Einstein: the creation of the theory of relativity and some gnosiological lessons

S. G. Suvorov

Usp. Fiz. Nauk **128**, 459–501 (July 1979)

Some characteristic methods that Einstein used to analyze the problems of the electrodynamics of moving bodies are discussed. Einstein rejected the method of "accumulation of hypotheses", reevaluated the results of electrodynamic experiments, generalized them in the form of the relativity principle, interpreted this as a law of nature, and, significantly, made it the point of departure of a new theory—the special theory of relativity. It is shown that Einstein gave great importance to generalizing the results of experiments and the part played by theory, in which nature is regarded as an integral whole; for Einstein, all the variable parameters used in a theory are interconnected and interdependent; this also applies to space and time, which lose their substantiality and, thus, absoluteness. It is pointed out that it was only the realization that the metric expresses the laws of physical interconnection that made possible the formulation of the general theory of relativity. Einstein's negative position with respect to the conventionalism of Poincaré and Reichenbach, in particular the interpretation of geometry as a conventionally chosen method of describing experience independent of physical interconnections, is discussed. In connection with the criticism of conventionalism, consideration is given to descriptions of one and the same experiment that have different forms, and it is shown that some of these descriptions merely represent formal transformations that do not reveal new connections in nature whereas others give the same results only at a definite stage of understanding, their subsequent development however revealing that they correspond to different levels of penetration into the essence of the phenomena.

PACS numbers: 04.20.Cv, 01.60. + q

CONTENTS

INTRODUCTION

In the present paper, I shall discuss the manner in which Einstein's method of investigating electromagnetic phenomena differs from Lorentz's, and also Einstein's relationship to the conventionalist method of Poincaré. We shall also consider the part played by experiments in Einstein's investigations and how his understanding of theory as a unity of the variable parameters contained in the theory led Einstein to the formulation and deep interpretation of the theory of relativity (the special theory) and its consequences.

I believe that this study is necessary, since, despite the continuing growth in the publication of Einstein studies, the subject "Einstein and his Method in Science" is by no means exhausted, and not everything that is published warrants support. But let us turn to the specific theme.

In his Autobiographical Notes written on the occasion of his 70th birthday, Einstein discussed the relationship between experiment and theory. Concerning experiment he said: "The first criterion is obvious: a theory must not contradict experimental data"(Ref. 1, p. 266).[1) This understanding of the need for unity of experiment and theory already provided Einstein with a powerful means for judging the worth of definite physical theories. A theory requires "external justification", and experiments are the criteria in this justification.

But while valuing experiments highly as the criterion of "external justification" of a theory, Einstein simultaneously noted the subtlety of its use. "The point is that frequently, if not always, one can preserve a particular general theoretical foundation provided one adapts it to the facts by means of more or less artificial additional assumptions" (hypotheses).

In these considerations, Einstein clearly reveals the bold idea that "a given general theoretical foundation" need not always be retained.

But how important a role did this problem play in the development of physics, i.e., the problem of whether one should preserve a general theoretical scheme by the introduction of more or less artificial hypotheses? Undoubtedly, it played a part of no small importance: essentially, it was this method that was widely used in the classical pre-Einstein physics. Since Newton's time, physics had a well-known proposition: by virtue of the law of inertia, a body moves uniformly and rectilinearly; if in a particular case a body moves differently, this does not mean that the law of inertia has ceased to operate, but that there is a cause, a force, deflecting the body from its inherent inertial motion;

---

1) *Translators Note.* As far as possible, the original references (as well as the Russian translations) are given in the bibliography, which the reader should consult. Because many of the quotations from Einstein and other authors are not readily accessible, I have had to translate the already translated Russian on numerous occasions.

0038-5670/79/070528-27$01.10

the problem is then to find the force. This method, which is based on the recognition that the original law *continues to hold,* but its action is *suppressed* by factors unknown to us and that these factors must be sought, was characteristic of classical physics. Not only in mechanics but also in more complicated phenomena such a device was frequently used following the discovery of new experimental results contradicting existing ideas: the original idea was itself retained, but a hypothesis was introduced to eliminate the resulting contradiction. This was how the solution to the problem of theoretical comprehension of nature appeared in practice—it was the method of subsequent complexification of the theoretical scheme by the introduction of appropriate new hypotheses.

Einstein himself did not give a concrete example of how this method was used. But it could hardly be wrong to assume that he was aware of a historical example that he had once encountered—the development of the electrodynamics of moving bodies in the work of physicists at the end of the 19th and the beginning of the 20th Century, and most especially in the work of Lorentz. We shall consider this example in more detail, which also casts light on one of the aspects of the relationship between theory and experiment which is of great importance both historically and from the point of view of logic; it will also make it possible to explain more clearly the essence of Einstein's own method.

I. LORENTZ: HIS METHOD OF INVESTIGATION

Fixed Ether as an Absolute Frame of Reference. When Lorentz came to physical investigations, the new and progressive (and by no means recognized by everyone) theory was Maxwell's of the electromagnetic field. Maxwell himself believed that electromagnetic vibrations are particular states of a universal medium, the ether, and he attempted to explain the particular properties of this medium. The idea that there should be a particular medium, the carrier of electromagnetic vibrations, undoubtedly arose under the influence of the successes of hydrodynamics, which had developed brilliantly in the studies of physicists and mathematicians in the first half of the 19th Century: Navier, Cauchy, Poisson, Ostrogradskiĭ, and others. The existence of ether was recognized by virtually all physicists. The only disagreements were about its properties, in particular the question of whether the ether is fixed or dragged along by moving bodies. Lorentz adopted the concept of a fixed ether: it agreed well with the reliably established phenomenon of aberration of the stars (the apparent annual displacement of the stars). Aberration was regarded as a simple consequence of the geometrical composition of two velocities—the "absolute" (i.e., with respect to the fixed ether) velocity of the light arriving from the star, and the "absolute" velocity of the Earth.

The fixed ether and its associated absolute frame of reference, and also an absolute universal time were basic to all of Lorentz's subsequent investigations.

As long as this scheme was based on optical phenomena, it provided a direct explanation only of phenomena such as aberration. But things already became more complicated in the explanation of the Fresnel-Fizeau experiments, in which the velocity of light in moving transparent media was measured. These experiments did not yield a vector addition of the velocities of light and of the medium and revealed the existence of a "coefficient of partial dragging of the ether" by the moving medium, so that the drag velocity had the form $\varphi = v(1 - 1/n^2)$, where v is the velocity of the moving medium, and n is the refractive index of light for this medium. Fresnel's formula was a source of mystery, and it was not clear how one could understand "partial dragging"; nor did the appearance of the refractive index of the medium in the formula receive a satisfactory explanation. Only the development of Maxwellian electrodynamics and its further development by Lorentz made it possible to overcome the difficulties.

After the discovery of elementary charges, it was natural to regard them as the sources of elementary electromagnetic fields. Lorentz associated these fields with Maxwell's macroscopic electromagnetic field. Lorentz regarded the intensity of the macroscopic field as the result of averaging of the corresponding intensities of the elementary fields.

This approach enabled Lorentz to associate the field with the properties of matter, which was a major contribution to the development of electrodynamics.

Lorentz's theory completely preserved the original picture—a fixed ether in which electromagnetic fields were realized. The ether was regarded as an ordinary dielectric, with the difference that for it the permittivity ε and the magnetic permeability μ took limiting values equal to unity. According to Lorentz, the fields excited by the electrons propagate in the same fixed ether, and their equations have the same form as Maxwell's equations for the macroscopic field; they take into account only the charge, convection current, and polarization inherent in matter.

On the basis of Lorentz's electron theory it proved possible to explain a number of important facts—electron magneto-optics, chromatic dispersion, magnetic rotation of the plane of polarization, among others; in particular, it was possible to explain rationally, from the point of view of electrodynamics, the mysterious Fresnel coefficient.

All this strengthened Lorentz's confidence in the correctness of his basic position. This position was subjected to test later when the problem arose of creating the electrodynamics of moving bodies.

The Michelson Experiment. Contraction Hypothesis. New Difficulties. However, on the path to the successful development of Lorentz's ideas there stood an experiment of a new type—Michelson's interferometer experiment. It was expected that this experiment would reveal an ether wind, which would be a direct confirmation of Lorentz's original conception; the result of the experiment showed that there is no ether wind. It seemed that Michelson's experiment refuted the concept of an absolutely fixed ether. Nevertheless, Lorentz retained this concept; he explained Michelson's result *in the spirit of his original premise* (absolute space and time, ether at rest). This was made possible by the adoption of the additional ("contraction") hypothesis, which asserts that all bodies are contracted in the direction of motion in the ratio $k(1-v^2/c^2)^{-1/2}$, where v is the velocity with which the body moves and c is the velocity of light.

The contraction hypothesis explained the absence of the ether wind in the Michelson experiment. But it also led to some physical consequences. The Lorentz contraction amounts to an anisotropic change in the linear dimensions of moving bodies. But the anisotropy in the body should lead to some electromagnetic phenomena, for example, birefringence in transparent bodies and the occurrence of an angular momentum when a charged capacitor is moved. However, the experiments of Rayleigh, Brace, Trouton, and Noble showed that such phenomena are not observed. This situation repeated itself: definite experimental results were expected but they did not materialize.

Lorentz now faced an even more complicated problem: retaining the previous basic assumptions about the absolute frame of reference, and retaining also the contraction hypothesis, as explanation of the negative result of the Michelson experiment, he still had to explain why the electrodynamic phenomena that should occur in moving bodies as a result of the anisotropy were nevertheless *not observed.*

The Theorem of "Corresponding States". The Problem of the Structure of Bodies. The Problem of Time. To solve the new problem, Lorentz ultimately made a new assumption: in bodies—at rest absolutely or moving (with respect to the ether at rest)—there must exist "*corresponding states*". Identical electrodynamic variables in the equations in corresponding states differ by the fact that in a body at rest they are referred to a system at absolute rest (the ether), whereas in a moving body they take a "local" value. But the connection between the variables in the moving body must remain the same as between the absolute values of the variables; in other words, the field equations for corresponding states must be invariant. Only this requirement has the consequence that measurements on a body that is in motion and changes its linear dimensions anisotropically give the same result as on a body at rest, i.e., no new electrodynamic phenomena are observed.

It would be natural to find the connection between the variables in a moving body and the absolute variables, i.e., find the "transformation formulas". But Maxwell's equations contain the partial derivatives of the field intensities ($\mathbf{E},\mathbf{H}$) with respect to not only the coordinates but also the time. This formal circumstance requires a transformation of the time as well, without which it is impossible to achieve invariance of the form of the equations of corresponding states in two systems. Lorentz called the transformed time t' in the moving system the "local" time (Ortzeit).[2]

Thus, in his searches for explanatory hypotheses, Lorentz ultimately arrived at the idea of corresponding states. Considering this idea retrospectively, it seems to us that Lorentz here came closest to the concept of a relativistic physics.

However, Lorentz assumed that the idea of corresponding states was merely a *theorem*, the validity of which still needed to be proved. From Lorentz's point of view, this meant it was necessary to show that in the changed configuration of the moving body there is established an equilibrium of all the existing forces known from classical physics. The entire energy of the scientist was directed toward this. But in following up this approach, Lorentz encountered insuperable difficulties. First, to solve such a problem, it would be necessary to know the structure of the body in detail, the nature of all the forces acting in it, and the laws of their interaction; however, concerning the structure of the electron one could only make hypotheses; even now it remains a subject of investigation.[3] Second, the situation was complicated by a new concept—the "local" time; its introduction was inescapable, since otherwise the whole idea of corresponding states would have been vitiated; but to find a connection between the time and the change in the configuration of a body was impossible, since in classical physics the time was regarded as a variable that does not depend on any physical conditions, i.e., it was assumed to be universal and absolute. Therefore, Lorentz regarded t' as a purely ancillary "mathematical" quantity and always admitted that he did not give significance to this variable, which played "only" the role which enabled it to formulate an invariant expression for the field equations on the transition from one corresponding state to another. Thus, in 1915, Lorentz

---

[2] In the paper of 1904, Lorentz formulated the "theorem of corresponding states" as follows (without naming it): "Suppose that in a system without translational motion there arises a state of motion for which at a definite position the components of the vectors $\mathbf{p}$, $\mathbf{d}$, and $\mathbf{h}$ are definite functions of the time; then in the same system, after it has been set in motion (and, therefore, has been deformed) there arises a state of motion in which at the corresponding place the components of the vectors $\mathbf{p}'$, $\mathbf{d}'$, and $\mathbf{h}'$ are the same functions of the local time" (see Ref. 2). The theorem is named by Lorentz in the book of Ref. 3, in which it is repeated almost literally (see Ch. V, §§162, 174, 175).

[3] At that time, not a few contradictory hypotheses were put forward concerning the structure of electrons; in the light of our present knowledge, their naivety seems obvious. Lorentz clearly recognized the shaky nature of these hypotheses; in lectures on the theory of electrons (1906) published in 1909 and 1915, he expressed himself as follows; "...in my opinion, it would be entirely justified to maintain the hypothesis of deformed electrons if in this way we really could make progress in understanding the phenomena. In theoretical considerations about the structure of these tiny particles we must not forget that there may be many possibilities of a kind that we cannot now imagine; it is very probable that there are other internal forces serving to give the system stability; finally, it is possible that we are on an entirely wrong track when we attempt to apply to individual parts of an electron our ordinary concept of a force" (Ref. 3, p. 312).

noted that he had not succeeded in achieving such simplicity in the theory of electromagnetic phenomena as had Einstein, and he wrote: "The main cause of my failure was that I always adhered to the idea that only the variable t can be taken as the true time and that my local time t' must be regarded as no more than an ancillary mathematical quantity" (Ref. 3, note 72). Lorentz never retracted this admission. About a year before his death, at the conference at Mount Wilson in 1927, he stated[4]:

"A transformation of the time was also necessary. So I introduced the concept of a local time which is different for different systems of reference which are in motion relative to each other. But I never thought that this had anything to do with the real time. This real time for me was still represented by the old classical notion of an absolute time, which is independent of any reference to special frames of reference. There existed for me only this one true time. I considered my time transformation only as a heuristic working hypothesis." (Ref. 13, note 72).

Lorentz on the Authorship of the Theory of Relativity. All this indicates that Lorentz never identified his "theorem of corresponding states" with the relativity principle, but merely regarded it in the spirit of his method as a "heuristic working hypothesis". But the adoption of this hypothesis led to an insuperable contradiction with the original concept of absolute space and time, to which Lorentz adhered to the end of his life. And it is not fortuitous that he accorded Einstein the authorship of the theory of relativity, which remained foreign to him in spirit.

Let us quote Max Born, who was an assistant of Lorentz during his lectures at Göttingen in 1910 and had the possibility of discussion with him. Born asserted: "Lorentz himself never claimed the authorship for the discovery of the relativity principle". Born also notes that in the lectures published by him[7] Lorentz speaks of "Einstein's principle of relativity". This is quite sufficient," concluded Born, "to show that Lorentz himself regarded Einstein's principle of relativity as fundamental. On the same page, and also in subsequent sections there are other commments which demonstrate Lorentz's reluctance to abandon the idea of absolute space and time. When I visited Lorentz a few years before his death, he still maintained a sceptical attitude toward the principle of relativity" (Ref. 8, p. 320, 321).

But there is also the evidence of Lorentz himself: the statement which Lorentz made at the conference at Mount Wilson in 1927 and is given above ends with the words: "*Thus, the theory of relativity is in fact the work of Einstein alone*". This is not simply the modesty of a great scientist, as it is sometimes interpreted; here there are deeper reasons. One gets the impression that Lorentz not only disclaimed the authorship but also did not wish to have the theory of relativity associated with his name.

Conclusions. Characteristic Features of the Method of "Accumulation of Hypotheses". The method of preserving theoretical premises by the introduction of more and more new hypotheses (the method of "accumulation of hypotheses") has the following characteristic features.

At some historical stage, basic theoretical concepts are formed about an investigated object or about a definite group of phenomena. These concepts are not created in a void; they are based on certain experience obtained at this stage of recognition of the phenomenon. In the subsequent development of the science, these theoretical concepts become unshakeable, and are defended as being justified by experience.

However, the class of experiments on which these concepts were based was of necessity restricted. It is natural that it did not include new classes of experiments carried out in the process of further study. The results of the new experiments are taken as being independent, and are examined in the light of the original system of concepts. As a consequence, the new knowledge is simply added to the already accumulated knowledge. If the result of the new experiment contradicts the original theoretical concepts, there is nothing the investigator can do but create hypotheses whose task is to preserve the original system of concepts but eliminate the contradictions and explain their features by an as yet unrecognized structure and explain why the previously formulated laws still operate but are not manifested.

At the gnosiological level, it is important to emphasize that this method of preserving the original concepts of the theory regards the process of acquiring knowledge as an additive process, i.e., as the simple summation of individual results. This understanding of the process of acquisition of knowledge never leads to a concept of the object studied as an integral unity of all its interacting aspects and does not lead to a qualitatively new level of understanding of the object. Theoretical objections to the interpretation of the process of acquiring knowledge as an additive process were already advanced by Hegel, and Lenin[10] supported and developed these objections.[4)] Modern science is coming more and more to the conclusion that it is necessary to grasp an object conceptually in its entirety.

It is not fortuitous that Lorentz, who approached close to the new concept of physical relativity through the idea of corresponding states, but without recognizing the need and justification for this concept, could not comprehend the idea of the equal validity of all times $t, t', t'', \ldots$ in different inertial systems $K, K', K'', \ldots$; more precisely, he could not comprehend the idea that for any inertial system there exists an intrinsic basis *of space-time variables* $x, y, z, t, x', y', z', t', \ldots$, *which* transform together as a collection, and that none of these bases is distinguished by nature from the others.

From the gnosiological point of view, any of these variables is on an equal footing with all the others, and it is therefore inconsistent to regard the time in any particular inertial frame as an "auxiliary mathematical

---

[4)] This question is discussed in more detail in the author's paper Ref. 11 (pp. 567, 568).

quantity" but the coordinates as real.

As we shall see below, Einstein solved the problem of the electrodynamics of moving bodies in ways that differed from Lorentz's.

II. EINSTEIN: THE CREATION OF THE THEORY OF RELATIVITY

Maxwell's Electrodynamics—the Basic Premise. In the investigations that led Einstein to the theory of relativity, Einstein took as basis Maxwell's electrodynamics, to which he gave the correct interpretation, this differing from the one generally adopted. Maxwell's electrodynamics occupied a particular position in physics, and it is here appropriate to recall its main features and some of the stages through which it passed before reaching its final establishment.

As we have already said, a very important circumstance was the fact that Maxwell's electrodynamics was formulated for vacuum in the form of a system of equations connecting the partial derivatives of the field intensities **E** and **H** with respect to the coordinates and the time; this mathematical form expresses the fact that all the variables occurring in the system of equations are *interconnected* and *it is only in this interconnection that the integral nature of the physical process is reflected.* In the following conclusions, this is decisive.

Formulated initially as a theory describing the interconnection of electric and magnetic phenomena which had previously been investigated thoroughly by Faraday, Maxwell's theory in a brief period underwent a huge generalization beyond the narrow field of electromagnetism and penetrated deeply into technology.

Above all, it also encompassed optical phenomena, a "serious justification"[12] for which was the fact that the constant in the system of electrodynamic equations that characterizes the ratio of electrostatic units to electromagnetic units has the dimensions of a velocity, the magnitude of this velocity agreeing, to within the error of the measurements, with the velocity of light. The establishment of the unit of electromagnetic and optical phenomena vastly increased the manifold of experiments in the region of optics and improved their accuracy. The most important consequence of the Maxwellian system of equations was the phenomenon of the propagation of electromagnetic waves, which was already predicted by Maxwell. The wave propagation was confirmed experimentally by Heinrich Hertz; later, it became the basis of radio communication.

The later generalization of Maxwell's electrodynamics by Lorentz related the properties of the field to the properties of matter carrying charge. Like Maxwell's system of equations, the Maxwell–Lorentz equations did not contain any velocities of the bodies, and for certain values of the characteristic parameters the system of Maxwell–Lorentz equations for the vacuum were transformed into the system of Maxwell's equations. The successes of Lorentz's electron theory, which encompassed electromagnetic phenomena in all material media, were naturally presented as a confirmation of Maxwell's electromagnetic theory.

There were, of course, also attempts to formulate the equations of electrodynamics for moving bodies. Following Maxwell's method, Hertz calculated the energy flux through a closed contour displaced in the field with velocity **u**. According to Hertz, the energy flux must depend on this velocity, and additional terms appeared in his equations. For $\mathbf{u}=0$, Hertz's equations were transformed into Maxwell's equations, but for vacuum they did not coincide with Maxwell's equations. Thus, Hertz created a new electrodynamics. However, it was in contradiction with not only optical experiments but also the results of new electromagnetic investigations.

Because of this, Hertz's theory fell by the wayside. So did the method of deriving electromagnetic equations based on the assumption that there is a change in the flux through a closed surface resulting from the *displacement* of a body from one region to another; and so did attempts to solve the problem of the electrodynamics of moving bodies by changing the form of Maxwell's equations for the limiting case of vacuum.

In the last quarter of the 19th Century and at the beginning of the 20th Century numerous investigations were made into different electromagnetic phenomena (the experiments of Röntgen, Rowland, Wilson, Eichenwald, Trouton, Noble, and others). Maxwell–Lorentz electrodynamics was repeatedly confirmed. This result was highly regarded by Einstein. Considering later his historical role, he emphasized the importance of the achievements of the theory in the theoretical description of experiments in the field of electrodynamics and optics, which led to the conclusion that "the bases of this theory must be recognized to be as firmly established as, for example, the equations of classical mechanics. *Nor can one take any other theory that could to any degree rival this theory*" (Ref. 13, p. 386; our italics).

At the beginning of the 20th Century, macroscopic Maxwell–Lorentz electrodynamics was to be regarded as a theory adequate to describe nature and a step in the acquisition of knowledge by man; it was a point of departure, one of the premises for the subsequent deepening of knowledge.

Einstein: New Evaluation of the "Unsuccessful Attempts" to Detect Absolute Motion. Already in the first paper in 1905 Einstein emphasized that a consistent electrodynamics of moving bodies can be *"based on Maxwell's theory for stationary bodies"* (Ref. 13, p. 8 of the Russian translation). He frequently asserted that "the special theory of relativity arose from Maxwell's equations of the electromagnetic field" (Ref. 14, p. 416 of the Russian translation, see also Ref. 13, p. 551 of the Russian translation).

However, Einstein pointed out the need for a correct interpretation of Maxwell's theory, *this differing from the usual interpretation.* He begins the fundamental paper of 1905 with the words: "Das die Elektrodynamik Maxwells—wie dieselbe gegenwärtig aufgefasst zu werden pflegt—in ihrer Anwendung auf bewegte Körper zu Asymmetrien führt, welche den Phänomenen nicht anzuhaften scheinen, ist bekannt". ("It is known that

Maxwell's electrodynamics—*as usually understood at the present time*—when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena.")[5]

What does Einstein mean when he speaks of an interpretation of electrodynamics that leads to asymmetries *which are not inherent in the phenomena*?

He refers to the widespread explanation for the appearance of a current in a closed conductor interacting with a magnet: usually one explantion is given when the "magnet moves", and another when the "conductor moves". Einstein notes the fundamental significance of a simple observation: in both cases, the result—the force and the direction of the current—is found to be the same if the *relative* motion of the conductor and the magnet is the same; we are not confronted by two cases, but one.

This simple fact, confirmed daily in electrotechnology, is coupled by Einstein with the "unsuccessful attempts" (die misslungenen Versuche) to detect a motion of the Earth with respect to the "light medium", which were widely discussed among physicists; although Einstein does not name these attempts, it is clear that they can be assumed to include the Michelson experiment, though not only that experiment but also a number of the other numerous electromagnetic experiments in which, contrary to expectation, no manifestation was found of either a direct influence of "ether wind" or of a distinguished part played by a unique absolute system in which absolute motion is supposed to be realized.

The first and most important service rendered by Einstein to science is that, shaking off the shackles of widely accepted concepts, he examined the failure of all these attempts from new positions; he found that they were not only valid but also sufficient to formulate the most important propositions from which one must depart in the construction of a "consistent theory of the electrodynamics of moving bodies". We are speaking of two principles—the principle of relativity and the principle of the constancy of the velocity of light in vacuum.

Einstein on Principles as a Result of the Generalization of Experimental Facts. Before we consider Einstein's two principles, it is necessary to say what he understood by principles, and to what he attributed their origin.

We recall Lorentz's scepticism with regard to the principle of relativity; noting the major difference between his solution to the problem of the electrodynamics of moving bodies and Einstein's, Lorentz wrote with a certain irony: "...Einstein *simply postulates* what we, with some difficulties and not always completely satisfactorily, attempted to derive from the basic equations of the electromagnetic field. Of course, he requires of us that *we believe in advance* that the negative result of experiments such as those of Michelson, Rayleigh, and Brace is not the fortuitous compensation of opposite effects but the expression of a general and basic principle" (Ref. 3, p. 333 of the Russian translation; our italics)[6]

However, by principles Einstein did not mean a priori rationalistic arguments whose truth must be "believed in advance"; on the contrary, he saw in them *the result of a generalization from the complete set of known experimental facts*, a generalization without which it is impossible to draw any theoretical conclusions.

It was to this problem of generalizing a body of experience, as a method of theoretical physics, that Einstein devoted his inaugural speech on his election to the Prussian Academy of Sciences in 1914. He asserted that a single experiment says nothing to the theoretician; the investigator must "draw out from nature clearly formulated general principles, *which reflect definite general features of a huge body of experimentally established facts*. If such a formulation has succeeded, one can then embark on developing the consequences, which frequently give unexpected relationships that lead far beyond the domain of the facts from which the principles were obtained. Until principles capable of serving as the basis for deduction have beeen found, individual experimental facts are useless to the theoretician, since he is not capable of undertaking anything with separate empirically established general features (Ref. 1, pp. 14–15, our italics).

This remark, which is directed against empiricism, is very characteristic of Einstein's method. It was this method that he used to analyze the contemporary situation in the electrodynamics of moving bodies. He pondered the fact that in numerous and varied experiments, at all levels of accuracy with which they were performed, it always proved impossible to detect the influence of the relative motion of the Earth; could one attempt to preserve the existing theory by the introduction of a priori improbable hypotheses? Are we not looking for something that does not exist? Later, he formulated this thought as follows: "Can one really think that, through a curious chance, the laws of nature are manifested to us in such a peculiar manner that none of them make it possible to study the rapid motion of our planet through the ether? Would it not be more justified to assume that we have been led into a blind alley by an incorrect argument?" (Ref. 13, p. 143 of the Russian

---

[5] In the Russian collection of Einstein's scientific works (Sobranie Nauchnykh Trudov), this important passage is inaccurately rendered: instead of the part italicized, the translation speaks of Maxwell's electrodynamics "in its present form" (Ref. 13, p. 7). Because of this translation, the subsequent text can be taken as a rebuke addressed at Maxwell's theory. But in the original Einstein speaks of the *interpretation* of Maxwell's electrodynamics. Einstein did not assume that Maxwell's electrodynamics could lead to a contradiction with phenomena and did not aim to change its form; rather, he sought the condition of its invariance. [*Translator's Note.* The English translation of the passage in The Principle of Relativity (Dover publications), which is the one given in the text, does not suffer from this flaw.]

[6] In the text there follows an attempt "to say something in favor of and about the method" by which Lorentz himself "attempted to present his theory". Lorentz's lectures were published in 1909.

translation). Numerous facts led to a generalization: nature is such that inertial (or almost inertial) motion does not have an influence on physical processes. This was the step to the principle of relativity.

Poincaré: the Principle of Relativity as the Impossibility of Establishing Absolute Motion. In the period when the problem of the electrodynamics of moving bodies became acute, the role and essence of the principle of relativity had been discussed among physicists fairly widely. Prior to Einstein, this principle had been accorded greatest significance by Poincaré, who some authors regard as one of the creators of the foundations of relativity theory, since among the foundations the principle of relativity is undoubtedly the most important.

Poincaré saw the main significance of this principle in eliminating any procedure for measuring the absolute properties of motion. According to Poincaré, neither experience nor intuition provides a basis for measuring absolute intervals of time, an absolute simultaneity common to events at different places, absolute velocities, and so forth. In some cases, Poincaré simply declares relativity: "1. Absolute space does not exist, we know only relative motion.... 2. Absolute time does not exist. The assertion that two intervals of time are equal is in itself meaningless and can only be understood conventionally," thus wrote Poincaré in 1902 in his book Science and Hypothesis.[7] This book also contains a second formulation: "The motion of every system must satisfy the same laws irrespectively of whether we refer its laws to fixed axes or to axes moving rectilinearly and uniformly. This, the principle of relativity of motion, is essential to us for two reasons: first, it is confirmed by the most commonplace experience, and, second, the contrary supposition is inconceivable" (Ref. 6, pp. 23–24). However, the following arguments show that the conclusions which Poincaré drew from this formulation go no further than examples from classical mechanics and the conclusion that on the basis of the principle of relativity, it is "more convenient to assume that the Earth rotates, because then the laws of mechanics can be expressed in the simplest language" (Ref. 6, p. 26).

In his well-known lecture at St. Louis, delivered in September 1904, after Lorentz's fundamental paper had already been published (May 1904), Poincaré gave the most expanded formulation of the principle of relativity: according to this principle, "the laws of physical phenomena must be the same for a stationary observer and for an observer in a state of uniform translational motion, so that *we do not have and cannot have any method of establishing* whether we are in such a state of motion or not" (Ref. 6, p. 30; our italics).

We see here that Poincaré extended the formulation of the principle of relativity, noting that it includes the requirement that *the laws of physical phenomena be the same* for stationary and inertially moving observers. However, as before, he sees the significance of the principle only in the fact that, by virtue of it, there exists no way of distinguishing a state of absolute rest from inertial motion; indeed, this is indicated in the second part of the assertion.

That Poincaré saw here the essence of the principle of relativity is confirmed by the fact that he saw the main significance of Lorentz's investigation (the paper of 1904) in Lorentz's finding, willy-nilly, physical arguments why even in the extremely accurate Michelson experiment absolute motion was not detected (although from Lorentz's point of view it does exist). In that period of shattering of ideas about physical laws, many physicists spoke of the arrival of an epoch of crises of principles and that the new electrodynamic experiments would also "breach" the principle of relativity. In such a situation, Poincaré regarded as very important the appearance of a theory which could be regarded as a confirmation of his idea that there exists no way of determining whether an observer is in a state of absolute rest or in inertial motion. This was achieved by Lorentz's theory. True, Poincaré regrets that Lorentz could master his difficult task only by "heaping up hypotheses". Among them the "most ingenious was the idea of a local time", a time in a moving system. Poincaré points out how one must regulate clocks in this system. "Clocks regulated in this manner will not show the *true time*. They indicate the so-called *local time*. Some of them go slow. This does not have great significance, since we *do not have means to note this*. All phenomena that take place at, for example, point *A* will be retarded, but they will still remain exactly the same and an observer will not note this, since his clocks are slow. Thus, *as is required by the principle of relativity,* the observer *will have no possibility of discovering* whether he is in a state of rest or absolute motion (Ref. 6, p. 34; our italics in the two last assertions). "Unfortunately," continues Poincaré, "this principle alone is insufficient, and additional hypotheses are required"—he is speaking here of the contraction hypothesis. And that in its turn requires the new "hypothesis concerning forces": all forces, irrespective of their nature, are reduced, and, since they are "reduced in equal proportion, *we do not have anything*". "Thus," concludes Poincaré, "in recent time the principle of relativity has been steadfastly defended, bu the very energy of this defense shows how serious was the attack" (Ref. 6, p. 35).

In his paper "On the dynamics of the electrons" (1906) Poincaré calls the postulate of relativity "the impossibility of demonstrating experimentally the absolute motion of the Earth", and says that this impossibility "is evidently a general law of nature" (Ref. 6, p. 118). In this paper, Poincaré considers the extent to which Lorentz's theory presented in the 1904 paper corresponds to this law. Summarizing how this theory analyzes the electromagnetic picture of phenomena, Poincaré wrote: "Thus, Lorentz's theory completely explains the impossibility of demonstrating experimentally the presence of absolute motion in the case when all forces are of electromagnetic origin. However, there exists forces that cannot be ascribed to an electromagnetic origin such as, for example, the gravitational force.... Therefore,

---

[7] Quoted in the collection of Russian translations Ref. 6 (p. 23). The pagination in the following text is from this publication.

Lorentz was forced to augment his hypothesis by the assumption that *forces of any origin, in particular the gravitational force, behave in the case of translational motion (...) in exactly the same way as electromagnetic forces*" (Ref. 6, p. 152).

Poincaré grants that the impossibility of detecting absolute motion can be justified in different ways (i.e., the principle of relativity can be justified in different ways). One could improve Lorentz's theory, simplify it, investigate its deeper consequences, etc.; it was to this that Poincaré, who had completely mastered the mathematical knowledge of his time, strived; Poincaré regarded this as useful irrespective of the subsequent fate of Lorentz's theory, since he assumed that no theory disappears without trace in the subsequent development of science. But *Poincaré did not rule out the possibility of replacing Lorentz's system of hypotheses by a different system of hypotheses,* which would be simpler or more natural; one such hypothesis he attempted to outline in the St. Louis lecture. This hypothesis, which, of course, was not developed in detail, was based on the assumption that "the ether is modified when it moves relative to a material medium embedded in it: in the changed state, it no longer transmits disturbances in all directions with equal velocity". If the disturbances are transmitted in the direction of motion of the medium more rapidly than in the transverse direction, then "one could get by without such an unusual contraction of bodies" (as was assumed in Lorentz's hypothesis). This hypothesis would probably require the introduction of some additional hypotheses, but they could be simpler. Poincaré makes the reservation: "I give this only as an example, since the modifications that one could try undoubtedly *admit infinitely many variations*" (Ref. 6, p. 40; our italics).

It follows from this that Poincaré saw the main problem confronting physicists in the electrodynamics of moving bodies in the creation of a theory that would demonstrate the nonexistence of means of establishing absolute motion and absolute time: one could construct a set of variants of physical hypotheses and apply them in different inertial systems but the investigator must come to the conclusion that absolute motion is not revealed in any system, since the physical theories in them do not differ in their form.

This conclusion is the *final aim* of Poincaré's investigations. With regard to the various basic hypotheses (contraction of a body in motion, anisotropic modification of the ether, or some other idea), these are merely conventionally chosen "rules of the game", and the simpler they are, the better: the important thing is that all rules lead to the same result—the postulate of relativity, the essence of which Poincaré sees in our having no means for establishing absolute motion, a fact which is confirmed "by the most commonplace experience" and the observation that "the contrary supposition is inconceivable" (Ref. 6, pp. 23–24).

Einstein: the Principle of Relativity as a Law of Nature, the Point of Departure for Subsequent Knowledge. Einstein did not assume that the principle of relativity follows "from the most commonplace experience" nor that the concept of absolute motion "is inconceivable". In contrast, he regarded the principle as a generalization of *scientific* experience and frequently referred to this. Thus, defending the theory of relativity from the attacks of "two distinguished specialists", Einstein wrote in 1914: "...from the experimental point of view it is hard to doubt that the principle of relativity does not hold. Indeed, if it did not, the natural phenomena in a coordinate system at rest relative to the Earth would be influenced by the annual motion of the Earth around the Sun: as a result of this motion, physical anisotropies should be manifested in terrestrial laboratories. However, despite the most strenuous exertions, physicists have never observed such an anisotropy. Therefore, the principle of relativity is as old as mechanics and it *is hardly possible for anyone to doubt it from the point of view of experiment*" (Ref. 13, p. 386 of the Russian translation; our italics).

In the absence of physical anistropy accompanying the annual motion of the Earth, Einstein saw the most important argument for the existence in nature of the relativity effect. He also advanced this argument in his well-known talk at the session of the Society of Natural Scientists at Zurich in 1911 (Ref. 13, p. 179 of the Russian original).

In accordance with his understanding of the origin of principles as "reflecting definite general features of a vast body of experimentally established facts", he regarded the princple of relativity as *experimentally justified* to that extent.

What did Einstein see as the essence of this principle? Already in the first paper of 1905 he clearly formulated its significance: "The laws by which the states of physical systems undergo change are not affected, whether these changes of state be referred to one or the other of two systems of coordinates in uniform translatory motion" (Ref. 6, p. 100, or Ref. 13, p. 10 of the Russian translation).

Similar definitions with slight variations are repeated in many of the subsequent papers devoted to elucidating the essence of the theory of relativity.[8] Einstein expressed the significance of the principle of relativity very perspicuously and correctly by pointing out that two physicists, together with their measuring instruments, in two different inertial systems "discover identical laws of nature" (Ref. 13, p. 175 of the Russian translation). We should here add that this opens up the exceptionally important methodological significance of the principle of relativity, which enables reasoning man to go beyond the narrow confines of the system in which he makes his first acquisition of knowledge.

There is no doubt that the success of Einstein's first paper in 1905 was due to the correct understanding of the essence of the principle of relativity and the physical conclusions that Einstein drew from this principle. Whereas Poincaré emphasized the negative function of the principle of relativity (an observer has no means of

---

[8] See, for example, Ref. 13, pp. 69, 144, 145, 152, 175, and 386 of the Russian translation.

establishing his state of motion), and saw in it a confirmation of his philosophical conception, Einstein was not content simply to state the fact of invariance of the form of the laws of nature. The main difference in Einstein's approach to the evaluation of the principle of relativity was that *he did not see it as the final result of knowledge but as the point of departure for subsequent acquisition of knowledge;* the princple made it possible, from a unified position, to discover the "mechanism" of numerous physical phenomena, predict new phenomena, and ultimately change and broaden ideas about the objective world and its connections.

This approach encountered psychological and methodological difficulties, conservatism of thinking, and the force of tradition.

The Problem of Time. One of the first difficulties in the way of acceptance of the theory of relativity was the problem of time. In Newtonian physics, time was regarded as something absolute that flows uniformly and is measured independently of physical processes; it was a kind of background, foreign to the material processes, on which they took place. Through this independence, time in classical physics played a particular part among the other variables and acquired a status of a certain *substantially*.

However, the situation changed when, through the solution of the problems of the electrodynamics of moving bodies, it was found that the system of Maxwell's equations preserves its form, expressing the connection between partial derivatives of variables of the same nature but expressed in terms of different inertial frames.

We have already seen that this caused Lorentz to introduce the concept of a "local time", which was regarded as an "ancillary mathematical quantity", to be contrasted with the "true" time. Many physicists adopted an unbelieving attitude to the new views because of the appearance of the different measures of time, and this led to difficulties in acceptance of the new concept.

But for Einstein this difficulty did not exist. The principle of relativity, experimentally confirmed, states that the frames of reference K and K' do not differ physically but are on an equal footing, and that, with allowance for the opposite sign of the relative velocity, the transition from K to K' is equivalent to the opposite transition from K' to K. This means that *variable parameters of the same kind play an identical part* in the theory formulated in the system K as in its invariant form in the system K'. In particular, this also applies to the variable parameter "time".

The fact that the interconnection of the partial derivatives is rigidly fixed in Maxwell's electrodynamics by a system of equations means that *none of the variable parameters has an independent law of variation* or a law of change determined *outside the given integral system* (theory), in contrast to Newton's conception with regard to time (and also space). It is only this circumstance that ensures the fulfillment of the principle of relativity in its deeper, Einsteinian interpretation.

The understanding of this part played by the interconnection of categories appeared in such an explicit form in physics for the first time.[9] It enabled Einstein to overcome the difficulties associated with the classical interpretation of time. The *substantial* interpretation, in which time was regarded as something independent of physical processes, as universal, unique, and uniformly flowing, was supplanted by the *relational* interpretation of time, according to which time is essentially related to physical processes and, ultimately, to the laws of nature.[10] In this interpretation, as Einstein asserted, "spatial and temporal data do not have a fictitious but a physically real meaning" (Ref. 14, p. 24 of the Russian translation).

This interpretation is close to the concept of time as a form of existence of matter, and it leaves possibilities for subsequent deepening of the concept of time, which is essential in the further development of our knowledge of nature when the transition is made to the cosmic and subatomic worlds.[11]

The Velocity of Light. Another difficulty that arose in the breakdown of the old concepts was that of the constancy of the velocity of light in different inertial systems.

In discussions in the literature devoted to the history of the theory of relativity it has been argued that at that time the hypothesis of the constancy of the velocity of light had not yet been experimentally confirmed, and that even Einstein, who made this into a basic principle in the 1905 paper, subsequently, in discussing the procedure for synchronizing clocks, adopted the equality of the velocity of light "there" and "back" only as a convention. Many physicists could not comprehend how the velocity of light could remain the same in two inertial frames moving relative to each other with velocity v; for the velocities are added in accordance with the law of composition of vectors, and the result of composition must depend on the relative velocity.

Even after the first recognition of Einstein's work not a few doubts were expressed with regard to the validity

---

[9] Implicitly, this interconnection is of course already contained in Maxwell's equations; geometers had arrived at this idea earlier (see below).

[10] We use the expressions "substantial time" and "relational time" as understood in Soviet philosophical literature (see, for example, Ref. 16).

[11] In our age, now that group theory has been developed and made more profound, one can formulate the idea that all variable parameters, including the time, are on an equal footing by using the fact that the Lorentz transformations form a group. The actual fact of the group nature of the transformations was already established by Einstein in his first paper in 1905 (Ref. 13, p. 21 of the Russian translation).
Poincaré devoted a special section to it (Ref. 6, p. 133) and used it to consider the conditions that gravitational forces must satisfy if, in accordance with Lorentz's hypothesis (see above), they behave in the case of translational motion in exactly the same way as electromagnetic forces. Only later did physicists recognize that this fact can be regarded as the mathematical form of expression of the fact that the variable parameters in different inertial frames are on an equal footing.

of the proposition about the constancy of the velocity of light.[12)]

It must however be recognized that the hypothesis of the constancy of the velocity of light in different inertial frames was fully justified in Einstein's conception and justified to the same extent as the system of equations of Maxwell's electrodynamics and the principle of relativity—experimentally.

Indeed, the velocity of light occurs in the equations of electrodynamics as a constant. According to Einstein, the princple of relativity asserts that the laws of nature, in this case the electrodynamic laws, have identical form in all inertial frames: this means, as we have seen above, that in some frame K' one has the same connections between the variable parameters despite the fact that the variables themselves in this new frame are measured by different scales (E', H', x', y', z', t); however, the constant c does not depend on these variable parameters. If on the transition to a different inertial frame this quantity were to change, in other words, if the velocity of light were then to have a different value, this would be in conflict with the principle of relativity, the form of the equations of electrodynamics would not be preserved, and this would contradict the experiments.

Einstein repeatedly emphasized this interconnection between the principle of the constancy of the velocity of light and the principle of relativity in its application to Maxwell's electrodynamics. "If we wish to retain the principle of relativity," wrote Einstein in 1910, "we must allow the validity of the constancy of the *velocity of light* for any system moving without acceleration" (Ref. 13, p. 146).[13)] Of course, this entails "the need to give up the usual law of composition of velocities, or better, to replace it by a different law" (Ref. 13, p. 146).

Explaining the fallacy of references to the classical law of composition of velocities as an argument against the principle of the constancy of the velocity of light, Einstein pointed out that if the velocity of light c with respect to a system K were to be added vectorially to the relative velocity v of the system itself, then "the laws of propagation of light in the system K' would differ from the laws of propagation of light in the system K, would be tantamount to violation of the principle of relativity. This would be a terrible conclusion. But it turns out that nature does not come to such a conclusion. It has arisen because of the fact that in our arguments φ we have tacitly made assumptions which must be rejected if we are to arrive at a consistent and simpler understanding of things" (Ref. 13, pp. 179–180).

For greater clarity, let us emphasize the following two circumstances. The interpretation developed by Einstein of the principle of the constancy of velocity of light in different inertial frames and his views on its connection to the principle of relativity preclude our regarding the two principles as two independent, albeit compatible principles, as could be concluded from the first paper of 1905. Second, this constancy of the velocity of light is valid only for sufficiently small values of the gravitational potential, i.e., only to the extent to which the very concept of inertial frames is valid.

Thus, both these difficulties can be resolved; the need for a transition to a new interpretation of time and the recognition of the principle of the constancy of the velocity of light in different inertial frames are justified by two facts— the validity of Maxwell's system of electrodynamic equations and the invariance of the form of these equations under Lorentz transformations (the principle of relativity). And they are in complete agreement with all experimental facts. Therefore, these difficulties only arose because of the traditional manner of thinking in classical physics.

Einstein's Physical Theory. As we have seen, when Einstein analyzed the problems confronting electrodynamics at the turn of the century, he found two basic principles by means of which one could advance further. The first was that the mathematical form of Maxwell's electrodynamics was adequate to describe all the varied experimental facts. The second principle, to the effect that the laws of nature exhibit the same form in all inertial frames, was the principle of relativity. This principle was the result of generalization of numerous and varied electrodynamic experiments. Einstein saw the task of the theoretician in not only stating this fact but in extracting from it positive conclusions that would lead to a deepening of physical knowledge.

The first and most important conclusion drawn from this principle is that it is identical to the condition that interconnections in each inertial frame are expressed in terms of an intrinsic set of variable parameters of the same nature $(\mathbf{E}, \mathbf{H}, x, y, z, t; \mathbf{E}', \mathbf{H}', x', y', z', t'; \ldots)$, measured in this system, and that these interconnections remain the same in any inertial frame. The next step, the establishment of the connection between the variables measured in different inertial frames, is a natural one.[14)] It should be noted that the transformation formulas have an objective meaning. They acquire great significance in connection with the fact that, because of the physical conditions, phenomena are observed, not in their proper inertial frame, but in some other one (for example, the laboratory frame). The transformation laws make it possible to discover the laws of phenomena in the proper frame on the basis of the situation observed in some other frame.[15)]

[12)] Pauli mentions the papers of Tolman (1910), Kunz (1910), Comstock (1910), and especially Ritz (1908) (see Ref. 17).

[13)] In fact, already in the second paper published in the next issue of the Annalen der Physik, Band 18, in the same year 1905, he wrote: "The principle of the constancy of the velocity of light used there [in the first paper] is of course contained in Maxwell's equations" (Ref. 13, p. 36).

[14)] As is well known, this can be done by different methods. One of them was indicated by Einstein in the paper on p. 183 in Ref. 13.

[15)] We do not consider here the question of the transport of a body (or a rod or clock) from one inertial frame to another with avoidable acceleration; recently, this question has been considered in a separate paper by Feinberg.[18]

Following his own method, Einstein derived from the transformation formulas (ultimately from the principle of relativity) numerous consequences—the objective nature of the relativity of length and simultaneity, the slowing down of time, the conclusion that electric and magnetic fields are components of a single electromagnetic field, the transverse Doppler effect, and so forth. We must not omit to mention the remarkable conclusion that "the mass of a body is a measure of the energy contained in it" (Ref. 13, p. 38 of the Russian translation). None of these effects depends on the nature of the acting forces, which are not considered in any of the premises. The interconnected consequences are confirmed experimentally.

Thus, Einstein created an integral logical system—the (special) theory of relativity. It can undergo improvements, but one cannot say that the theory of relativity had all been prepared before Einstein and it only remained for him to place the stone that crowned the arch: the entire logical system was systematically developed by Einstein himself.

Methodological Aspects of Einstein's Investigations. We see that Einstein solved the problem of the electrodynamics of moving bodies without attempting to find the conditions of equilibrium of forces in a moving body, as Lorentz had done. His deep understanding of the principle of relativity as a law of nature guaranteeing invariance of the equations in different inertial frames made it possible to solve the outstanding problems without having to resort to an analysis of the structure of moving bodies or to the unending "heaping up of hypotheses" (Poincaré). Einstein's method turned out to be fruitful.

Further, Einstein did not consider the status of time without analyzing the part that it played in the confirmed theory. He was not blinded by the concept suggested by classical physics of a universal, unified and uniformly flowing time, nor by the assertion that absolute time cannot be observed. The important thing was that time was organically included in an adequate theory (already in Maxwell's system of equations) and that, according to the principle of relativity, it is different for different inertial systems. Having recognized this connection, Einstein did not shrink from the most fundamental conclusions, even those leading to the shattering of the classical notions of substantial nature of space and time. It was this method that ensured the success of the new physical theory and opened up the next step to the general theory of relativity.

This method of evaluating the content of categories in the light of the part they play in an adequate theory has a general gnosiological significance. Any theory which has passed the test of adequacy with regard to objective reality constitutes an integral whole to which the content of the categories used in it must conform. This connection of logical categories (and the corresponding aspects of objective reality) was justified long ago in the analysis of problems of political economy, as realized by Marx and his followers.

The application of this idea in physics played an important part in the establishment of the theory of relativity. For Einstein, it was not a chance episode; it can be traced in many of his subsequent studies, and we shall draw attention to it where appropriate. For physics, this was a new method of theoretical generalization of the results obtained from the investigation of nature.

It is this method of Einstein that should attract the attention of those who wish to know who created the theory of relativity and how; they should not get involved in discussions to which Keswani devoted entire sections in his paper of Ref. 5: "Did Einstein know of Poincaré's work" and what did he extract from Poincaré's book Science and Hypothesis or "Did Einstein know about Lorentz's paper of 1904" before publication of his paper in 1905.

III. WHAT IS THE STARTING POINT OF EINSTEIN'S THEORY?

"Kinematic Part" and the Procedure of Measurement. Do the ideas about space and time that follow as consequences of the theory of relativity find their reflection in the practice of direct measurement of variable parameters? Of course, yes.

Since the theory of relativity established a connection between space and time and physical processes, it is clear that this connection must also be taken into account in the procedure of space-time measurements.

But one may ask how and at what stage is it most expedient to develop this procedure and whether it should be represented as a consequence of the logical system outlined in the preamble or as an independent point of departure.

It may be assumed that, bearing in mind the need to overcome the conservatism of the ruling opinions of physicists, Einstein felt it necessary to show immediately that definitions of concepts are by no means as simple as is generally assumed, and that this is revealed already in an analysis of the procedure of measurement.

Whatever the truth may be, Einstein began the "Kinematic Part" of the 1905 paper by considering various procedures by means of which one could measure different physical quantities in both an original ("stationary") system as well as in a system moving inertially with respect to the first. The most important problem is that of time. Every judgement in which time plays a part, Einstein wrote, is a judgement about simultaneous events. Simultaneity of events that occur next to each other can be established directly. But simultaneity of events at different places A and B cannot be directly established. Clocks at A and B can only be synchronized indirectly, for example, by means of light signals; this requires knowledge of the velocity of light, which is assumed to be equal in both directions between A and B (assumed by definition, since in this restricted situation there are no means for measuring the velocity of light). Using this method of synchronization to define simultaneity of two events in a moving system, Einstein finds that "two events which, viewed from one system of coordinates, are simultaneous, can no longer be looked upon as simultaneous events when envisaged from a system which is in motion relatively to that system" (Ref. 13, p. 13 of the Russian translation). The concept of simultaneity is not absolute. No more is the concept of the length of rods or intervals of time; quite generally, the numerical values of physical quantities are related to the system with respect to which the measurement is made.

As a result of his analysis of the measurement procedure, Einstein arrived at the already mentioned conclusion that "to any system of values x, y, z, t, which completely defines the place and time of an event in the stationary system, there belongs a system of values ξ, η, ζ, τ, determining that event relatively to the system K [i.e., another inertial frame], and our task is now to find the system of equations connecting these quantities".

To do this, Einstein of course used a number of consequences of the principle of relativity (constancy of the velocity of light, homogeneity of space and time); ultimately, Einstein showed that the obtained transformations lead to invariant forms of the equations of a spherical light wave in two inertial systems (see above) and, therefore, prove the consistency of the two basic principles—the principle of relativity and the principle of the constancy of the velocity of light.

Thus, in the "Kinematic Part" Einstein went in a direction opposite to that which at the start he had defined as the program for investigating the electrodynamics of moving bodies.[16)]

Einstein Commentators—Bridgman and Reichenbach. The "Kinematic Part" was the stimulus for many physicists and philosophers to interpret in their manner, and not at all in the spirit of the preamble to Einstein's pioneering paper, the essence and origin of the theory of relativity. We have seen that this part of the paper begins with the definition of concrete physical procedures relating to the concepts of simultaneity, length, etc.; in contrast, the principle of relativity and the principle of the constancy of the velocity of light, which Einstein had declared in the preamble to be premises of the theory, are, it is true, used in the derivation of the transformation formulas but do not appear prominently and seem to be used rather as ancillary mathematical devices. This circumstance encouraged the physicists and philosophers who assumed that any theory begins in this manner— with the analysis and selection of individual concepts, the "building bricks" of the theory, and that one could neglect an analysis of the path laid out by Einstein in the preamble (and subsequently realized).

Let us consider the positions of some of the commentators whom Einstein felt obliged to answer; his answers reveal the views of Einstein himself on the path to the establishment of the theory of relativity.

The well-known physicist P. Bridgman saw the fruitfulness of Einstein's work in that he first enabled physicists to understand that the meaning of a concept introduced in a theory is disclosed only through a definite physical operation of measurement; in the concrete case, he had in mind the concept of simultaneity. Bridgman assumed that any measuring procedure always contains all the details necessary to reveal the meaning of the concept, though their significance is frequently ignored. He asserted that one must make a careful and ever deeper analysis of all the details of existing operations of measurement and the hidden assumptions behind them, which could be the key to a new situation. In Bridgman's opinion, Einstein made a "deeply penetrating analysis" of this kind.

Bridgman's gnosiological conclusion was that a theory can and must be constructed solely on the basis of previously formulated concepts, and moreover these concepts must be such as can be associated with some physical operation; all other concepts must be eliminated. This was the beginning of a gnosiological direction in physics—*operationalism*. Bridgman himself regarded Einstein as the originator of the operationalistic ideas; however, Bridgman viewed his paper "Einstein's theories and the operational point of view", dedicated to Einstein on his 70th birthday, as a reproach to Einstein for the fact that in formulating the general theory of relativity he had actually abandoned the method which he and himself taught physicists in constructing the special theory of relativity.[17)]

In the same publication,[19] Hans Reichenbach, Professor at the University of California and the author of many papers on gnosiological problems of physics, published his paper "The philosophical significance of the theory of relativity." Reichenbach regards the establishment of the conventional and defining nature of physical propositions as the "definitive formulation of the logical significance of the theory of relativity". "The logical basis of the theory of relativity," writes Reichenbach, "is the discovery that many statements, which were regarded as capable of demonstrable truth or falsity, are mere definitions" (p. 293). Even more precisely: "... that the simultaneity of events occurring at distant places *is a matter of definition* was not known before Einstein based his special theory of relativity *on this logical discovery*" (p. 294; our italics). According to Reichenbach, definitions are established at will. Thus, the result of comparing intervals that are separated from one another is determined by the nature of the adopted congruence, i.e., the adopted method for comparing these intervals: "Another definition would result if we regarded a rod, once it had been transported to another location, as twice as long, thrice transported as three times as long, and so on," writes Reichenbach (p. 294), demonstrating by this example that adopted

---

[16)] In the "Electrodynamic Part" measuring procedures are not considered at all, and it is merely noted that the differentiation of the components of vectors E and H is made with respect to the transformed variables of the coordinates and the time, and the principle of relativity is used to derive the connection between the field intensities in one inertial frame and those in another.

[17)] The paper is printed in the well-known collection edited by Schilpp, published on the occasion of Einstein's 70th birthday.[19] The pagination in the text refers to this publication.

definitions are conventions. Thus, a concrete physical theory is regarded from the start as the product of convention. From the point of view of conventional definitions, asserts Reichenbach, "...we could let both observers [in the system "at rest" and in the moving system] employ the same definition, *for instance that of* the system "at rest". Such variations would lead to different transformations; for instance, the last mentioned definition would lead, not to the Lorentz transformation, but to the classical transformation from a system at rest to a moving system" (p. 295). "Definitions are arbitrary," says Reichenbach, dotting all his *i*'s," and it is a consequence of the definitional character of fundamental concepts that with the change of the definitions various descriptional systems arise" (p. 295).

We see that Bridgman merely interpreted from his point of view the logical bases of the special theory of relativity and did not pretend to reconstruct it. Reichenbach's conception goes further, and has a tendency to justify other formulations of the theory of relativity. If the logical basis of the theory was the definition of the procedure for measuring simultaneity of separated events, and every definition is, according to Reichenbach, a matter of convention, this interpretation would open up possibilities for regarding the theory of relativity—the one known to all physicists—as *only one of the variants of a conventional theory*.

This conception, which makes its own definition of the logical basis of the theory of relativity, completely ignores the direct indications of Einstein himself to the experimentally confirmed basic principles, and also the methods that led him to the construction of the theory of relativity.

Einstein: the Answer to Reichenbach and Bridgman. Einstein answered them in the same festschrift in which their papers were published (translated in Ref. 1, p. 304 ff).

He gives a more detailed answer to Reichenbach, whom he immediately engages in a hypothetical discussion with Poincaré. The question under consideration is whether one can define the meaning of concepts, in particular geometrical concepts, outside a theory and prior to theory. Poincaré regards the choice of geometry as a matter of convention. Geometry applies, not to real, but ideal bodies, the concept of which is taken entirely from our mind (see Ref. 25, A-90, B-83). There is no geometry that can be either confirmed or refuted. Reichenbach essentially adopts the same position, with the only difference that he assumes geometry to be incapable to confrontation with experiment until one has specified a "coordinative congruence", i.e., until one has specified a rigid body by means of which the concept of geometrical interval is realized: if the rigid body is produced, experiment confirms Euclid's geometry.

Einstein demonstrates the illusory nature of Reichenbach's attempt to adopt Poincaré's position and raise himself above it; if you accept the conventional nature of geometry, no specification of a congruence can improve the situation, since a new problem supplants the old: can one operate with the concept of a rigid body and how does one define it? It is with the aim of demonstrating tactfully the proximity of the two positions that the form of the hypothetical debate between Reichenbach and Poincaré is chosen.

Thus, we are again confronted with a problem—how do we define a basic concept *before all experiences and in advance of theory;* previously, we were discussing simultaneity and the velocity of light, now we are discussing a geometrical interval, a *rigid body*, and the connection between geometry and experience.

In the hypothetical discussion, Poincaré answers Reichenbach that it is impossible to identify a rigid body, since "empirically given bodies are not absolutely rigid and, therefore, cannot serve as a realization of geometrical intervals. Therefore, the theorems of geometry cannot be verified in practice." We choose Euclidean geometry as the most convenient and the simplest. Reichenbach agrees that there are no absolutely rigid bodies in nature; but could they not be replaced by ordinary, real bodies, since physics gives us knowledge how they change with physical conditions (as a result of heating, magnetization, etc.)? Here too Poincaré refutes Reichenbach: you have used physical laws, but their formulation presupposes Euclidean geometry; hence, you have verified, not geometry, but geometry in conjunction with physics, "...an examination of geometry by itself is consequently not thinkable. ...why should it consequently not be entirely up to me to choose geometry according to my own convenience (i.e., Euclidean) and to fit the remaining (in the usual sense "physical") laws to this choice in such a manner that there can arise no contradiction of the whole with experience?" (Ref. 19, p. 677).

Einstein then makes Reichenbach in the debate incapable of answering Poincaré and he more or less says that there is something attractive in Poincaré's conception, but, on the other hand, we have in classical physics used the concepts of *interval, distance, and rigid body* and there were no complications—why cannot one proceed further in the same manner?

At this point of the debate, Einstein replaces Poincaré by a different opponent, a Nonpositivist (Nicht-Positivist), who also criticizes Reichenbach, but from opposite positions; in all probability, he expresses Einstein's own thought. This interlocutor notes contradictions in Reichenbach: on the one hand, Reichenbach adheres to a principle: the expression "has meaning" is identical to the expression "is verified experimentally"; on the basis of this he ought to eliminate geometrical concepts and theorems, since it is recognized that they cannot be verified experimentally. On the other hand, defending himself from Poincaré's criticism, he is forced to refer to the actual situation, to history; theories did develop, and they were a gain, using concepts such as a rigid body although in nature there are no absolutely rigid bodies, and therefore this is possible.... But here the Nonpositivist traps Reichenbach in a contradiction: what about your basic principle that a concept has meaning if it is verified experimentally? And the Nonpositivist concludes: "Must you not admit that it is quite impossible to give any "meaning" in your understanding to individual concepts and propositions of a physical theory, and that meaning can be given only to an integral system since it renders experimental data "amenable to cognition"? Why do the individual concepts (die Einzelbergriffe) encountered in a theory require special justification *if they are needed only in the framework of the logical structure of the theory,* and the theory *is confirmed as a whole* (als Ganzes)?" (Ref. 20, p. 503; our italics).

Einstein expresses the same opinion directly in his own name in the brief answer to Bridgman, who, as we have said, required that every concept be associated with a definite physical operation. "In order to be able to consider a logical system as physical theory it is not necessary to demand (ist es nicht notwendig zu verlangen) that of its assertions can be independently interpreted and "tested" "operationally"; *de facto* this has never yet been achieved by any theory and can not at all be achieved. In order to be able to consider a theory as a *physical* theory it is only necessary that it implies empirically testable assertions in general" (Ref. 19, p. 679).18)

Einstein regarded this brief answer to Bridgman as exhaustive, but he also notes that his comments addressed to Reichenbach also have a direct bearing on Bridgman's paper.

We see that the commentators on Einstein's methods, Reichenbach and Bridgman, proceeded from the assumption that in the first place it is necessary to give a conventional or an operational definition of the concepts that are to be used in the theory; this requirement unified the representatives of many directions, although the question of the method of defining concepts was resolved differently.

Einstein radically altered the formulation of the problem: it is not correct to require justification of the concepts separately, since they play a part only in a theory that is confirmed *as a whole.*

Einstein and the Principle of Observability. We have seen that there are many different accounts of how the theory of relativity "began"—with the convention of equality of the velocity of light in opposite directions, with a preliminary definition of the concepts, with a choice of the procedure for measuring simultaneity, ... And each interpretation of the "beginning" illuminates the significance of the theory in its own way. In the literature, one can also encounter the assertion that "everything began" with the elimination of absolute space and time, as unobservable. In this connection, it is appropriate to recall an episode that brings to light clearly Einstein's own attitude of the problem of "how it began" and his evaluation of the significance of theory in the process of cognition.

Comparatively recently, in 1968, Heisenberg spoke at The International Symposium on Problems of Modern Physics at Trieste about a discussion he had with Einstein in 1926 (Ref. 21). Einstein criticized Heisenberg for taking as the philosophical basis of the quantum mechanics which Heisenberg had developed at that time the principle of eliminating unobservables. To which Heisenberg responded: but Einstein himself created the theory of relativity on the basis of the philosophy of eliminating unobservables "because he also denied absolute [i.e., unobservable] time and introduced time only for a definite system of coordinates". Einstein explained to me, continued Heisenberg, that it was really the other way around. He said: "Whether you observe a phenomenon or not depends on the theory that you adopt. Namely, the theory determines what you can observe and what you cannot". Heisenberg admitted: "This remark was very important for me later, when together with Bohr I discussed the interpretation of quantum mechanics.... Einstein had drawn my attention to the fact that to assert that one should talk only about observable quantities could even by dangerous. This is because every reasonable theory, besides directly observable quantities, must also give the possibility of observing something more indirectly. Mach, for example, was convinced that the concept of an atom was adopted only for its convenience, for the sake of economy of thought, and he did not believe in the reality of atoms. In our day everyone would say that this is nonsense; it is perfectly clear that atoms exist."

It follows from this that the ideas of the theory of relativity developed not at all in the direction that Einstein first attempted to free himself from absolute space and time—because they are not directly observable—but "really the other way around", i.e., he proceeded from the discovery of real laws of nature and concluded that they left no room for the concepts of absolute space and time.

Conclusions. We see that the most varied interpretations of the sources of the theory of relativity have been put forward over the many decades since the formulation of the theory. Einstein's objections show that he defended his conception of the development of theory outlined in the preamble to the 1905 paper and realized subsequently. It proceeds from a generalization of the rich experience in the domain of electrodynamics and the search for the conditions of consistency of its results; it does not proceed from conventional or operational definitions of concepts taken by themselves prior to the creation of the theory. In particular, although he did in the "Kinematic Part" give a definition of simultaneity of separated events, he did not accord it fundamental status in the establishment of the theory. The definition is a *consequence* of an adequate theory of relativity, just as the elimination of the idea of absolute space and time is a consequence of the theory.

IV. TWO LINES: POINCARÉ–EINSTEIN

The studies devoted to the history of the establishment of the theory of relativity have noted two curious circumstances associated with the name of Poincaré.

---

18) For the Russian translation, see Ref. 1, pp. 305, 306; I have made the translation more precise; the German text in brackets is rendered in Ref. 1 by the words "neobkhodimo potrebovat (it is necessary to require)", which attributes to Einstein an opposite meaning incompatible with what follows.

One of them is that Poincaré, who in 1906 published a major investigation "On the dynamics of the electron," in which he directly responded to Lorentz's well-known paper of 1904, never uttered a word about Einstein's papers of 1905 and subsequent ones, in which the complete theory of relativity was developed and its consequences demonstrated. How is one to explain the fact that papers which immediately became the subject of lively discussion in scientific circles were ignored by a great scientist who in a number of papers himself defended relativistic ideas?

The second circumstance is the fact that Poincaré—a brilliant mathematician, the author of a classic on celestial mechanics, on the qualitative analysis of differential equations, and other deep mathematical problems—while "coming close to Einstein's idea" still "nevertheless did not make the decisive step and left to Einstein the honor of extracting all the consequences from the principle of relativity". This bitter admission is due to a great physicist and compatriot of Poincaré, Louis de Broglie, who, according to his own account and still during Poincaré's life "unceasingly read the volumes of Poincaré's course of mathematical physics and his publications on the philosopy of science".[22] His statement was made almost in our time, in 1954, in a speech to mark the centenary of Poincaré's birth[19].

How can one explain the unexpected "departure from the game" of the active fighter for the idea of relativity after the appearance of Einstein's papers?[20]

Goldberg explains Poincaré's silence by the suggestion that Einstein's theory appeared in Poincaré's eyes as insufficiently simple, insufficiently flexible (because of "logical rigidity"), and insufficiently natural, "and to such an extent that he did not feel it necessary to mention it".[24]

Goldberg's answer seems to us unconvincing, and a more persuasive answer requires at least a brief comparative analysis of the gnosiological conceptions of the two scientists and an examination of the problems that they posed for natural science. This is all the more necessary is that Einstein's standpoint with regard to Poincaré's views is not represented entirely accurately in the literature, and this confuses the situation.

Therefore, we shall answer the questions we have posed at the end of this section, after a preliminary discussion of the Poincaré–Einstein theme.

Einstein's views as a natural scientist. We have seen above how the theory of relativity was created by Einstein, and we have summarized the process in earlier pages. Einstein proceeded from a generalization of numerous experiments to the formulation of a theory in which he emphasized its integral nature, and then turned to an objective experiment predicted on the basis of theory.

Einstein accorded great importance to experiments—both as the basis for generalizations and as criteria for the truth of propositions and the adequacy of a created theory. It is well known to physicists that in his theoretical papers of that period he frequently immediately indicated what experiment and under what conditions one should set up to verify his newly formulated theory.

This shows that Einstein regarded theory itself as a means to the discovery of the laws of nature as well as to the discovery of the structure of individual systems. It is very characteristic that, for example, studying Brownian motion and the phenomena of fluctuations generally, he subordinated his investigations into statistical mechanics and thermodynamics to the same task, of which he wrote: "My main aim was to find facts that would establish as reliably as possible the existence of atoms of a definite finite size" (Ref. 1, pp. 275–276 of the Russian translation).

Similarly, his interest in Planck's investigations was dictated not so much by the individual consequences of Planck's results, however important they may have been, as by "what general conclusions does the radiation formula enable one to draw about the structure of radiation and, generally, the electromagnetic basis of physics" (Ref. 1, p. 275).

And did not the highly theoretical investigations into stimulated radiation lead to the discovery of objective laws of atomic structures that eventually in our time have become the basis of modern laser technology?

Alongside this, Einstein's entire scientific activity was characterized by a single general line; he strove to pass from one physical picture of the world to another, ever deeper and more generalized. Thus, from the special theory of relativity he passed to the general theory, including in it noninertial frames of reference; moreover, he believed that in this way he would arrive at a unified picture that would encompass the entire world in the system of a unified field in which discrete elements must be expressed as singular points determined by the field parameters. From the point of view of physics in the seventies, it is easy to see onesidedness in his searches for concrete ways to solve this problem and to point out that he did not learn about the tremendous extension of our knowledge of the microscopic world, etc., and so forth. But one cannot deny that Einstein posed before physics much grander tasks than those posed by some theoreticians, i.e., the ordering of our sense perceptions by means of physical thought. Einstein's aims were the *grandiose* one of reflecting the objective world in our cognition. In 1927, he formulated them thus: "We wish to know not only *how* nature is constructed (and *how* natural phenomena occur), but we also wish to achieve the aim, perhaps

---

[19] Another compatriot of Poincaré, the well-known physicist Brillouin, criticizing the ideas of the general theory of relativity (from the point of view of Bridgman's operationalism, in his understanding of it), nevertheless said that (in contrast to the general theory) "Einstein's special theory of relativity is an eminent achievement",[23] i.e., Brillouin, like de Broglie, did not doubt that the authorship of the special theory is due to Einstein.

[20] Einstein's paper "On the electrodynamics of moving bodies" was submitted on June 30, 1905 and published in September 1905; Poincaré's paper "On the dynamics of the electron" was submitted on July 23, 1905 and published in 1906.

utopian and audacious, of finding out why nature is thus, and not otherwise. In this, scientists find their highest satisfaction" (Ref. 14, p. 245 of the Russian translation).

Poincaré Gnosiology. Conventional Choice of the Means of Description. Reichenbach. Poincaré's concept of cognition was radically different from that of Einstein, the natural scientist.

Poincaré was one of those thinkers for whom the existence of material objects was no more than a "convenient hypothesis" (Ref. 25a, p. 246, Ref. 25b, p. 231). He treats the problem of acquiring knowledge not from the standpoint of elucidating the ways in which the objective laws of nature are reflected in the consciousness of man but from the standpoint of analyzing the connections between the sensations that man obtains through his sense organs, of elucidating their relationship to the intellect, to the capacity of the intellect, on the basis of imperfect sensations, to create logical abstract schemes, to classify the facts of sensations, and so forth.

Poincaré also spoke of experience as the first source of knowledge, but he regarded this experience as an accumulation of sense perceptions. At the level of sensual experience, many concepts are formed—different spaces (visual, tactile, motor) and their properties (homogeneity, isotropy, dimensionality, etc., determined, in Poincare's opinion, by the *physiological* properties of the human organs); there are the physiological concepts of solids, continuity, and so forth (see, for example, Ref. 25, Ch. 4—Space and Time).

But sensual experience is imperfect, and Poincaré concluded that there arises a need for a perfecting of concepts, which is achieved by the intellect. This last, on the basis of the space of physiological concepts, creates a perfect logical scheme—a *geometrical* space, different schemes of abstract geometries, the concept of mathematical continuity, and so forth. Poincaré does not attribute any of these abstract schemes to the external world but regards them as having the aim of overcoming the imperfection of sense perceptions.

Thus, over the world of perceptions Poincaré constructs an abstract world, albeit stimulated by the world of perceptions but one that becomes independent of it; the ideal elements of its construction do not have direct analogs in nature.

Intellect constructs a *theory* of phenomena. In Poincaré's conception viewing theory (equals "mathematical physics") as an objective image of the external world is ruled out from the very start. In his view theory can only have the function of *ordering* the obtained perceptions. Poincaré compares this function of theory vis-à-vis the facts of perception with the function of a library catalog: it *gives nothing new*, but through its systematization facilitates the use of facts (Ref. 25a, p. 172, Ref. 25b, p. 160).

It is also important to emphasize other aspects of his understanding of the essence of theory. Thus, the ordering of the facts of experience is not unique; it can be made according to a stipulation, conventionally. On the other hand, experience, regarded as the only source of our knowledge, having stimulated the development of a theory does not in what follows control it; a new experiment is not a criterion for the truth of a created theory, since the introduction of appropriate hypotheses removes the contradiction between theory and the new experiment, preserving the original basis of ideas. And the reservoir of hypotheses is inexhaustible, concludes Poincaré (see Ref. 25b, Ch. 10).

Thus, Poincaré frees theory from the influence of a new experiment. This also precludes the possibility of a qualitative jump in our knowledge of the world.

Assertions relating directly to paths of knowledge in physics are permeated with the ideas of conventionalism. Let us recall the principal ones.

As early as 1898, Poincaré asserted that the constancy of the velocity of light in all directions "can never be directly verified in an experiment", but it does not contradict experiments; such definitions "are merely the outcome of an unrecognized convention".[26] By experiment he understands a limited gedankenexperiment, in which a light signal is sent from the point $A(0)$ to the point $B(x)$ at time $t_1(0)$, and then at the time $t_3(0)$ the signal reflected at $B(x)$ is received at the point $A(0)$.

In this "restricted" single experiment, which eliminates the multitudinous connections that are generalized and verified through theory, a closed logical circle is indeed formed, and it is impossible to establish the constancy of the velocity of light "directly in an experiment", since one cannot establish the time $t_2(x)$ of reflection of the signal at $B(x)$.

What is the meaning of Poincaré's assertion that the proposition concerning the constancy of the velocity of light is not an experimental fact but merely a conventional definition? It means that one could with the same justification adopt a different convention, according to which the velocity of light "there" and "back" changes within definite limits. For the empirical experiment, which above we have described as "restricted", Reichenbach derived an expression for the time of reflection of the light from the mirror at $B(x)$:

$$t_2(x) = t_1(0) + \varepsilon\,[t_3(0) - t_1(0)],$$

where ε is a "coefficient of conventionality", which varies in the range $0 < \varepsilon < 1$.

The constancy of the velocity of light in all directions assumed by Einstein corresponds to the single value $\varepsilon = \frac{1}{2}$ of this coefficient. Therefore, for all other values of ε permitted in accordance with Reichenbach's formula we would obtain certain different "variants of the theory", but not the "variant" that we know as the result of Einstein's work.[21)] Would physicists really agree to regard Einstein's theory of relativity as a special case of a conventional stipulation, distinguished among a lar-

---

[21)] We recall that among all the possibilities, Reichenbach also admitted the variant in which a unique time, and with it the classical transformation, is preserved for all inertial systems (see the earlier discussion).

ger class of "equivalent theories"? But this is the drift of Reichenbach's formula.

Of course, if we approach the matter properly, we readily see that Reichenbach's formula is not a physical fact but merely a mathematical algorithm, from which it follows that under the condition $t_1(0) < t_2(x) < t_3(0)$ the variable parameter ε will vary in the interval $0 < \varepsilon < 1$. This is unquestionably a mathematical fact. But it does not have any bearing on Einstein's theory of relativity.

Historical analysis also shows that the experiments of a developing science by no means reduce to a single "restricted" experiment, which is limited to the transmission and reception of a reflected signal at *one and the same point* of space, and we have attempted to show in this paper how comprehensive experimental foundations are generalized in the basic principles of the theory of relativity.

Moreover, if we were to accept the conventional method of Poincaré and Reichenbach and accept in the bases of the convention all other values of ε permitted by Reichenbach's formula, we would see that all of them (except $\varepsilon = \frac{1}{2}$) lead to inequality of the velocity of light in opposite directions, i.e., to *a contradiction with the experimentally confirmed principle of relativity.*[22]

Let us consider one further example of Poincaré's conventionalistic approach to epistemology. This concerns the relationship between geometry (G) and physics (P).

Already in his early paper "On the fundamental hypotheses of geometry" (1887), Poincaré wrote that "the fundamental hypotheses of geometry are not facts taken from experience" and that "our chosen group [of hypotheses] is merely more convenient than another, and one cannot say that Euclidean geometry is true and Lobachevskii's geometry is false, any more than one can say that Cartesian coordinates are true and polar coordinates false" (Ref. 27, p. 398).

In this statement, Poincaré already puts geometry and a coordinate system in the same category, identifying their functions as means of description. Here, as in many later formulations, Poincaré regards geometry as a conventionally chosen method of description, as *a premise of physical theory completely independent of the properties of the described object.*

In this asserted independence of geometry from the properties of the object lies the justification for the assertion that in a given physical domain one can use any method of description, according to convention, and, say, replace Euclidean geometry by Riemannian geometry, etc. But then, to express the same experiments, one must change the laws of physics; therefore, one could use either $G_1 + P_1$, or $G_2 + P_2$, $G_3 + P_3$, etc.

Thus, Poincaré laid the foundation of the idea that there exists a class of equivalent descriptions of the same experience.[24]

After these considerations of Poincaré, it was not difficult for Reichenbach to go over from the "class of equivalent descriptions" of Poincaré to the "class of equivalent descriptions" of Reichenbach: the entire symbolic formula is retained, and so is the interchangeability of its elements; merely G is replaced by an "unobservable phenomenon", an "interphenomenon" (I); more precisely, by any hypothesis of it. Then the "class of equivalent descriptions" takes the form: $I_1 + P_1$, $I_2 + P_2$, $I_3 + P_3, \ldots$; each equivalent description is a description of one and the same set of perceptions—Reichenbach defines this precisely.

With regard to the interphenomenon (I), Reichenbach proposes that by this we should understand everything that, at least for the time being, is unobservable; for example, if we turn away from a tree, then behind us we should already have an interphenomenon, i.e., something unobservable, and we could with justification adopt any hypothesis: the tree has disappeared, the tree has split in two, in three, and so forth, but then we would be obliged to change physics in order to explain the perception nevertheless of just *one* shadow (see Ref. 28). At the first glance, these arguments appear naive, but Reichenbach proposes to solve for us in the framework of this gnosiology the problem of quantum physics, and it then becomes clear that, in seducing the theoretician with the possibility of admitting any hypothesis about a quantum interphenomenon provided the set of perceptions is explained, Reichenbach incorrectly orients the theoretician with regard to the paths of acquiring knowledge of deep secrets of nature.

And the roots of this gnosiology stem from Poincaré. There are indications that Poincaré's gnosiology has had a certain influence on both philosophers and physicists. Some theoreticians have apparently been enticed by the will-o'-the-wisp of theoretical thought being unconnected with external conditions. Poincaré himself assumed that the intellect manifests itself the more fully, "the more it liberates itself from the tyranny of the external world".[43]

Was Einstein a Conventionalist? What was Einstein's attitude to conventionalism? Some authors assert that Einstein agreed with Poincaré's opinion that the hypothesis of the constancy of the velocity of light "can never be directly verified in an experiment", and that such definitions "are only the outcome of unrecognized con-

---

[22] In fact, this formula has generated a tendency to consider "equivalent forms" of the theory of ralativity with the parameter ε in the interval $0 < \varepsilon < 1$.

[23] Poincaré wrote: "Even our Euclidean geometry is only a kind of conventional language: we could present the facts of mechanics by referring them to a non-Euclidean space, which would be less convenient but just as valid as our ordinary space; the exposition would be more complicated, but would still be possible" (Ref. 25b, Ch. 6).

[24] According to Poincaré, one can retain the same method of description, regarding it as the simplest and most convenient; for example, one can retain Euclidean geometry. Then, if experience changes, one must complicate the physics accordingly in accordance with the scheme: $G_e + P_1$; $G_e + P_2$; $G_e + P_3$; ... , where G_e is Euclidean geometry.

vention". There have even appeared in print assertions to the effect that Einstein was forced to recognize the unavoidability of relying on basic propositions that have a conventional nature. Einstein is reproached for the fact that, recognizing Poincaré's correct point of view, he did not follow it up consistently and did not analyze the extent to which individual assertions of theory depend on the adopted convention.

In Einstein one can indeed find the assertion that from the experiment with the transmission of a light signal and the reception of the reflected signal at the same point one can draw the conclusion of equality of the velocities "there" and "back" only as an "arbitrary assumption". In particular, he expressed this thought in his lecture to the Society of Natural Scientists at Zurich in 1911. However, a careful and unprejudiced analysis of this lecture shows that Einstein by no means shared Poincaré's point of view concerning the conventional nature of the basic propositions of theory. In that lecture, Einstein intended to demonstrate the unusual situation in the problem of the electrodynamics of moving bodies: the existence in each inertial system of a corresponding set of variables (consequence of the principle of relativity); the breakdown of the classical law of composition of velocities, which appeared to lead to a "terrible conclusion"—the refutation of the principle of relativity, whereas the classical law of composition of velocities was itself a consequence of an "arbitrary assumption" about physical concepts, above all that of absolute time; among other things, he pointed out that from the limited ("restricted") experiment the equality of the velocities in opposite directions can be assumed only as an arbitrary assumption, which is of course beyond doubt. In a word, Einstein aimed to show that the new situation had led to unusual conclusions, had revealed a number of "arbitrary assumptions" (old and new, real and imaginary), and had turned kinematics "upside down". "How can we put kinematics back on its feet?" asked Einstein. And he answered: "The answer presents itself: precisely those circumstances that caused us earlier such difficulty now lead us to the correct path once we have obtained a greater freedom of action, *having given up those arbitrary assumptions*. It turns out that those two seemingly incompatible postulates, *to which experiments lead us*, namely, the principle of relativity and the principle of the constancy of the velocity of light, lead to a completely definite solution to the problem of the transformation of the coordinates and the time" (Ref. 13, p. 183 of the Russian translation; our italics).

In a footnote at this point, Einstein states: "If x, y, z, t and x', y', z', t' denote the space-time coordinates in two frames of reference K and K', then these two fundamental principles require that the transformation equations be such that each of the two relations $x^2+y^2+z^2=c^2t^2$ and $x'^2+y'^2+z'^2=c^2t'^2$ be a consequence of the other. Since for reasons that we shall not explain here the transformations must be linear, it follows from a brief investigation that the transformation law is thereby established" (Ref. 13, p. 183; our italics).

The picture Einstein sketched in the lecture of the birth and development of the ideas leading to the formulation of the theory of relativity is completely clear, and it is already familiar to the reader from the first 1905 paper. This picture has no place for Poincaré's conventionalistic ideas, and the need did not arise to analyze the consequences of the conventions adopted in this conception.

A. Grunbaum, professor of philosophy at the University of Pittsburg, grossly distorts Einstein's standpoint with regard to conventionalism. He directly ascribes to Einstein a conventionalistic thought: "You can always keep the geometry you like by an appropriate alteration in the correcting physical laws associated with it". Grunbaum does not give any references from which one could deduce that Einstein actually expressed such a conventionalistic thought. That remains an unfounded guess of Grunbaum. Instead, he quotes verbatim from Einstein's Autobiographical Notes the hypothetical polemic between Poincaré and Reichenbach. The reader will recall (from the earlier passage) that Einstein imagined this polemic in order to say that the conventionalism of Poincaré differs little from Reichenbach's (this was also recognized by Carnap[30]). But Grunbaum announces that he has (??) "the right to replace the name *Poincaré* in the Einsteinian dialogue by the names *Duhem* and *Einstein,*" which he does throughout the whole of the quoted dialog thus identifying Einstein's views with those of Poincaré and Duhem.[29] [25)]

However, an arbitrary change of the names in the dialog proves nothing. We must return to the most reliable source and consider the actual place alloted by Einstein to geometry in his theoretical investigations, namely, in his work on the general theory of relativity, in which this question became particularly acute.

On the Role of Geometry in Einstein's Physical Investigations. The physical ideas that provided the foundation of the general theory of relativity were formulated by Einstein immediately after the creation of the special theory of relativity. Already in 1907, analyzing physical processes in a noninertial system, he established that in these systems one cannot adopt the definition of the measurement of time (simultaneity) that he had given in the case of inertial systems, since in a noninertial system the time changes from point to point, depending on the acceleration γ in the given element of space ξ, i.e., on the gravitational potential $\Phi=\gamma\xi$.

Later (1918), Einstein noted that the main difficulty in the mastering of the general theory of relativity is probably to be found in the circumstance that in the general theory of relativity the connection between the quantities *occurring in an equation* and *measured* quantities (Einstein's italics) is much more indirect than in ordinary theories (see Ref. 13, p. 621 of the Russian

---

[25)] The author of the concluding remarks, É. M. Chudinov correctly notes that the reader "will detect serious distortions of his [Einstein's] philosophical conception. If we are to believe Grunbaum, then Einstein was an extreme conventionalist of the Duhem type ..." (Ref. 29, p. 550 of the Russian translation).

translation). An interesting remark directed against empiricists and operationalists!

Thus, for noninertial systems, Einstein found that

$$\sigma = \tau\left(1 + \frac{\gamma\xi}{c^2}\right) = \tau\left(1 + \frac{\Phi}{c^2}\right)$$

(here, σ is the "local time" of a point event in the element of space ξ, and τ is the time of the point event at the coordinate origin). Investigating then the influence of the gravitational field on electrodynamic processes, Einstein naturally found that the wavelength of light is also a function of the gravitational potential and changes from point to point; in particular, it followed from this that the wavelength of light emitted by atoms on the surface of the Sun at a point with potential Φ is accordingly greater than the wavelength of light emitted by the same atoms on the surface of the Earth; as a result of this, there must be a shift of the spectral lines ("red shift").

Making then a number of transformations, which take into account the new metric in the accelerated system, Einstein arrives at the equations of electrodynamics in this system, which have the same form as in an unaccelerated system with however the difference that all the electrodynamic variables are multiplied by $(1 + v\xi/c^2) = (1 + \Phi/c^2)$, and the velocity of light c is replaced by c', which depends on the gravitational potential: $c' = c(1 + \Phi/c^2)$, i.e., the velocity of light is here not a constant quantity.

From this there followed a new phenomenon: a light ray must be bent in a gravitational field.

All these previously unobserved phenomena were predicted by Einstein already in the 1907 paper (see Ref. 13, p. 65 of the Russian translation) (experimentally, they were confirmed partly only in 1919—the bending of light—and partly even later).

One can say that the need for the transition to a generalized theory (the general theory of relativity) was already recognized by Einstein at that period, but he still faced great difficulties associated with the formulation of the equations of gravitation, in which it was necessary to express generalized physical laws in a specific form. An entire decade was needed for the solution of this difficult problem.[42]

Of course, this entire pilgrimage is well known to physicists. However, it is important to emphasize that, in determining the physical foundations of the generalized theory, Einstein neither in this nor his subsequent papers posed a preliminary question of which geometry he should use as a basis for his investigations—Euclidean or Riemannian—as a *method of description chosen conventionally*. There was no preliminary *choice* of the method of description *in accordance* with a convention, i.e., there was no choice of a particular geometry; rather, there was actually an investigation of the physical connections between electrodynamic and gravitational fields that occur under definite conditions. The possibility of the metric actually changing in an investigated region was discussed by Einstein explicitly much later—not, it seems, earlier than 1914–1915—when he was already on the point of formulating generally covariant equations for physical processes in the general theory of relativity, i.e., when he began to employ in this theory the mathematical (tensor) formalism. Considering how accelerated systems influence the results of linear measurements and the running of clocks at different points of physical space, Einstein found that a configuration of effectively rigid bodies in a rotating system "is described by Euclid's geometry inaccurately and that the rate of clocks is a function of position. In other words, in the general theory of relativity *there does not exist a geometry or kinematics independent of physical processes*, since the properties of rods and clocks are determined by the gravitational field" (Ref. 13, p. 424; our italics).

Thus, in his physical investigations Einstein assumed that the geometry depends directly on physical processes, and therefore *it is not a method of description chosen conventionally*, to which physics must be adapted. "From this point of view the laws of geometry must be regarded as integral physical laws," wrote Einstein in 1914 (Ref. 13, p. 379 of the Russian translation).[26)]

We see that after the first step—the assertion of a relational status of space and time—Einstein made the second, natural, step and discovered the metric as the expression of objective physical connections.

Einstein: "Geometry and Experience". Although in the development of the general theory of relativity Einstein arrived at the conclusion that the laws of geometry must be regarded as integral physical laws, for a long time he did not explicitly confront his views with the conventionalism of Poincaré. But Poincaré's conception had a strong bearing on the methods of natural scientists in general and Einstein's method in particular.

In 1921, Einstein felt obliged to give an account to the Prussian Academy of Sciences of his understanding of the relationship between geometry and physics in a lecture entitled Geometry and Experience. In this lecture, Einstein considered the widely held view of geometry as an absolutely precise science that dictates its logical schemes to physics and bears no relation to experience, although its very name indicates its experiential origin.

According to Einstein, this situation is explained by the axiomatic method of constructing geometrical schemes. This method is based on the a priori determined nature of the logical connections of arbitrarily adopted axioms after the manner that, if this is true, then so is this. In such a geometry, neither the premise nor the conclusion are governed by experience. And although the scheme uses "geometrical" concepts such

---

[26)] It is not superfluous to remark that Einstein also considered Euclidean geometry from the same point of view: he wrote: "It has been forgotted that Euclidean geometry, in the form that it is used in physics, also consists of physical assertions, which, from the physical point of view, were established from the integral laws of Newtonian point mechanics" (Ref. 13, p. 379).

as point, line, plane, and so forth, the axiomatics does not embody any definite meaning in these concepts. "Neither knowledge of these objects nor conceptions about them are presupposed but only the validity of axioms of a purely formal nature, i.e., devoid of all perspicuous and vital content, as in the example given above. These axioms are free creations of the human mind. All the remaining theorems of geometry are logical consequences of these axioms (and do not have a real prototype)" (Ref. 14, p. 84 of the Russian translation).

Thus, axiomatic geometry is only a logical scheme with no bearing on content. "But if we deny the connection between a body of axiomatic Euclidean geometry and a real, effectively rigid body, we readily arrive at the point of view adhered to by that original and deep thinker Henri Poincaré: Euclidean geometry differs from all possible axiomatic geometries by its simplicity" (Ref. 14, p. 85). But, as we have seen, we must pay for the simplicity of the geometry by the greater complexity of the physics. "Thus," summarizes Einstein the views of Poincaré, "one can arbitrarily choose both (G) and some parts of (P); all these laws are conventions. To avoid contradictions, it is then necessary to choose the remaining parts of (P) in such a way that (G) and the complete (P) are together confirmed in experiments. According to this opinion, axiomatic geometry is from the point of view of epistomology equivalent to making some of the laws of nature into conventions" (Ref. 14, p. 86).

"Sub specie aeternitatis hat Poincaré mit dieser Auffassung nach meiner Meinung recht," continues Einstein: "In my opinion, this view of Poincaré is correct sub specie aeternitatis" (i.e., from the point of view of immutability of standpoint, abstractness, and isolation from the concrete consideration of connections).[27)]

Because of this, he regards the use of axiomatic geometry in physics as an error, pointing out that the physicist does not deal with axiomatic but *practical* geometry. "It is clear that from the system of concepts of axiomatic geometry one cannot draw any conclusions about the actually existing objects that we call effectively rigid bodies. To make arguments of this kind possible, we must deprive geometry of its formal and logical nature and confront the real objects of our world with the empty scheme of concepts of axiomatic geometry" (Ref. 14, p. 85).

Einstein carries out this operation by confronting the behavior of "effectively rigid bodies" with Euclidean geometry as a whole: "In regard to the different possibilities of configuration, rigid bodies behave like the bodies of Euclidean geometry of three dimensions" (Ref. 14, p. 85). The assertions of practical geometry "rest essentially on conclusions drawn from experience and not only on logical conclusions". Without this understanding of geometry, says Einstein, "I could not have established the theory of relativity. Namely, without it, the following consideration would have been impossible: in a frame of reference that rotates with respect to some inertial system, the laws of configuration of rigid bodies do not correspond to the rules of Euclidean geometry because of the Lorentz contraction; thus, if we permit noninertial systems on an equal footing, we must give up Euclidean geometry" (Ref. 14, p. 85).

But in axiomatics it is asserted that in the real world there are no objects exactly corresponding to the concept of a rigid body or a clock—the concepts employed in the theory of relativity. We see how persistently this idea is put forward: theories can be created only after *agreement has been reached* as to the meaning that must be attached to any particular basic concept. To this, Einstein gives a deep answer. Yes, there are no such ideal rigid bodies or clocks. "It is also clear that rigid bodies and clocks are not primary concepts but complex concepts that cannot play an independent part in theoretical physics" (Ref. 14, p. 86).

Here, the view that concepts and theory are interconnected is expressed explicitly for the first time[28)]; we already know from Heisenberg's information that, five years later in a discussion with him, Einstein expressed similar ideas, while at the end of the forties, in the answers to Bridgman and Reichenbach, Einstein gave his most developed formulation of the idea.

However, in 1921 in his arguments against the axiomatic approach he still attempted to go in another direction. Although it is true that the concepts of a rigid body and a clock do not have independent significance outside theory, "we are still very far from secure knowledge of the theoretical foundations of atomistics, so that we cannot specify the precise theoretical structure of these formations;" therefore, we are forced to use them as independent concepts. What meaning can we place in them?

In reality, absolutely rigid bodies do not exist. But there do exist properties of real bodies that are sufficiently well defined for us to use these bodies as effectively rigid. These properties are expressed in the following principles of practical geometry, which have experimential origin: "a) two intervals are said to be "equal to each other" if the ends of one interval can be kept next to the ends of the other for a long period, b) if two intervals at a given time and at a given place were equal, then they will be equal always and everywhere".

These principles have universal significance. "Not

[27)] As follows from the complete text and Einstein's subsequent conclusions (see the following quotation in the main text), the quoted phrase cannot be rendered by the words: "In my opinion, this standpoint of Poincaré *is completely correct from the fundamental point of view*," as it was translated in the publication of Nauchnoe Knigoizdatel'stvo (Scientific Book Publishing House) in 1922 and was automatically transferred to Ref. 14, p. 86 of the Russian translation. Without access to the original and trusting this translation, the present author somewhat inaccurately, in the spirit of the translation, explicated Einstein's position on this question in Ref. 31; our italics.

[28)] The connection was already implicit in the 1905 paper.

only practical Euclidean geometry but also its direct generalization— practical Riemannian geometry, and with it the theory of relativity—rest on this assumption" (Ref. 14, p. 87).

On the basis of these principles and using the phenomenon of the propagation of light in empty space, which relates intervals of time and distance, one can also arrive at similar conclusions for intervals of time measured by clocks: "...If two ideal clocks at some instant of time and at some place run at exactly the same rate (and they are in the immediate proximity of each other), then they will always have the same rate irrespective of where and when (at the same position) they are compared" (Ref. 14, p. 87).

Einstein emphasizes that this is not a conventional nor a fantastic proposition. "If this proposition were not satisfied for clocks in nature, the intrinsic frequencies of atoms of the same element would not agree with one another to the accuracy that is demonstrated experimentally. The existence of spectral lines is a convincing demonstration of the correctness of the above principle of practical geometry. Ultimately, it is this that is the justification for the possibility of intelligent statements about a metric in the sense of a four-dimensional Riemannian space-time continuum".

We cannot omit noting here the deep analysis with which Einstein approaches experimental facts whose meaning was, it would seem, already grasped.

The final answer which Einstein gives Poincaré is clear: "According to the view advanced here, *it is a question of physics whether this continuum has Euclidean, Riemannian, or some other structure,* and the answer must be given by experiments; it is not a question of a choice based on pure expediency" (Ref. 14, p. 87; our italics).

Four years later, Einstein published his paper "Non-Euclidean geometry and physics". In this too he criticizes the conventional approach of Poincaré to the choice of geometry and Poincaré's preference for Euclidean geometry as the simples, and defends his view of practical geometry "as corresponding best to the present state of our knowledge".

We again see that the method of conventional definitions was foreign to Einstein.29)

"The Honor Left to Einstein". Poincaré Silence. We now return to the questions posed at the beginning of this section.

Why did not Poincaré complete the creation of the theory of relativity but "left this honor" to Einstein?

Because the theory of relativity, like any physical theory, could be constructed only as a theory reflecting the real connections of nature at all stages of its establishment—from the basic principles to the verification of the consequences. But a scientific theory can only meet the task of reflecting the laws of nature if it is invariably based on information that the theoretician extracts from the interconnection with nature itself; it is *rigidly connected to this information.*

Poincaré did not regard theory as reflection of an external world; in his view, it is a system for ordering perceptions which can be realized in any form, by convention. Already in 1908, Lenin, criticizing the positivistic views of Poincaré, wrote: "For Poincaré [...] the laws of nature are symbols, convention, which man creates for *"convenience"*. "The only genuine objective reality is the internal harmony of the world", with Poincaré regarding as objective that which is generally comprehensible and cognizable to the majority of people or to all of them,[32] i.e., in a purely subjectivist manner annihilating objective truth, like all Machists...".[33] In giving this evaluation, Lenin also referred to the evidence of the well-known neopositivist P. Frank, who asserted that for Poincaré "many of the most general propositions of theoretical natural science (the law of inertia, the conservation of energy, etc.)" belong neither to the propositions of empirical origin nor a priori origin, "being purely conventional premises dependent on human judgment" (see Ref. 33). In spirit close to Poincaré, Frank rejoices that the most modern natural philosophy "revives in an unexpected manner the fundamental thought of critical idealism, namely, that experience only fills the frame that man himself creates" (see Ref. 33).

Why do we recall these philosophical views of Poincaré and their criticism by Lenin if we are only concerned with Poincaré's relationship to Einstein's theory of relativity? Because in philosophy Poincaré was a *militant* thinker: he always set himself the task of interpreting the results of physical investigations with a view to confirming his philosophical conception. The conventionalistic gnosiology prevented him from generalizing the results of the investigations of electrodynamics of his time and completing this generalization in a subsequent theory reflecting the laws of the external world. Essentially, this is admitted by de Broglie in his speech quoted earlier, in which he said that Poincaré, being a pure mathematician, "adopted with regard to physical theories a somewhat sceptical position, assuming that in general there exist infinitely many logically equivalent points of view and pictures of reality, from which scientists, being guided exclusively by considerations of convenience, choose a particular one. It is probable that this nominalism once again prevented him from recognizing the fact that among logically possible theories there are some that are closer to

---

29)It is here worth adding that already in 1835 Lobachevskiĭ wrote that the vain efforts over two thousand years to prove the theorem of parallels forced him "to suspect that the concepts themselves do not yet contain the truth which it was desired to prove and which can be verified, like other physical laws, only by experiments such as, for example, astronomical observations" (Ref. 27, pp. 61, 62). Further,: "...some forces in nature follow one particular geometry and others another" (Ref. 27, p. 64). Riemann (1866) expressed similar views to the effect that the properties of space "cannot be gleaned other than from experience" (Ref. 27, p. 310). Gauss had similar views.
In his understanding of the dependence of geometry on real physical laws, Einstein was closer to Lobachevskiĭ and Riemann than to Poincaré.

physical reality, or at least agree better with the intuition of the physicist and can therefore be of greater assistance to him".[22] In 1955, repeating this argument, Louis de Broglie added: "If this point of view is correct, it was precisely this philosophical inclination of his intellect to "nominalistic convenience" [and this after all is the position of conventionalism] that prevented Poincaré from grasping the significance of the idea of relativity in all its grandeur!".[34]

And this is true: Poincaré did not comprehend the scale and realistic nature of Einstein's work. And his silence was not due to the fact he was unsatisfied by the external, allegedly insufficiently simple and insufficiently flexible form of Einstein's theory. It seems to us that Poincaré's silence in the first years after the publication of the first and basic paper of Einstein was explained by the fact that it was not clear to Poincare how and whether he could interpret Einstein's theory in the light of his own conception. He could neither accept nor reject it fully. This was a collision of two methods in the development of concrete physical problems in which conventionalism suffered a defeat. It was only after a seven-year silence, shortly before his death, that Poincaré in 1912 prepared his paper "Space and time", in which he discussed the question of whether "Lorentz's principle of relativity" necessitates a change in attitude to his previously made assertion that "geometry must be a convention, and the principle of relativity must be regarded as a convention". And, posing the question: "What is to be our attitude to these new ideas? Do they force us to change our conclusion?", Poincaré answers: "No: we adopted a certain convention because it appeared to us convenient, and we said that nothing forces us to abandon it. Now some physicists wish to adopt a new convention. This does not mean that they are forced to do this; they regard the new convention as more convenient, that is all; and those who do not adhere to this way of thinking can perfectly well retain the old way so as not to disturb their old customs. Among ourselves, I believe that they will continue to do this for a long time".[35]

And this was all that the great mathematician could say about the new physical ideas (which he did not even attribute to Einstein but to Lorentz). Even at that time this sounded like a feeble defense of the gnosiological standpoints of conventionalism under attack from the developing physics; seen historically, the inadequacy of such a conclusion has become even more obvious. Not Poincaré's conventionalistic line offered promise but Einstein's realistic line.

V. ON DIFFERENT FORMS OF DESCRIPTION

As we have said above, the conventionalists assert that the form of description of an experiment can be chosen by convention, and that the physics must be adapted to this form. It seems to me that in the literature the question of "equivalent" descriptions has been treated too schematically; it requires discussion.

On Identical Descriptions on the Basis of One-to-One Correspondence. A perspicuous demonstration of how different methods of description affect the content of physical laws is sometimes given by means of the graphical method (see, for example, Ref. 30).

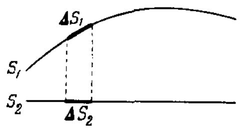

FIG. 1.

Figure 1 shows part of an arc S_1 and an interval of a straight line S_2. Suppose that from each point of the arc S_1 a perpendicular is dropped onto the straight line S_2. It joins two corresponding points on the two lines (one-to-one correspondence). Let us follow the motion of an infinitesimally small interval ΔS_1 from left to right and consider the nature of the motion of its projection ΔS_2. Suppose we know the physical law of motion of ΔS_1: its length remains unchanged. How will the moving projection ΔS_2 change? Initially it will become longer, and then shorter. Suppose that the fact of motion of the segment along the arc S_1 means that a physicist uses one method of description, for example, Riemannian geometry, while motion along S_2 means that he uses a different method of description, Euclidean geometry. Then for the projection, the physicist writes down a different law of motion: he introduces into his expressions "correction terms", forces that first stretch and then contract the segment. The geometry on S_2 is simpler than S_1, but the physical laws are more complicated. It is this that Poincaré asserts: the laws of physics depend on the chosen method of description. And the method of description chosen by the physicist is a matter of convention.

It might appear that the logic of these arguments is irrefutable and that one can adopt the idea of a class of equivalent descriptions. However, one can draw attention to their abstract schematic nature. As Einstein noted, as we have seen earlier, such arguments of Poincaré are true abstractly—sub specie aeternitatis—from the point of view of eternity.

But the actual process of cognition is not restricted to a definite level, it is steadily being deepened. This means that if one were to retain the simplest method of description (G) it would be necessary to add ever new "correction terms", extending to infinity. Then the process of cognition will resemble Lorentz's method of accumulation of hypotheses, in which each new discovery that disturbs the original scheme of concepts is explained by the introduction of new hypotheses.

The advance of knowledge can, quite generally, destroy the arguments of the abstract one-to-one correspondence between two descriptions: at some stage in the acquisition of knowledge there may be discovered a discrepancy between the consequences of two descriptions, and it then follows that the two methods of description are not identical at all but that one of them penetrates deeper into the essence of nature. To show that this occurs in the history of man's acquisition of knowledge, we may quote a convincing historical example. In his critical and historical account of the Science of Mechanics[36] (highly regarded in its time by leading scientists, including Einstein), Mach put forward throughout the entire book the idea that all the principles of mechanics—Newton's principle, the principle of virtual displacements, Hamilton's principle, and so forth—are equivalent, since by means of any of them one can solve the problems of mechanics. And indeed, at a definite stage of knowledge this conclusion was possible, but only because and as long as Mach forcibly reduced all problems of mechanics to a single problem—the interaction of bodies producing accelerations in each other inversely proportional to their masses. But the development of our knowledge has shown that the problems of mechanics are more complicated, and that Hamilton's principle penetrates into the essence of nature much more deeply than the other principles of mechanics: although it was formulated first in the domain and in the language of mechanics, it also enables one to solve problems of atomic physics.

The reader is probably familiar with not a few such transformations of concepts, of deepening of "methods of description", and the relative and historical nature of the equivalence of different methods of description; below, we shall give some more examples.

What we have said means that different methods of description of the same experience should be evaluated not in the light of a static state of knowledge, as was done by Poincaré and Reichenbach, but in the light of *the development of knowledge,* in the light of an analysis of the connection of a particular method of description with the preceding knowledge and, what is no less important, also in the light of the *prospects* of the given method of description for subsequent deepening of knowledge. And these prospects are revealed only when the method of description has an objective basis.

On Different Methods of Description Having an Objective Basis. A one-to-one correspondence can be realized by the replacement in all equations of all variables by linear functions of them. Widely known examples of such substitutions are the transitions from one coordinate system to another (for example, from Cartesian to polar or curvilinear coordinates, etc.) or from one system of units to another.[30)] Such operations modify the superficial form of the equations and, in a number of cases, may even simplify the calculations, but they do not uncover new connections in nature nor do they lead to a deeper level of knowledge. Attempts to present such transformations as an innovation in science call forth perfectly justified objections (see, for example, the paper of Kadomtsev *et al.*[37]).

But in physics one can encounter examples of the coexistence of different forms of description, which, however, are not obtained by the method of one-to-one correspondence but as a result of different and independent lines of discovery. Let us consider examples.

The first example is provided by the matrix and wave aspects of quantum mechanics. Each of these aspects has its foundations in quantum phenomena, in their dual nature, and neither is an object of a conventional stipulation; they are not identical, as some authors suppose, since one aspect characterizes the change in the state of a system for constant dynamical variables, while the other characterizes the change of the dynamical variables for a constant state (see, for example, Ref. 38); but the two aspects are interconnected, as is revealed in the generalized theory of operators.

Second example: There exist two forms of description of Raman scattering of light—the classical and the quantum. The classical theory was developed by G. S. Landsberg and L. I. Mandel'shtam; it is based on the classical concept of the modulation of the scattered light by the vibrations of atoms in molecules or at the sites of a crystal lattice. This theory does not give the correct relationship between the intensities of the Stokes and anti-Stokes components of the original spectral line or their dependence on the temperature. The quantum theory (the necessity of which was immediately pointed out by Landsberg and Mandel'shtam) is based on objective but much deeper properties of the emitting systems; it is valid for all temperatures, including low temperatures.

It is already clear from these examples that in science one can encounter different forms of description of the same body of phenomena, but this is not an argument in favor of conventionalism, since these descriptions are not identical in their essence and differ in that either they reflect complementary aspects of reality that cannot be reduced to each other, or one of the descriptions reflects a deeper level of penetration into the nature of the phenomena, which need not be revealed immediately.

Seen in this light, searches for different forms of description are natural, unavoidable, and desirable. They reflect the multitudinous properties of nature and the possible connections with it, and facilitate the discovery of optimal paths of investigation. But this imposes an obligation on the theoretician to seek, in each case, the objective properties that provide the basis of the given form of description. A painstaking analysis of these properties is an important task of the theoretician. It is the unavoidable path to the establishment of the limits of applicability and potential possibilities of each of the parallel descriptions. And it is the path to generalizing theories.

Einstein and the Problem of Different Forms of Description. It is natural that Einstein, who made revolutions in the development of science, should have been

---

[30)] Poincaré used the possibility of such substitutions as an argument in favor of conventionalism (see, for example, Ref. 25b, p. 152). However, this possibility cannot serve as an argument. For the use of a particular system of units (or a system of coordinates) does not change the relationships in the actually measured object and merely has the consequence that the description of the objective connections contains certain dimensionless coefficients which characterize the relationships between the different units of measurement (or coordinates). Poincaré incorrectly transferred his arguments to geometry as well. But a particular geometry reflects a corresponding metric, *which is realized in the object itself,* and the use for the description of its connections of an a priori chosen geometry with an *arbitrary metric* cannot reveal objective connections. Conventionalism as a gnosiology does not find confirmation.

deeply concerned with the problem of the possibility of different descriptions of the same facts. Let us consider what he wrote on this subject.

"For the same complex of experimental facts there may exist several theories that differ significantly from one another. But with regard to the conclusions from the theory that are amenable to experimental verification the agreement between the theories may be so complete that it is difficult to find conclusions in which these theories differ.... An example of a far reaching coincidence of the consequences of two theories is encountered in Newtonian mechanics, on the one hand, and in the general theory of relativity on the other. This coincidence goes so far that to the present time we have succeeded in finding only a few consequences of the general theory of relativity amenable to experimental verification that do not already follow from prerelativistic physics; and this is so despite the deep difference in the basic propositions of the two theories" (Ref. 13, pp. 593–594). It is to be recalled that Einstein wrote this in 1917, when the general theory of relativity had already been created but none of its consequences had yet been confirmed experimentally [except, of course, the perihelion advance of mercury; translator].

Let us emphasize the situation pointed out by Einstein: 1) relativity theory predicted all the consequences verified experimentally for Newtonian theory; but Einstein did not draw from this the conclusion that the two theories are identical; 2) in contrast, he was convinced that the "deep differences between the fundamental premises of the two theories" must lead to differences in the consequences; 3) nevertheless he expressed disappointment that it was possible to find only a few consequences of the general theory amenable to experimental verification—which had not yet even been realized.

This seems to have disturbed Einstein to some extent, since even earlier he had noted: "In striking contrast to the deep change made by the general theory of relativity in the foundations of physics is the negligible difference between the quantitative predictions of the new and the old theory" (Ref. 13, p. 424).

But of course, there were no grounds for disquiet. That at that time he "could find" only the three well-known consequences predicted by general relativity (later, they were all confirmed experimentally) is not surprising, since this was due to the level and capability of astrophysical investigations at that time.31) With regard to the small number of new predictions, this fact is not of fundamental significance; it is already sufficient to show that the two theories are not only not identical in their premises but also in their consequences.

However, the general theory of relativity still had to encounter a new test in a confrontation with ideas which have their roots in Poincaré's conception and in his assertion that geometry is not only independent of physics but that the conventional choice of geometry determines the laws of physics, and that at all levels of cognition one can (and indeed it is desirable to do so) retain Euclidean geometry and flat space.32)

We are referring to the attempts to show that the relativistic phenomena of gravitation can be reproduced not only by the general theory of relativity but also by another theory—the relativistic theory of gravitation in flat space developed by Thirring, and also by other investigators.[39] Some authors assert that the two theories are completely identical with regard to all observable effects. Indeed, Thirring and other theoreticians have shown that some important effects (for example, the bending of a ray of light and the red shift in a gravitational field) can be represented as effects in a flat space, just as well as they can in general relativity, which introduces the curvature of Riemannian geometry.

It is not without interest to note that Einstein also considered the possibility of preserving Euclidean geometry; for this, it would be necessary to obtain equations for physical processes with allowance for the fact that an interconnection between the g_{ik} and the gravitational field is not presupposed. But this would lead to requirements by means of which "the laws of Euclidean geometry would be reduced to differential equations; however," concludes Einstein, "in such a formulation of the essence of the matter one feels that from the point of view of a consistent implementation of the theory of action at short range this possibility is by no means the simplest and the most obvious" (see Ref. 13, p. 379 of the Russian translation).

This supposition of Einstein is confirmed by the fact that the attempt to create a relativistic theory of gravitation in flat space was realized only almost half a century after the formulation of the general theory of relativity, and, as we shall see in what follows, it, naturally, turned out to be restricted and can be regarded only as an approximation with no pretence to more.

As is noted by Zel'dovich and Novikov in their book of Ref. 40, the abandonment of the idea of curvature of space-time leads to the need to make physically unjustified assumptions (a change in Maxwell's equations in vacuum and, accordingly, abandonment of the proposition of the constancy of the velocity of light in vacuum; the introduction of an absolute time that cannot be observed in any experiment, and so forth).33)

And the most important thing from the gnosiological point of view is that the attempt to create a theory of gravitation in flat space, leading to identical results in the simplest problems, is futile for the solution of new and more complicated problems in cosmology, where the investigator must deal with strong fields, gravitational mass defect, and other complicated problems.

---

31)It is well known that by the middle of the seventies astrophysics had been enriched with many major discoveries, from which physicists await new confirmation of the ideas of the general theory of relativity.

32)We recall that concerning space and time Poincaré asserted directly: "It is not nature that imposes them on us; we impose them on nature because we find them convenient ..." (Ref. 32, p. 7).

33)For details, we refer to the quoted book.

Whereas the two theories may be regarded as competitors in the solution of very simple problems, and conventionalists may regard the giving of preference to one or the other as a matter of convention, these arguments fail when more complicated problems are encountered.

Thus, two systems of description of the same results of an experiment may appear equivalent (and even identical) at some initial stage of knowledge, but further development can reveal their difference, and show that one has more prospects for further development than others.[34)]

VI. SUMMARY

Einstein as the Author of the Theory of Relativity. What general conclusions can we draw from our review of what Einstein did in developing the ideas of relativity? They are as follows.

1. Einstein gave the theory of relativity both its justification and its interpretation, and found its most important consequences. His role does not reduce to that of placing the crowning stone in an arch constructed by predecessors. He constructed the theory of relativity himself from the foundation on the basis of a clarification of the existing interpretations of Maxwell's electrodynamics and of the experimental foundation of the principle of relativity and the discovery of its deep physical content. This content he saw above all in the invariance of the laws of nature in all inertial systems, and, as a consequence of this, in that every inertial system has its own set of variables, these being related to the variables of other inertial systems by definite transformations. Noteworthy in Einstein's appreciation of the significance of the principle of relativity is his reversal of the aim of the investigation—he made the principle of relativity, not the final aim of investigation, but the point of departure and the key to the subsequent generalizations that he realized.

2. Einstein radically altered the method of analysis of factual material and the conclusions of theory; he departed from the classical method of accumulation of hypotheses, insisted on a radical rearrangement of obsolete basic propositions, enhanced the significance of an experimentally verified (adequate) theory (Maxwell's electrodynamics) and with all force emphasized the relational (interconnected) status of all the parameters occurring in a theory, including space and time, with the consequence that space and time lost their status of substantiality and absoluteness. This led Einstein to the understanding that geometry appears, not as a form of description of physical facts of the world that is chosen by convention, but as an important aspect of the physical interconnections in it, as a system of theorems reflecting the metric inherent in the world.

3. Einstein not only gave a physical justification of the theory of relativity and demonstrated its content and its main consequences, but he also represented it as a transitional stage to a theory with broader generalizations capable of dealing with cosmological questions. This breadth of investigations in the activity of a single scientist is unique in the history of physics.

Without the conception and without the inner logic that Einstein gave to the theory of relativity it would have not been possible to create the general theory of relativity nor to elicit the connection between masses, gravitational fields, and radiation, i.e., the breakthrough to problems of cosmology would have been impossible. It is the subsequent development of knowledge, i.e., the creation of the general theory of relativity, that illuminated the meaning and significance of the Einsteinian (special) theory of relativity.

4. What we have said indicates that "indeed Einstein was the author of the theory of relativity in the true sense of the word."[37]

Einstein's entire theory—from the basic, experimentally verified principles to the important practical conclusions—is a logically connected and perspicuous integral whole, and it therefore makes an irresistible impression.

Einstein did not follow a unified philosophical system, and he even assumed that such was impossible for a natural scientist (Ref. 1, p. 307 of the Russian translation). And, undoubtedly, in his sporadic philosophical statements one encounters dubious arguments about the intellect as "free play with concepts", the meaning of which supposedly consists in the attainment by means of this play of the "possibility of ordering sense perceptions" (Ref. 1, p. 261 of the Russian translation).[35)] Such statements of Einstein do not differ from the arguments of many great scientists, in particular Poincaré.

But Einstein himself suggested that one should not listen to what the scientists themselves say about their method but rather study their actions (Ref. 1, p. 181 of the Russian translation). This we have attempted to do in tracing Einstein's path leading to his creation of the theory of relativity. And we see that, as a natural scientist, Einstein referred the results of his investigations to an external world, to nature, in learning about which he saw his direct task. As we have seen, he subordinated his theoretical investigations to this task. And it is not surprising that this position of the natural scientist, passionately wishing to uncover the secrets of nature, led him to criticize conventionalism and its methods. Here, we must specially emphasize that Einstein criticized Poincaré's conventionalism, not from the standpoint of philosophy, but from that of physics, since he clearly understood that Poincaré's convention-

---

[34)] It is noteworthy that Einstein evaluated physical theories precisely from the point of view of their potential for the further development of knowledge. It is appropriate to recall his words said in connection with his evaluation of the connection between the special and the general theory or relativity: "The finest fate of a physical theory is to point the way forward to the creation of a new and more general theory, in the framework of which it survives as a limiting case" (Ref. 13, p. 568 of the Russian translation).

[35)] For a more detailed exposition of Einstein's philosophical views, see Ref. 31.

alism stood in the way of the development of physics.[36]

However, the deep gnosiological solutions that Einstein found and successfully applied in creating the foundations of the theory of relativity were not fully developed by him in connection with the new problems that arose in the investigation of atomic physics. And here there is a deep contradiction with the remarkably bold and huge contribution to the development of physics that he made by showing that, to the astonishment of leading physicists of the time, light has a quantum structure and, further, in a number of brilliant investigations, that quantum interconnections penetrate deeply into all physical phenomena (theory of the specific heat of solids, the photoelectric effect, the emission and absorption of radiation, and so forth), which stimulated the use of quantum ideas to explain the spectral features in the radiation from atoms.

And despite this, in the quantum domain, he adhered to the classical ideas of an abstract object and its properties and to causal connections as understood classically. This defense of the classical concepts was his form of protest against the new trends that asserted that a physical object reduces to phenomena in an apparatus, and that interconnections in nature have the character of absolute randomness, allegedly eliminating tendencies for regular development of integral systems. But Einstein did not take into account that atomic physics itself provides the ground for the conclusion that the concepts both of a physical object and of interconnections also change, becoming deeper, and losing their character sub specie aeternitatis.

W may surmise that Einstein was prevented from understanding this by the fact that he embraced the dialectical nature of the acquisition of knowledge only spontaneously and also by the fact that he was repelled by many of the arguments of his opponents, which were badly formulated, frequently under the influence of positivistic philosophies (Kierkegaard, Höffding), and were one-sided and exaggerated, and moreover were evolving themselves. It is possible that all this had the consequence that, having done so much for the development of quantum ideas, he refused to participate in the development of quantum theory, despite the fact that scientists turned to him in the critical period in the development of physics.[41] [37] At this period, Einstein was totally absorbed by the idea of encompassing all laws of the world in a unified field theory, and he underestimated the fact that this task could not be fulfilled without the contribution that quantum physics makes to our knowledge of nature.

Because of their depth and objective nature, Einstein's theoretical studies had a huge influence on all sections of physical science—from atomic physics to cosmology. To a large extent, they have determined the level of present technology; in particular, they provided the basis for the development of a very important field of technology—lasers—and, most importantly, opened up possibilities for the solution to an acute problem for mankind, the energy problem of the future, by showing theoretically the possibility of releasing atomic energy.

Einstein contribution to the development of modern physics is so great that his name will forever remain in the history of human civilization.

---

[36] This should be borne in mind by those who assume that there is a physical conventionalism, which is acceptable, and there is a philosophical conventionalism, which lies somewhere beyond the pale of concrete sciences and can be left to be discussed by philosophers.

[37] A. F. Ioffe in a discussion with Einstein (1926) on this question characterized this critical period as follows: "One cannot fail to see the mystical haze obscuring the clear contours of physics; lack of belief in its own strength and a denial of the reality of nature itself is infiltrating science. There is only one way out—Einstein must do his duty and has no right to hide in the abyss of the unified field" (Ref. 41, p. 5).

[1] A. Einstein, Sobranie Nauchnykh Trudov (Collection of Scientific Works), Vol. IV, Nauka, Moscow (1967); the connection between the Russian pagination given in the text to these Russian translations and the original articles is as follows: p. 24: Antrittsrede, Sitzungsber. preuss. Akad. Wiss. 1914, p. 2, 739–742. p. 181; On the Method of Theoretical Physics, Clarendon Press, Oxford (1933). pp. 261, 266, 275–276: Autobiographisches (Autobiographical Notes), in: Albert Einstein–Philosopher-Scientist (ed. by P. A. Schilpp), Evanston (Illinois) (1949). p. 307: *Ibid*, in the article: "Remarks concerning the essays brought together in this cooperative volume," pp. 665–688.

[2] H. A. Lorentz, "Electromagnetic phenomena in a system moving with any velocity less than that of light," Proc. Academy of Sciences of Amsterdam, 6, 1904 (Russian translation published in: Printsip Otnositel'nosti: Sbornik Rabot Klassikov Relyativizma (The Principle of Relativity: Collection of Classic Papers on Relativity), Moscow (1935), p. 39.

[3] H. A. Lorentz, The Theory of Electrons, 2nd ed., 1915, Reprinted by Dover, 1952 (Russ. Transl., Gostekhizdat, M. 1956).

[4] H. A. Lorentz, Astrophys. J. **68**, 350 (1928); quoted in the paper of Ref. 5.

[5] G. H. Keswani, "Origin and concept of relativity," Brit. J. Phil. Sci. **15**, 268 (1964/5) (Part I); **16**, 19 (1965/6) (Part II) (Russian translation published in the book of Ref. 6, p. 262).

[6] Printsip Otnositel'nosti (The Principle of Relativity; Russian collection of papers), Atomizdat, 1973.

[7] Phys. Zs. **2**, 1234 (1910).

[8] M. Born, Physik im Wandel meiner Zeit, Vieweg, Braunschweig, 1959 (Russ. Transl., IL, M. 1963).

[9] M. Born, "Physics and relativity," (Lecture at Bern in 1955 at the International Conference to mark the 50th Anniversary of Theory of Relativity); in Ref. 8.

[10] V. I. Lenin, Poln. Sobr. Soch. T. 29 (Complete Collection of Works), Vol. 29; Filosofskie Tetradi (Philosophical Notebooks), p. 192.

[11] S. G. Suvorov, Usp. Fiz. Nauk **100**, 64 (1970) [Sov. Phys. Usp **13**, 24 (1970)].

[12] J. C. Maxwell, Izbrannye Sochineniya po Teorii Élektromagnitnogo Polya (Selected Publications on the Theory of the Electromagnetic Field; Russian translations), Gostekhizdat, M. 1952.

[13] A. Einstein, Sobranie Nauchnykh Trudov (Collection of Scientific Works), Vol. I, Nauka, M. 1965; the connection between the Russian pagination given in the text to these Russian translations and the original articles is as follows: pp. 7, 8, 21: "Zur Elektrodynamik der bewegter Körper," Ann. Phys. **17**, 891–921 (1905). pp. 36, 38: "Ist die Trägheit eines Körpers von seinem Energieinhalt abhängig?" Ann. Phys. **18**,

639–641 (1905). pp. 65, 69: "Über das relativitätsprinzip und die aus demselben gezogenen Folgerungen," Jahrb. d. Radioaktivität u. Elektronik **4**, 411–462 (1907). pp. 144, 145, 146, 152: "Principe de relativité et ses conséquences dans la physique moderne," Arch. Sci. Phys. Natur., Ser. 4 **29**, 5–28, 125–144 (1910). pp. 175, 179, 183: "Die Relativitätstheorie," Naturforsch. Gesellschaft, Vierteljahresschrift, Zürich **56**, 1–14 (1911). p. 379: "Die formale Grundlage der allgemeinen Relativitätstheorie," Sitzungsber. preuss. Akad. Wiss. **2**, 1030–1085 (1914). p. 386: "Zur Relativitätsproblem," Scientia (Bologna) **15**, 337–348 (1914). p. 424: "Die Relativitätstheorie," in: Die Physik (ed. E. Lechner), T. 3, Abt. e, Bd. 1, Teubner, Leipzig (1915), pp, 703–713. p. 487: "Die Grundlage der allgemeinen Relativitätstheorie," Ann. Phys. **49**, 769–822 (1916). pp. 551, 568, 593, 594: Über die spezielle und die allgemeine Relativitätstheorie (Gemeinverständlich), Vieweg, Braunschweig (1920). p. 621: "Eine Dialog über Einwände gegen die Relativitätstheorie,"Naturwiss. **6**, 697–702 (1918).

[14] A. Einstein, Sobranie Nauchnykh Trudov (Collection of Scientific Works), Vol. II, Nauka, Moscow (1966); the connection between the Russian pagination given in the text to these Russian translations and the original articles is as follows: p. 24: The Meaning of Relativity, Princeton Univ. Press, Princeton, New York, 1921. p. 84: "Geometrie und Erfahrung," Sitzungsber. preuss. Akad. Wiss. **1**, 123–130 (1921). p. 245: "Über den gegenwärtigen Stand der Feld-Theorie," in: Festschrift, Füssli Verlag, Zürich u Leipzig (1929), p. 126–132. p. 416: "Elementary derivation of the equivalence of mass and energy," Bull. Am. Math. Soc. **61**, N 4, 223–230 (1935).

[15] L. I. Mandel'shtam, Poln. Sobr. Trudov (Complete Collection of Works), Vol. V, Izd-vo Akad. Nauk SSSR, Moscow-Leningrad, 1950.

[16] Yu. E. Molchanov, Chetyre Kontseptsii Vremeni v Filisofii i Fizike (Four Conceptions of Time in Philosophy and Physics), Nauka, M. 1977.

[17] W. Pauli, Relativitätstheorie, in: Enzyklopädie der mathematischen Wissenschaften, II, Teuber, Leipzig (Russ. Transl. Gostekhizdat, Moscow-Leningrad, 1947).

[18] E. L. Feĭnberg, Usp. Fiz. Nauk **116**, 709 (1975) [Sov. Phys. Usp. **18**, 624 (1975)].

[19] P. A. Schilpp (ed), Albert Einstein: Philosopher–Scientist, Harper, New York, 1949.

[20] P. A. Schilpp (ed), Albert Einstein als Philosopher und Naturforscher, Stuttgart (1955), p. 493 (German translation of Ref. 19).

[21] From a Life of Physics: Evening Lectures at the International Centre for Theoretical Physics, Vienna: IAEA, p. 36–37 (Russian translation (improved) from: Usp. Fiz. Nauk **102**, 279 (1970).

[22] L. de Broglie, "Henri Poincaré and physical theories," in: Izbrannye Trudy Anri Puankare (Selected Works of Henri Poincaré), Vol. III, Nauka, M. 1974, p. 707.

[23] L. Brillouin, Relativity Examined, Academic Press 1970 (Russ. Transl. Mir, M. 1972).

[24] S. Goldberg, "Poincaré's silence and Einstein's theory of relativity," in: Éĭnshteĭnovskiĭ Sbornik. 1972 (Einstein Collection, 1972), Nauka, M. 1974.

[25] a) H. Poincaré, La Science et l'Hypothese, Flammarion, Paris (1902). b) H. Poincaré, (Russian Transl. of Ref. 25a, Moscow, 1904).

[26] H. Poincaré, "The measurement of time," in: Izbrannye Trudy Anri Puankare (Selected Works of Henri Poincaré), Vol. III, Nauka, M. 1974, pp. 427, 428.

[27] Ob Osnovaniyakh Geometrii (On the Foundations of Geometry; Russian collection), Gostekhizdat, M. 1956, p. 398.

[28] M. Reichenbach, Philosophic Foundations of Quantum Mechanics, University of California Press, Berkley–Los Angeles, 1965.

[29] A. Grunbaum, Philosophical Problems of Space and Time, Knopf 1964 (Russ. Transl., Progress, M. 1969, p. 160).

[30] R. Carnap, Philosophical Foundations of Physics: An Introduction to the Philosophy of Science, 1966 (Russ. Transl., Progress, M. 1971).

[31] S. G. Suvorov, Usp. Fiz. Nauk **86**, 537 (1965) [Sov. Phys. Usp. **8**, 578 (1965)].

[32] H. Poincaré, La Valeur de la Science, Paris 1905, pp. 7, 9; (cited by Lenin in Ref. 33) (Russ. Transl. Tvorcheskaya Mysl', M. 1906).

[33] V. I. Lenin, Poln, Sobr. Soch., T. 18 (Complete Collections of Works), Vol. 18; Filosofskie Tetradi (Philosophical Notebooks), p. 170.

[34] L. de Broglie, On the Paths of Science (Russ. Transl. IL, M. 1962, p. 307).

[35] H. Poincaré, Last Thoughts (Russ. Transl., Nauchnoe Knigoizd-vo, 1923, p. 31); also in: Novye Idei v Matematike (New Ideas in Mathematics), Collection 2, Obrazovanie, St. Petersburg, 1913, p. 90. (The original was published after Poincaré's death).

[36] E. Mach, The Science of Mechanics: A Critical and Historical Account of its Development, Open Court, La Salle, 1960 (Russ. Transl., St. Petersburg, 1909).

[37] B. B. Kadomtsev, L. V. Keldysh, I. Yu Kobzarev, and R. Z. Sagdeev, Usp. Fiz. Nauk **106**, 660 (1972) [Sov. Phys. Usp. **15**, 230 (1972)].

[38] P. A. M. Dirac, The Principles of Quantum Mechanics, Oxford, 1958, Ch. V (Russ. Transl., Fizmatgiz, M. 1968).

[39] W. Thirring, Ann. Phys. **16**, 69 (1961); V. I. Ogievetskiĭ and I. V. Polubarinov, Dokl. Akad. Nauk SSSR **166**, 585 (1966) [Sov. Phys. Dokl. **11**, 71 (1966)]; A. Z. Petrov, Dokl. Akad. Nauk SSSR **190**, 305 (1970) [Sov. Phys. Dokl. **15**, 40 (1970)].

[40] Ya. B. Zel'dovich and I. D. Novikov, Teoriya Tyagoteniya i Évolyutsiya Zvezd (Theory of Gravitation and the Evolution of Stars), Nauka, M. 1971.

[41] A. F. Ioffe, "Albert Einstein (on the fifth anniversary of his death)," Usp. Fiz. Nauk **71**, 3 (1960).

[42] V. P. Vizgin and Ya. A. Smorodinskiĭ, "From the equivalence principle to the equations of gravitation," Usp. Fiz. Nauk **128**, 393 (1979) (in the present issue).

[43] H. Poincaré, Science and Method, 1914, Reprinted by Dover (Russ. Transl., St. Petersburg, 1910, p. 26).

Translated by Julian B. Barbour