CAUSALITY AND PSYCHOANALYSIS

A Letter to the Editors of The Psychoanalytic Quarterly

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Sirs:

You have requested me to discuss Dr. Radó's *Die Wege der Naturforschung im Lichte der Psychoanalyse* [1] from the standpoint of a representative of the exact sciences, the occasion being its publication in an English translation, in the pages of your QUARTERLY. Of course, I shall have nothing to say on the psychoanalytic content of the article, which in this age of extreme specialization correlates with extraordinary versatility fields as remote from each other as theoretical physics and psychoanalysis. As to the emotional attitude of the defenders of the traditional concept of causality, I may quote the physicist Heisenberg, who states that "the violence of the arguments regarding the law of causality often gave one the impression that the problem under discussion was not a scientific but a religious subject".[2] Moreover, even Planck, whose quantum theory has, in fact, undermined the system of classical—that is, strictly causal—physics, supports the a priori causality principle in the Kantian sense, contending that it alone can be regarded as "satisfying".[3]

Dr. Radó's disclosure of the psychoanalytic root (not the only root, of course) of the concept of causality is probably a lasting contribution, unaffected by the advance which has taken place in the field of physics since 1922, the year in which the article was first published in German. However, this advance, which

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[1] First appeared in Imago VIII:401, 1922. (English trans., this QUARTERLY, I, 4, 1932.)

[2] Erkenntnis II:172, 1931.

[3] *The Universe in the Light of Modern Physics.* New York, 1931, p. 88.

in fact progressed most rapidly after that year, brought about new developments (some of them entirely unexpected) in two directions, on which, I think, psychoanalysis should now take a stand. I have in mind, first, the general problem of determinism, regarding which Dr. Radó had merely said, that it was certainly a subject of no slight importance to psychoanalysis, and had left it at that, secondly, an entirely new point of view introduced into theoretical physics, which I might term "the principle of interference with the phenomenon by the act of observation, if both are of the same order of magnitude"—a point of view which seems to me to be of fundamental importance to psychoanalysis. This will be clarified in the course of my argument.

1.

To outline the attitude of physics to the problem of "causality and statistics", it would be well to start with Dr. Radó's example of the Galton frame. The career of one of the discs, which begins its life-course from some compartment, always under the pull of gravity, bouncing against the various pegs, and repelled by them, falling through the gaps between them, finally ending its course somewhere on the bottom, can be very well determined by the laws of strictly causal Newtonian mechanics—hardly less so than the orbit of a comet traveling in space through gaps between heavenly bodies—gaps, to be sure, so immense that collisions hardly ever occur. The important question is whether we are interested in the career of *any one* particular disc. If that be the case, we must naturally make an effort to determine its exact form and to map out the topography of the entire universe of pegs through which it will have to travel, just as in computing the orbit of a comet we must have an accurate chart of the part of the heavens through which it passes. Finally, we must also know, at some definite point of time, the position as well as the instantaneous momentum of the disc; [4] then we can compute

[4] This is expressed in a somewhat simplified form, but it does not matter.

its entire history (or career) from its "birth" at the top of the frame until its "death" at the bottom with the certainty and accuracy of solar eclipse calculations. . . . This, the statistical method cannot do, as Radó, too, has pointed out (p. 684). The statistical method cannot tell us what will happen to the *one* disc (which I shall assume painted red for the sake of identification) and where it will end its career; and in this respect the statistical method is actually less efficient than the causal. Of course there is the difficulty of providing "the necessary mass of data" and of mastering "endless calculations" (Radó, p. 685). But, this difficulty is one of practice, not of principle. Hosts of scientific workers throughout the world are ready to provide the "necessary mass of data" and the "endless calculations" of astronomy. We need only suppose that the Nizam of Hyderabad becomes suddenly convinced that his fate depends upon the course of that red disc on an endless Galton frame (and just such motives procured for the young science of astronomy the financial backing of the medieval princes of Europe) to recognize that the practical difficulty is in fact a very common one—a simple question of money and work.

This point, which in Dr. Radó's presentation might have led to some misunderstanding, had to be clarified. If the career of the individual disc does not interest us, and if we are interested only in the ultimate distribution of a large number of discs, then we rise one rung higher in the ladder of magnitudes and in this mass phenomenon the individual object becomes merely an unidentifiable statistical unit.

Classical physics—and this is all physics preceding the quantum physics, which could not have been considered in Dr. Radó's article—knew therefore two kinds of laws, the causal for individual phenomena and the statistical for mass phenomena. But these two kinds of laws were in no way opposed to each other. In the same way, the census statistician, while stating certain laws regarding the average behavior of the population, does not doubt for a moment that each *indi-*

vidual case is causally determined, and absolutely so, though it slips by the law of the average.[5] Similarly physicists were convinced that investigation had shown "that if we assume for each occurrence [6] the validity of dynamic laws and therefore of strict causality, there will result just those laws of probability which observations have established".[7] In a classical system, therefore, it was possible for the statistical methods of correlation psychology to exist side by side with the strictly causal method of psychoanalysis; it was possible to regard the laws of the former as theoretically deducible from the findings of the latter. The method of treatment of the individual was likewise resolved into the very practical consideration of work and remuneration.

This situation has been changed fundamentally.

2.

Before I describe this changed situation, it will be necessary to answer a question which my readers may have wished raised earlier.

The question is: Just how can the causal method make rigorously accurate assertions about the course of events? And, on what fact of reality is it based?

This problem is complicated. I can treat of it here only superficially, yet treat of it I must, in order to clarify what will follow.

The method of strict causality is no other than the one which we employ in the general process of thinking: it is largely the making of *concepts*. But the "rigor" of the method requires that the concepts should be defined with equal rigor. The rigorous laws of science are nothing else than "implicit" definitions of such rigorous concepts. From the laws we derive

[5] Compare, e. g., Hugo Bergmann, *Der Kampf um das Kausalitätsgesetz in der jüngsten Physik.* Braunschweig, 1929, p. 30.

[6] In molecular dynamics, M. R.

[7] Planck: *Kausalgesetz und Willensfreiheit.* Berlin, 1923, p. 33.

logical deductions, that is, we construct, according to an expression by L. Wittgenstein,[8] "tautological transformations" of the propositions, and these deductions are the strictly valid assertions mentioned above.

An example from the field, familiar to grammar school pupils, will clarify this. We all remember the impressive structure of Euclidean geometry. From time immemorial this was the *model of rigorous theory,* whose propositions laid claim to infallibility. Yet, this was so only because these propositions are tautological transformations of the axioms, with the aid of which the fundamental concepts of geometry were no more than defined. Thus, the axiom that only one straight line can be drawn between two points is nothing else than an implicit definition of the concept "straight line". Another Euclidean axiom (the eleventh) defines the concept of a parallel. From these two axioms follows the necessary conclusion that the sum of the angles of a triangle is equal to 180 degrees. But all this has nothing to do with reality. In reality, the straightest of straight edges will not give us a Euclidean straight line, for it is possible to draw many "straight lines" between two points with this straight edge. Therefore the sum of the angles of a real triangle is not 180 degrees but more or less than 180 degrees. This method, the axiomatic method, can be applied to any arbitrary idea whatsoever with equal rigor. It is possible, for example, to start out with a rigorous definition of the concept "Devil" and then proceed to derive the Devil's characteristics, no matter whether the Devil exists or not.

But if such a theory is to have scientific significance a connection with reality must be established. This is effected by referring to really existing things which correspond to the concepts. Then it can be said that inasmuch as there are such things as (approximately) straight lines and (approximate) parallels, the sum of the angles of a triangle is (approximately) 180 degrees.

[8] *Tractatus logico-philosophicus.* London, 1922.

Such a method must appear to the reader as distinctly artificial, and so it is. Allowing ourselves a variation on an expression of Einstein's, it is possible to say that the laws of the exact sciences are exact, only in so far as they do not refer to reality; and that in so far as they do refer to reality they are not exact. But if you believe that the calculus of probabilities, which is the basis of statistical discipline, proceeds by a different method, you will be disillusioned by the philosophical chapters in v. Mises' textbook.[9] For the mathematical science of mass phenomena, the calculus of probabilities, must also derive its deductions from axioms and must start its calculations with certain given "initial probabilities" [10] which likewise cannot be gathered empirically. Then if the theoretical result be compared with actual experience, the degree of agreement between the two can only indicate how far the real thing for which the calculation was carried out corresponds to the assumed pattern with which it started. Thus, the initial probability for the Galton frame is that the chances for the disc's falling to the right or to the left of the peg are equal. The calculation will then result in a distribution of the discs at the bottom in accordance with the Gauss curve.[11] In reality, this distribution will never be observed, analogous to the above-mentioned case of Euclidean geometry; but in so far as the real distribution is Gaussian, there exists in reality an approximation to a Galton frame with an initial probability of 1/2.

Now we shall proceed to answer the second part of the question: what fact of reality makes the method, which by way of greater generalization we call mathematical, a working method? The answer is not difficult: it is that fact which underlies the construction of concepts—namely, the fact that *there are things which are similar*. This platitude and nothing more constitutes the empirical content of experience. The

[9] *Wahrscheinlichkeitsrechnung*. Leipzig und Wien, 1931.
[10] Radó: *op. cit.*
[11] Radó: *op. cit.*

mathematical method is based upon the axiomatic *equating* of *conceptual* objects on the strength of the *resemblance* of objects *in the world of reality.*

At this point I cannot resist the temptation of a digression. It is psychologically interesting that it was a constant aim of scientific speculation to exemplify the axiomatic equating *(Gleichsetzung)* of conceptual objects *in reality*—that is, by viewing the undeniable variety of real objects as products of various combinations of real things, which in themselves are equal in every respect, as for example, equal atoms, or electrons or protons. Of course, there is a preconceived notion in this,—a notion which fled into a realm beyond all experience. After all, it would be more natural to assume that real things, including atoms, are dissimilar, diverse. In H. G. Wells's *First and Last Things* this idea was treated more impressively than anywhere else. Here is a problem worthy of the sagacity of the psychoanalyst. Its solution would at the same time throw light upon the origin of the process of concept making.

Let us return to our main subject. We have acquainted ourselves with the mathematical method. The method of rigorous causality is of the same nature. Now, wherein does it differ from, say, rigorous geometry?

Schopenhauer believed that it was possible to differentiate "four roots of the principle of sufficient reason", four ways of answering the question "Why?" One of these is the logical "reason of knowing": the reason from which the conclusion will logically follow. "Caius is mortal, because all men are mortal." Then, as a disciple of Kant's, he introduced the "principle of sufficient reason of being" as a specific class. This was to be the basis of the geometric perception, which is direct, intuitional and not derived from concepts. This standpoint was shown to be untenable by the work of Hilbert, who at the same time developed the above-mentioned "axiomatic" conception, which reduced the intuitive geometric "reason of being" to a logical "reason of knowing". (p. 118, 18.) The third root is the "sufficient reason of *becoming*"—causality. The causal relation of *becoming* is differentiated, then, from the geometric relation of *being* by the addition of the time-coördinate: cause and effect stand in a time relation to each other, the cause precedes the effect, and the latter follows the former.

3

I have at last reached the point where I can say what constitutes one of the two new points of view in physics as to causality. As early as 1905, Einstein and Minkowski [12] in their special theory of relativity had treated time as a coördinate similar to usual space coördinates. And just as in reality there is no front or back, right or left, above and below—these concepts being relative, or even arbitrary—so there is not in reality "time before" or "time after", although time-relationships do exist. That is to say, just as it is possible to designate the route Bremen-New York as "outgoing" or "incoming", we can similarly lay out the time path Napoleon I-Mussolini, equally well in the direction of "into the future" or "into the past". From the point of view of causal relations, this would mean: either Mussolini as the consequence of Napoleon I or *vice versa* Napoleon I as the cause of Mussolini—in which case future and past, as well as cause and effect, would be just as interchangeable as "incoming" and "outgoing" in the case of Bremen-New York. In fact, physics became aware of this, too; that in all its strictly causal laws the time element is *reversible*. For example, it is possible to photograph a stone in the process of falling to the ground and then let the film run in the opposite direction: in that case, we will see a stone whose motion obeys exactly the laws for an object thrown from the ground upwards; nothing will appear to contradict this.

To be sure, it was believed that there is one law—the second law of thermodynamics—in which time has a very definite

[12] Einstein, A., Ann. d. Physik XVII:891, 1905. Minkowski, H., Physik. Ztschr. X:104, 1909.

direction. Popularly expressed, this law states that in a system left to itself the elements become increasingly disordered. (We may in this instance think of a bachelor's clothes-closet which, let us assume, had been once neatly arranged by his mother.) "Become increasingly" implies and defines a direction from past to future. But, Boltzmann had already shown as early as 1895, that the second law of thermodynamics refers to a mass phenomenon, that is, it depends upon the probabilistic approach of physics.[13] It became clear then that cases in which *entropy* (the measure of disorder) decreases instead of increases not only may, but *must* occur in the long run. If the "increasing time" is to vary directly with "increasing entropy", time itself would have to increase and decrease alternately in the course of time, if one may say so. In connection with the physicist's sharpened awareness of the physical meaning of time, thanks to the theory of relativity, it has been concluded finally that the "concept of direction of time", ("the arrow of time", as Eddington calls it) that is, the idea of an absolute "earlier" and "later" is foreign to physical science.[14]

With this, occurrences in time become fixed; they form, as has been aptly expressed, a "time-landscape". [15] Physics differs from geometry merely by an additional fourth dimension: hence, the difference is not essential. The "sufficient reason of becoming" is identical with the "sufficient reason of being", and both go back to the logical "reason of knowing". It is not true, therefore, that physical reality consits of similar three dimensional objects, which *behave* similarly (which axiomatically has been expressed through strict causality). Physical reality consists of similar four dimensional objects—that is, objects which are similar in all four dimensions—one time dimension and three ordinary space dimensions—and nothing but that!

With this, the concept of causality ceases to exist in physics.

[13] Radó: *op. cit.*

[14] Schrödinger: *Über die Umkehrung der Naturgesetze,* Sitzber. der preuss. Akad. d. Wiss. VIII, IX, 144, 1931.

[15] Bergmann: *l. c.,* p. 61.

4.

This radical conclusion is merely the consummation of a process that was deemed essential for the development of physics. It is the de-anthropomorphization of experience. It has been pronounced with great emphasis by Planck himself, to be the goal of physics.[16] Accordingly, the task of physics is to construct a world from which consciousness is excluded.[17] With this goal, neither course of time, the direction of which is determined *solely* by inner perception, nor its consequence, causality, could survive in physics. Yet all the more both of these concepts find their justification in *psychology*. For, as we have found, they are psychological concepts. This situation has been indicated by Radó,[18] although, as would be expected from an article written in 1922, not very definitely. Peculiarly, the belief prevailed that with the certainty of strict causal relations in physics, the human being was included in the physical scheme and that psychological phenomena were determined by strict causality—an attitude that went so far as to deny the existence of any freedom of will. (It was not unlike a story that went the rounds and finally got back to the very individual from whom it originated.) Then it suddenly became known that an electron jumps from its orbit at its pleasure, so to speak—and people assumed that they could derive from this conclusions regarding causality and freedom of will; nothing less than that! We are confronted here by an independent root, the fourth Schopenhauerian root, of the principle of sufficient reason—"motivation". And this is the autochthonous domain of psychoanalysis, which it may explore freely, unhampered by considerations specific to physics. For, was it not one of the outstanding achievements of psychoanalysis that it found "motives" for occurrences apparently belonging to the domain of physics, as for instance stumbling or some other parapraxis, and thereby annexed them for psychology?

[16] *Physikalische Rundblicke*, p. 1, 1932.
[17] Bergmann: *l. c.*, p. 3.
[18] Radó: *op. cit.*

5.

It is now in order to clarify one conceptual ambiguity which has been responsible for much confusion. It concerns the difference between causality and determinism. The two expressions are often used synonymously.[19] But they are not synonymous. For instance, we can say that one unknown is determined by one mathematical equation, i. e., the value of the unknown can be derived from the equation. But, on the other hand, two unknowns may very well be conditioned by a single equation, though they cannot be derived from it—i. e., they are not determined by the equation. Here there can be no question of causality. Determinism is essential in mathematical method, in geometry, in rigorous physics. This latter we would have now to call strictly deterministic physics, instead of strictly causal. Certain it is that it is no longer strictly *causal;* nevertheless, it could be deterministic, if it were not for an objection which I shall mention later.

An expansion of the "animistic, fetishistic" (anthropomorphic) conception of causality without any implied renunciation of determinism was attempted years ago by Mach, when he substituted functional relation for causality.[20] From the relation $F(xy)=0$, one may obtain a definite value of x if the value of y is given, and *vice versa.* Neither has superior rank. There is no cause and effect relation here, and because of this there is no causality; but there *is* determinism.

The exact reverse of this is probably true in psychology. Causality is certainly inherent in the discipline of psychology. But is determinism? If psychoanalysis reveals the underlying causation—that is, the motive—of a case of stumbling, does it have to maintain that the subject could not have escaped stumbling? What could be the meaning of such an assertion? I do not know. Here lies the problem of determinism in causal psychoanalysis. The question is, does causality suffice in analysis or does analysis have to insist on determinism?

[19] Also by Radó: *op. cit.*
[20] Radó: *op. cit.*

In non-causal physics, the problem is as follows: under what circumstances is a deterministic physics possible? This brings us to the second point of view of modern physics, mentioned in the introduction, which is undoubtedly important in psychoanalysis.

6.

Let us look at the methods of deterministic physics. As has been explained in the first part of the paper, deterministic physics (substituting now "deterministic" for "causal") does not deal with masses but with individual objects, or better, with a limited configuration of individual objects. The configuration must always be theoretically equal (cf. part 2, above), which means that it must be similar in reality. Such a configuration of objects is called in physics a closed system. Now, we put the closed system under observation. Observation is the essential act. Experiment, for example, is observation on an artificial, closed system.

Is there not an inconsistency in this? Is it possible to observe a closed system? Is not every observation an interference? This problem has found its extreme formulation in the famous question put by Lichtenberg,[21] "Do girls blush in the dark?" Clearly, this question cannot be answered by means of observation. The closed system is the girl in the dark, pressed by some ideation. Observation requires light. Hence, it is an interference, altogether destructive to the "closedness" of the system.

The other extreme is the observation of a planet in its course. The planet moves in its orbit, following its autochthonous law, indifferent to observation.

The situation in physics which made Lichtenberg's joke a serious problem is, according to Heisenberg, the following: [22] Like the orbit of a planet, the course of an electron is completely determined if its position in space and its momentum at some definite point of time are known; or, which is the

[21] The German satirical philosopher of the end of the eighteenth century.

[22] Heisenberg, W.: Ztschr. f. Physik XLII, 172. 1927.

same, if its position in space is known at two different points of time (from which its momentum results). To determine these data, the electron must be observed, that is, seen. For this purpose it is illuminated. Light, however, exercises pressure. I am able to determine the exact initial position of the electron by adequate illumination. This, however, pushes the electron out of its way, and it does not arrive at the second position, which it would have reached had I not observed it. I am, therefore, unable to determine its velocity exactly. This means that it is intrinsically impossible to determine the exact career of an electron. The observation involves a far-reaching interference. This interference does not destroy the closedness of the system to the same extent as in Lichtenberg's problem, but it tears down the boundaries of the system and introduces what Heisenberg called the "principle of indeterminancy".

Wherein lies the difference between the observation of the planet and the observation of the electron? The difference is in their respective orders of magnitude. In the first case, the process "movement of planet" is of infinitely higher order of magnitude than the process "observation of a planet with terrestrial instruments". In the second case, the observation requires a collision between the electron and a quantum of light, and the processes "movement of electron" and "collision of electron with quantum of light" are of the same order of magnitude. In the first case, observation does not interfere with the phenomenon observed, while in the second case, the interference makes the process indeterminate.

7.

Now, observation is the basis of *every* scientific method. The difficulty revealed by Heisenberg in the case of quantum physics, therefore, must exist in every scientific endeavor where the phenomenon observed and the act of observation are of the same order of magnitude

This difficulty must, therefore, be met with in psychology where man is observed by man. We now understand the dilemma in Lichtenberg's problem much better. The ideation of being observed is generally of the same order as an ideation that causes one to blush. Of course, it may be possible to approach phenomena in the psychology of the conscious by means of observations of lower order of magnitude. This can, however, never be the case in depth-psychology. Here, the psychologist sees himself confronted with the task of breaking through unconscious resistances. The means he utilizes can, therefore, be only of the same order of magnitude as the resistances themselves.

Should psychoanalysis aim to be more than a psychotherapeutic technique—namely, a scientific method—it will have to answer the following question: What relation is there in psychoanalysis between the phenomena observed and the means of observation? Or, in other words, how far is the psychological system under observation modified through the procedure of psychoanalysis?

Psychoanalysis has been condemned on the grounds that the unconscious reflects, like an echo, what the psychoanalyst has breathed into it. Psychoanalysis, now, can reply that a similar difficulty exists in physics (!) without at all endangering the scientific standards of that discipline. And for this reason, psychoanalysis will be in a better position to assume an unbiased attitude towards this problem.

I should mention here what has been suggested in physics as a way out. In brief the suggestion is: Include the apparatus of observation in the closed system. True, this will bring about a modification of the original system, but it will save the closed system. It is, of course, not necessary for me to translate this suggestion into the language of the psychoanalyst.

Let him have the floor now.