, whether there are too many such solutions.

And now just a remark concerning the discussions about the Einstein-Podolski-Rosen Paradox. I do not think that Margenau's defense of the "orthodox" ("orthodox" refers to the thesis that the ψ-function characterizes the individual system *exhaustively*) quantum position hits the essential [aspects]. Of the "orthodox" quantum theoreticians whose position I know, Niels Bohr's seems to me to come nearest to doing justice to the problem. Translated into my own way of putting it, he argues as follows:

If the partial systems A and B form a total system which is described by its ψ-function ψ/(AB), there is no reason why any mutually independent existence (state of reality) should be ascribed to the partial systems A and B viewed separately, ***not even if the partial systems are spatially separated from each*** *other at the particular time under consideration.* The assertion that, in this latter case, the real situation of *B* could not be (directly) influenced by any measurement taken on *A* is, therefore, within the framework of quantum theory, unfounded and (as the paradox shows) unacceptable.

By this way of looking at the matter it becomes evident that the paradox forces us to relinquish one of the following two assertions:

(1) the description by means of the ψ-function is *complete*
(2) the real states of spatially separated objects are independent of each other.

On the other hand, it is possible to adhere to (2), if one regards the ψ-function as the description of a (statistical) ensemble of systems (and therefore relinquishes (1)). However, this view blasts the framework of the "orthodox quantum theory."

One more remark to Margenau's Sec. 7. In the characterization of quantum mechanics the brief little sentence will be found: "on the classical level it corresponds to ordinary dynamics." This is entirely correct—*cum grano salis;* and it is precisely this *granum salis* which is significant for the question of interpretation.

If our concern is with macroscopic masses (billiard balls or stars), we are operating with very short de Broglie-waves, which are determinative for the behavior of the center of gravity of such masses. This is the reason why it is possible to arrange the quantum-theoretical description for a reasonable time in such a manner that for the macroscopic way of viewing things, it becomes sufficiently precise in position as well as in momentum. It is true also that this sharpness remains for a long time and that the quasi-points thus represented behave just like the mass-points of classical mechanics. However, the theory shows also that, after a sufficiently long time, the point-like character of the ψ-function is completely lost to the center of gravity-co-ordinates, so that one can no longer speak of any quasi-localisation of the centers of gravity. The picture then becomes, for example in the case of a single macro-mass-point, quite similar to that involved in a single free electron.

If now, in accordance with the orthodox position, I view the ψ-function as the complete description of a real matter of fact for the individual case, I cannot but consider the essentially unlimited lack of sharpness of the position of the (macroscopic) body as *real*. On the other hand, however, we know that, by illuminating the body by means of a lantern at rest against the system of co-ordinates, we get a (macroscopically judged) sharp determination of position. In order to comprehend this I must assume that that sharply defined position is determined not merely by the real situation of the observed body, but also by the act of illumination. This is again a paradox (similar to the mark on the paperstrip in the above mentioned example). The spook disappears only if one relinquishes the orthodox standpoint, according to which the ψ-function is accepted as a complete description of the single system.

It may appear as if all such considerations were just superfluous learned hairsplitting, which have nothing to do with physics proper. However, it depends precisely upon such considerations in which direction one believes one must look for the future conceptual basis of physics.

I close these expositions, which have grown rather lengthy, concerning the interpretation of quantum theory with the reproduction of a brief conversation which I had with an important theoretical physicist. He: "I am inclined to believe in telepathy." I: "This has probably more to do with physics than with psychology." He: "Yes." —

The essays by Lenzen and Northrop both aim to treat my occasional utterances of epistemological content systematically. From those utterances Lenzen constructs a synoptic total picture, in which what is missing in the utterances is carefully and with delicacy of feeling supplied. Everything said therein appears to me convincing and correct. Northrop uses these utterances as point of departure for a comparative critique of the major epistemological systems. I see in this critique a masterpiece of unbiased thinking and concise discussion, which nowhere permits itself to be diverted from the essential.

The reciprocal relationship of epistemology and science is of noteworthy kind. They are dependent upon each other. Epis-