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oppenheimer memorial lecture

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University of California

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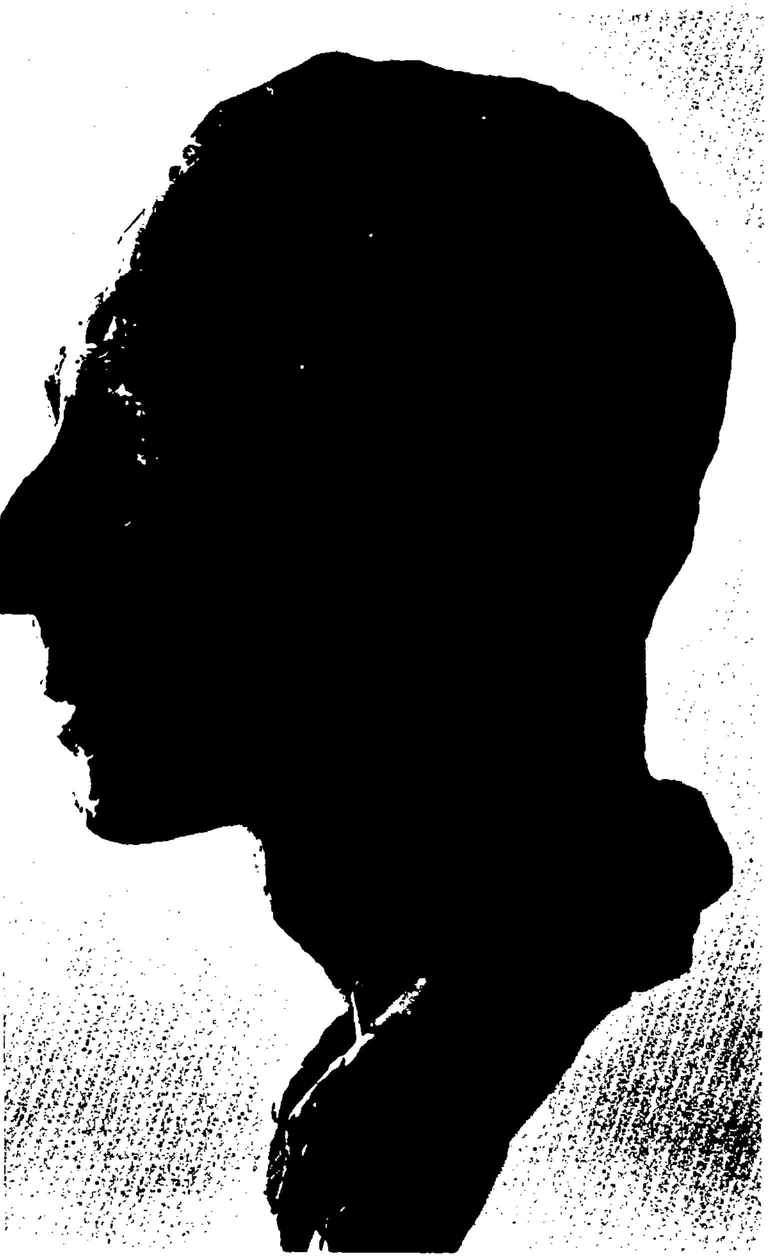

J. Robert Oppenheimer
(April 22, 1904—February 18, 1967)

AN INTRODUCTION

There is a remarkable interweaving in the careers of this evening's three principals; namely, of Robert Oppenheimer, to whom the evening is dedicated, Subrahmanyan Chandrasekhar, our speaker, and Albert Einstein, his subject.

In the early thirties Oppenheimer was interested in the structure of matter and predicted the existence of neutron stars. Confirmation of this new concept in astronomy was made some thirty years later. On the other hand, Chandrasekhar, in his astrophysical studies of the same period, was interested in some curious stellar objects—the so-called white dwarfs. Today we know that neutron stars and white dwarfs are companion endpoints of stellar evolution.

There is another remark of comparison. It could be said of Oppenheimer that he was the last of the universalists in physics. Today the main parts of that discipline have been further partitioned, almost without end, and specialization has run rampant—it is unlikely that another Oppenheimer-like physicist will emerge. It is probably true that our speaker with his wide-ranging and fundamental researches, together with his comprehensive knowledge of the field, is the last of the universalists in astronomy.

Also in the early thirties, Einstein left Europe to live in Princeton. He was, of course, the **raison d'etre** *for the then new Institute for Advanced Study. Shortly after World War II, Oppenheimer was invited to the Institute and became its distinguished director. In his second year he gave a series of evening lectures on modern developments in physics. There were quite a few in the audience who had made nontrivial contributions to that development, but the centerpiece was Einstein. It was interesting to note that he attended all the lectures. For others in the audience, the dazzle of starlight was almost too much.*

Our speaker has sometimes remarked that a scientist should change his field of specialization every decade or so. He, himself, has practiced this point of view and has worked with great intensity in several areas. At the end of each such period he has written the definitive tome in the field. He has done this for stellar dynamics and stellar structure, hydrodynamics and hydromagnetics. We hope that this is happening for general relativity, his current interest.

Now for some of the fundamentals. He was born in the ancient and venerable city of Lahore, in the Punjab—what is today the capital of West Pakistan. The date is easy to remember—19,10,1910 (19 October 1910)

After completing his undergraduate studies in Madras, he took his doctorate in theoretical physics at Cambridge. Not to be outdone, Oxford was later to award him an honorary degree.

He was a Fellow of Trinity College, Cambridge, for several years following and then to the University of Chicago for at least the next forty years. As early as 1946, the University conferred upon him its highest honor by appointing him a Distinguished Service Professor.

Many honors and honorary degrees accumulated. I mention only three:

Royal Medal of the Royal Society

Gold Medal of the Royal Astronomical Society

National Medal of Science.

He is a Fellow of the Royal Society as well as a member of the National Academy of Sciences.

There is a little-known story about his election to that Academy, very likely not even known to him. It was told to me by a mutual colleague and friend, Willie Zachariasen.

Election to the Academy begins with a nomination originating in the appropriate professional section and then if successful, it goes to the full membership for a final vote.

It is a rarity that a candidate gets a unanimous endorsement at the sectional level. It was unprecedented to achieve unanimity from the full membership, that is until the name of our speaker was proposed. That event has not lost its uniqueness.

In the course of his editorship, for nearly twenty years, the Astrophysical Journal became the prestigious publication in the field—primarily because he failed to compromise any of his standards.

One final remark. The Oppenheimer Committee cannot claim any precedence in inviting Professor Chandrasekhar for this evening's activity. Ten years ago, he was invited and gave the Nehru Memorial Address in India.

It is especially timely that the subject chosen by Professor Chandrasekhar is Einstein and General Relativity—Historical Perspectives. (1979 is the hundredth anniversary of Einstein's birth.)

It is also appropriate to quote one of the last published remarks of Oppenheimer.

Science is not everything
but science is very beautiful.

It is very good to have you back, Chandra. Welcome.

N. Metropolis

S. Chandrasekhar

J. Robert Oppenheimer Memorial Lecture

1978

EINSTEIN AND GENERAL RELATIVITY— HISTORICAL PERSPECTIVES

Einstein's place in the physics of the 20th century is generally considered unique. And one may ask, "Why?" For one could name several whose fundamental contributions to the physicist's common stock of knowledge may be considered even more relevant than Einstein's—at any rate, comparable to his. Here are the names of some: Lorentz, Poincaré, Rutherford, Bohr, Fermi, Heisenberg, Dirac, and Schrodinger.

Einstein's contributions to that part of physics with which all students of physics, without exception, would be familiar are those derived from his three famous papers of 1905: dealing with his founding of the special theory of relativity, the theory of Brownian motion, and the concept of the photon. While all these contributions, singly, and even more, together, place Einstein among the foremost physicists of our time, one cannot be confident that, on these accounts, his place is one of exceptional uniqueness. After all, Lorentz, and even more Poincaré, were not that far behind Einstein in formulating the principles of special relativity; and it is to Minkowski that we turn for the deepest formulation of the concepts of special relativity. Smoluchowski, independently of Einstein, discovered the theory of Brownian motion; and it is to Smoluchowski that we turn for the unravelling of all the multifarious aspects of the theory. And in the formulation of his concept of the photon, Einstein was preceded by Planck and followed by Bohr. And let us not forget the great figure of Poincaré who looms behind so much of the mathematics of the 20th century and of physics indirectly.

Why, then, is Einstein unique? To this question the answer undoubtedly is that besides all his contributions that have been enumerated, he was the sole and the lonely discoverer of the general theory of relativity.

With that assessment, I agree.

But Einstein's unique fame deriving from his development of the general theory of relativity has many paradoxical aspects. Perhaps the most striking of these is the exalted place which Einstein was given for his discovery of the general theory of relativity by some of the early investigators who were eminent men of science themselves and the benign neglect to which his theory was consigned by the professional scientific community for some fifty years, not to mention the active hostility to which his theory has been subjected over the years. The unravelling of the many conflicting strands of opinion with respect to general relativity is not an easy task. It is made somewhat easier for me since I share and endorse Hermann Weyl's description of general relativity "as the greatest example of the power of speculative thought."

Let me begin by describing in the most general terms the basic ideas which led Einstein to his theory of gravitation by the sheer power of his speculative thought. But it should be emphasized first, that Einstein's replacement of the Newtonian theory of gravitation by his own theory did not arise in any of the normal ways in which new physical theories emerge.

It is almost invariably the case that new theories of physics, or novel generalizations of the old, result from a definite conflict with experience; and the ideas for the new theory are distilled from the need to incorporate the facts which appear conflicting with what is already known into a harmonious whole. Further, the successes of the new ideas are judged by the extent to which they can account for new phenomena. The general theory of relativity did not originate in this fashion.

Einstein started with the premise that Newton's theory required a reformulation, since it was in manifest conflict with his own special theory of relativity. The basic tenet of the special theory of relativity is that in physics there can be no instantaneous action at a distance; and that no signal of any kind can be propagated with a velocity exceeding that of light. And, of course, Newton's laws of gravitation postulate instantaneous action at a distance. Besides, at the base of Newton's laws of gravitation is an enigmatic fact—well established, but not understood, before Einstein. The enigmatic fact goes back to Galileo's well-known demonstration from the leaning tower at Pisa, that all bodies, large or small, are accelerated equally in the local gravitational field of the earth. From this equality of acceleration of different masses, one concluded that mass as a measure of the quantity of matter and mass as a measure of its weight are identically the same. This identity is commonly referred to as the equality of the inertial and the gravitational mass of a body. But this equality has no theoretical basis: it is an empirical fact which requires experimental evidence. Newton was well aware of the need for experimental evidence for this crucial fact. Thus, in the opening paragraph of his *Principia*, Newton wrote:

"Air of double density, in a double space, is quadruple in quantity; in a triple space, sextuple in quantity. It is this quantity that I mean hereafter, everywhere, under the name of body or mass. And the same is known by the weight of each body, for mass is proportional to the weight, as I have found by experiments on pendulums very accurately made."

(May I parenthetically note that this equality of inertial and gravitational mass to which Newton makes reference, has in recent years been established to an accuracy of one part in several billions.)

The extremely general facts I have stated provided Einstein with the entire basis for his formulating the general theory.

Even given these bases for generalizing Newton's theory, there really was no compelling need for an exact theory of gravitation. For, on general grounds, one could have argued that the Newtonian theory should be valid so long as the velocity of a planet or a satellite is small compared to the velocity of light. Even Mercury, the planet closest to the Sun, describes its orbit around the Sun with a velocity which is 6000 times smaller than the velocity of light. Accordingly, any departures from the predictions of the Newtonian theory can be estimated to be no more than a few parts in a billion. On this account, it would have been entirely sufficient to generalize the Newtonian theory to allow for such small departures which may arise from the finiteness of the velocity of light since we expect the Newtonian theory to be exact if the velocity of light could be considered as infinite. And that would have been the normal way.

But Einstein did not proceed in that way. He searched for an exact theory which would be valid even if the velocities of the gravitating bodies approached that of light. Certainly, an exact theory, even if one should succeed in formulating it, could never be confirmed by experimental or observational features which, as I have stated, must be minute, by all criteria, in the solar system; and moreover, when Einstein sought for a new theory he had no prior conception as to what the nature of the departures may be that the new theory would be asked to account for. But as was stated by one of his early associates, Cornelius Lanczos, by a combination of constructive mathematical thinking, philosophical imagination, and a nonerring aesthetic sense, Einstein arrived at his exact equations governing the theory of gravitation, a theory in which the three fundamental entities, space, time, and matter were unified.

As a rule, Einstein generally refrained from any emotional exclamation marks in his publications; but he overcame his reticence in the concluding sentence of his first communication in November 1915 to the Berlin Academy in which he announced the basic equations of his theory. He wrote:

> "Scarcely anyone who has fully understood this theory can escape from its magic; it represents a genuine triumph of the method of the absolute differential calculus of Christoffel, Ricci, and Levi Civita."

Hermann Weyl and Arthur Eddington, who wrote the first serious expositions of relativity, responded to Einstein's magic. Thus, in the preface to his *Space, Time, and Matter*, published in the spring of 1918, Weyl wrote:

> "It is as if a wall which separated us from the truth has collapsed. Wider expanses and greater depths are now exposed to the searching eye of knowledge, regions of which we had not even a presentiment. It has brought us much nearer to grasping the plan that underlies all physical happening."

And in Eddington's *Space, Time, and Gravity*, published in 1920, the opening sentence reads:

> "By his theory of relativity, Albert Einstein has provoked a revolution of thought in physical science."

Others of comparable eminence, who have studied general relativity and made contributions to it have written similarly. Thus, Landau and Lifshitz in their well-known book on *Classical Theory of Fields* in introducing the general theory of relativity, state that it "represents probably the most beautiful of all existing physical theories." And Dirac has said that Einstein's generalization of the special theory of relativity to include gravitation "is probably the greatest scientific discovery that was ever made."

From these statements of the eminent men of science who have studied the theory of relativity and made important contributions to it, one might conclude that the general theory of relativity is an accepted theory and that only cranks would doubt its validity. But that is not the case. A great number of eminent men have either given faint praise or have considered Einstein's theory as just plainly incorrect. Let me quote some varying shades of opinion.

Max Born, who was an assistant of Einstein's in Berlin during the very years when Einstein was developing his general theory of relativity, in 1955, on the occasion of the 50th anniversary of Einstein's great paper on special relativity, stated:

> "The foundation of general relativity appeared to me then, and still does as the greatest feat of human thinking about Nature, the most amazing combination of philosophical penetration, physical intuition, and mathematical skill. But its connection with experience is slender. It appealed to me like a great work of art, to be enjoyed and admired from a distance."

But what are we to make of this seeming praise of general relativity? Has it only to be admired from a distance? Does it not then require study and development like any other branch of the physical sciences? And a cynic might add that the description of Einstein's work as a work of art is often the cloak in which physicists disclaim the relevance of general relativity to the advance of physics. Here is J. J. Thomson:

> "Einstein has given a second theory known as 'general relativity' which includes the theory of gravitation. This involves much very abstruse mathematics, and there is much of it I do not profess to understand. I have, however, a profound admiration for the masterly way in which he has attacked a problem of transcendent difficulty."

And here is Rutherford:

> "The theory of relativity of Einstein, quite apart from its validity, cannot but be regarded as a magnificent work of art."

The description of general relativity as a work of art is double-edged. One senses that in describing the theory in this way, one is trying not to dissociate oneself from the general acclaim that is accorded to Einstein. But the matter is not that simple. It is, in fact, the case that the literature dealing with theories alternative to Einstein's are as numerous as the positive contributions devoted to exploring the content of the theory itself. It is not merely that cranks and pseudoscientists have written tracts disputing Einstein. Several eminent men of science, whom we all respect, have also considered Einstein's theory as scientifically unsound. Let me list the names of some of those who have written books and tracts presenting theories which they consider as viable alternatives to Einstein's theory. Alfred North Whitehead, the distinguished philosopher-mathematician; George Birkoff, the distinguished mathematician; E. A. Milne, the distinguished astrophysicist; and Hoyle and Narlikar. Besides, several earlier adherents of Einstein's theory have now discerned either flaws or grave crises; e.g., Nathan Rosen and Christian Mϕller.

While I shall not give any account (I could not do so dispassionately), I will quote a few sentences from Whitehead, whose many writings on science and philosophy many of us have admired.

In 1922, Whitehead published a book entitled, *The Principle of Relativity*, an alternative to Einstein's theory. Whitehead starts by quoting with approval an aphorism of J. J. Thomson:

> "I have no doubt whatever that our ultimate aim must be to describe the sensible in terms of the sensible."

After this quotation, Whitehead goes on to say, "I do not agree with Einstein's way of handling his discovery," meaning that Einstein's theory does not describe sensible things in a sensible way. Here are some other quotations from Whitehead which will give you a flavor of his reasoning:

> "So many considerations are raised that we are not justified in accepting blindfolded the formulation of principles which guided Einstein."

Or, again:

> "In the comparative absence of applications, beauty, generality, and even Truth, will not save a doctrine from neglect in scientific thought. ... To expect to reorganize our ideas on Time, and Space, and Measurement without some discussion, which must be ranked as philosophical, is to neglect the teaching of history and the inherent possibilities of the subject."

Convinced that Einstein's formulation of space-time as a Riemannian manifold with a metric is invalid on philosophical grounds, Whitehead goes on to develop a detailed theory of his own. But unfortunately for Whitehead, some of the implications of his theory have been shown to be blatantly contrary to experience in several instances in which Einstein's theory succeeds admirably. Whitehead's philosophical acumen has not served him well in his criticisms of Einstein.

I said a little earlier that I was making an exception of Whitehead's criticisms of Einstein. But I cannot desist quoting the reaction to relativity of one of my personal friends, the late Professor E. A. Milne. In developing his kinematical theory of relativity, Milne stated:

> "Einstein's law of gravitation is by no means an inevitable consequence of the conceptual basis given by describing phenomena by means of a Riemannian metric. I have never been convinced of its necessity .... General relativity is like a garden where flowers and weeds grow together. The useless weeds are cut with the desired flowers and separated later."

Milne goes on to say, referring to his own theory:

> "In our garden we grow only flowers."

I think that I have stated enough to convince you that the general theory of relativity did not receive acceptance from many respected scholars, who have apparently tried to understand the theory. But one may ask, "What was the attitude of a serious physicist (of the period say from 1925 to 1965) to general relativity?" Born's remark that the connection of general relativity with experience is slender is representative. I have been told that at a dinner in honor of Einstein's 70th birthday, held at the Institute for Advanced Study in 1949, Oppenheimer made remarks to the effect that general relativity had been singularly without influence in the development of physics during the period 1925-1950. Indeed, general relativity as a discipline in physics was simply ignored or at any rate neglected benignly in most institutions devoted to its study. As an illustration of this fact I might refer to the circumstance that from 1936, when I joined the faculty of the University of Chicago, to 1961, no courses in general relativity, not even for one single quarter, were given at the University. And the University of Chicago is not atypical. And I am not sure how well the principles of general relativity, as laid by Einstein, are appreciated by the common physicists of today.

Where, then, does Einstein's fame come from?

It will be presumptuous of me to suggest an answer of my own to the question I have just raised. But I will give an answer given by Rutherford, during a conversation 45 years ago, at which I was present.

The conversation took place in the Senior Combination Room in Trinity College, after dinner, during the Christmas recess of 1933. During the Christmas recess, very few people normally dine in the College. On this particular occasion there were only five of us: Lord Rutherford, Sir Arthur Eddington, Sir Maurice Amos (at one time, during the 1920s, the Chief Judicial Advisor to the Egyptian government), Dr. Patrick DuVal (a distinguished geometer), and myself. After dinner, we all sat around a fire and in the ensuing conversation Rutherford was in great form.

At some point during the conversation, Sir Maurice Amos turned to Rutherford and said:

> "I do not see why Einstein is accorded a greater public acclaim than you. After all, you invented the nuclear model of the atom; and that model provides the basis for all of physical science today and it is even more universal in its applications than Newton's laws of gravitation. Whereas, granted that Einstein's theory is right—I cannot say otherwise in the presence of Eddington here—Einstein's predictions refer to such minute departures from the Newtonian theory that I do not see what all the fuss is about."

Rutherford, in response, turned to Eddington and said:

"You are responsible for Einstein's fame."

And more seriously, he continued:

> "The war had just ended, and the complacency of the Victorian and Edwardian times had been shattered. The people felt that all their values and all their ideals had lost their bearings. Now, suddenly, they learned that an astronomical prediction by a German scientist had been confirmed by expeditions to Brazil and West Africa and, indeed, prepared for already during the war, by British astronomers. Astronomy had always appealed to public imagination; and an astronomical discovery, transcending worldly strife, struck a responsive cord. The meeting of the Royal Society, at which the results of the British expeditions were reported, was headlined in all the British papers; and the typhoon of publicity crossed the Atlantic. From

that point on, the American press played Einstein to the maximum."

I could see from Eddington's reaction that he agreed with Rutherford; and he, in turn, recalled some events of that time.

Let me go back a little to tell you about the circumstances which gave rise to the planning of the British expeditions. I learned of the circumstances from Eddington (in 1935) when I expressed to him my admiration of his scientific sensibility in planning the expeditions during "the darkest days of the war." To my surprise, Eddington disclaimed any credit on that account—indeed, he said that, left to himself, he would not have planned the expeditions since he was fully convinced of the truth of the general theory of relativity! And he told me how the expeditions came about.

In 1917, after more than two years of war, England enacted conscription for all able-bodied men. Eddington, who was then 34, was eligible for draft. But as a devout Quaker, he was a conscientious objector; and it was generally known and expected that he would claim deferment from military service on that ground. Now the climate of opinion in England during the war was very adverse with respect to conscientious objectors: it was, in fact, a social disgrace to be even associated with one. And the stalwarts of Cambridge of those days—Sir Joseph Larmor (of the Larmor precession), Professor H. F. Newall, and others—felt that Cambridge University would be disgraced by having one of its distinguished members a declared conscientious objector. They therefore tried through the Home Office to have Eddington deferred on the grounds that he was a most distinguished scientist and that it was not in the long-range interests of Britain to have him serve in the army. (The case of H. G. J. Moseley, who discovered the concept of atomic number and who was killed in action at Gallipoli, Turkey, was very much in the minds of the British scientists at that time.) And Larmor and others nearly succeeded in their efforts.

A letter from the Home Office was sent to Eddington, and all he had to do was to sign his name and return it. But Eddington added a postscript to the effect that, if he were not deferred on the stated grounds, he would claim it on conscientious objection anyway. This postscript, naturally, placed the Home Office in a logical quandary: a confessed conscientious objector "had to be sent to a camp." Larmor and others were annoyed. Eddington told me that he could not understand their annoyance; and as he expressed himself, many of his Quaker friends found themselves in camps in Northern England "peeling potatoes" for holding the same convictions and he saw no reason why he should not join them. In any event, at Sir Frank Dyson's intervention—as the Astronomer Royal, he had close connections with the Admiralty—Eddington was deferred with the express stipulation that if the war should have ended by 1919, he should lead one of two expeditions that were being planned for the express purpose of verifying Einstein's prediction with regard to the gravitational deflection of light.

In any event, Eddington clearly realized the importance of verifying Einstein's prediction with regard to the deflection of the light from distant stars as it grazed the solar disk during an eclipse. It is best that I continue the story in Eddington's own words.

> "In a superstitious age a natural philosopher wishing to perform an important experiment would consult an astrologer to ascertain an auspicious moment for the trial. With better reason, an astronomer today consulting the stars would announce that the most favorable day of the year for weighing light is May 29. The reason is that the sun in its annual journey round the ecliptic goes through fields of stars of varying richness, but on May 29 it is in the midst of a quite exceptional patch of bright stars—part of Hyades—by far the best starfield encountered. Now if this problem had been put forward at some other period of history, it might have been necessary to wait some thousands of years for a total eclipse of the sun to happen on the lucky date. But by strange good fortune an eclipse did happen on May 29, 1919.
>
> "Attention was called to this remarkable opportunity by the Astronomer Royal (Sir Frank Dyson) in March 1917; and preparations were begun by a committee of the Royal Society and the Royal Astronomical Society for making the observations.
>
> "Plans were begun in 1918 during the war, and it was doubtful until the eleventh hour whether there would be

any possibility of the expeditions starting. Two expeditions were organized at Greenwich by Sir Frank Dyson, the one going to Sobral in Brazil and the other to the Isle of Principe in West Africa. Dr. A. C. D. Crommelin and Mr. C. Davidson went to Sobral; and Mr. E. T. Cottingham and the writer went to Principe.

"It was impossible to get any work done by instrument makers until after the armistice; and as the expeditions had to sail in February, there was a tremendous rush of preparation. The Brazil party had perfect weather for the eclipse; through incidental circumstances, their observations could not be reduced until some months later, but in the end they provided the most conclusive confirmation. I was at Principe. There the eclipse day came with rain and cloud-covered sky, which almost took away all hope. Near totality, the sun began to show dimly; and we carried through the program hoping that the conditions might not be so bad as they seemed. The clouds must have thinned before the end of totality, because amid many failures we obtained two plates showing the desired star-images. These were compared with plates already taken of the same star-field at a time when the sun was elsewhere, so that the difference indicated the apparent displacement of the stars due to the bending of the light-rays in passing near the sun.

"As the problem then presented itself to us, there were three possibilities. There might be no deflection at all; that is to say, light might not be subject to gravitation. There might be a "half-deflection," signifying that light was subject to gravitation, as Newton had suggested, and obeyed the simple Newtonian law. Or there might be a "full deflection," confirming Einstein's instead of Newton's law. I remember Dyson explaining all this to my companion Cottingham, who gathered the main idea that the bigger the result, the more exciting it would be. 'What will it mean if we get double the deflection?' 'Then,' said Dyson, 'Eddington will go mad, and you will have to come home alone.'

"Arrangements had been made to measure the plates on the spot, not entirely from impatience, but as a precaution

> against mishap on the way home, so one of the successful plates was examined immediately. The quantity to be looked for was large as astronomical measures go, so that one plate would virtually decide the question, though, of course, confirmation from others would be sought. Three days after the eclipse, as the last lines of the calculation were reached, I knew that Einstein's theory had stood the test and the new outlook of scientific thought must prevail. Cottingham did not have to go home alone."

It was some months before the two expeditions returned to England and the participants were able to measure their plates and collate their results. But rumors of the successful confirmation of Einstein's prediction reached Einstein in early September 1919. And on September 22, 1919, the Dutch physicist, Hendrik Antoon Lorentz, sent Einstein a telegram confirming the rumors to which Einstein replied (also by telegram), "Heartfelt thanks to you and Eddington. Greetings." Einstein's own satisfaction with the outcome of the British expeditions is shown by the postcard (dated September 27, 1919) to his ailing mother in Switzerland. It said:

> "Dear Mother: Good news today. H. A. Lorentz has wired me that the British expeditions have actually proved the light deflection near the Sun..."

There is a further anecdote relative to Einstein's reaction to the news from England which I should like to recall.

Ilse Rosenthal-Schneider, a student of Einstein in 1919, recalls that Einstein showed her the cable from Eddington informing him of the successful verification of his prediction. And she asked him, what if there had been no confirmation of his prediction? Einstein's response was:

> "Then I should have been sorry for the dear Lord; but the theory is correct."

The Times of London for November 7, 1919, carried two headlines: "The Glorious Dead, Armistice Observance. All trains in the Country to Stop," and "Revolution in Science. Newtonian Ideas Overthrown." The second of these headlines referred to the meeting of the Royal Society in London on November 6 at which Dyson had reported on the results of the British expeditions.

Alfred North Whitehead's account of this meeting of the Royal Society has often been quoted. It is worth quoting once again.

> "The whole atmosphere of tense interest was exactly that of the Greek drama: we were the chorus commenting on the decree of destiny as disclosed in the development of a supreme incident. There was dramatic quality in the very staging—the traditional ceremonial, and in the background the picture of Newton to remind us that the greatest of scientific generalizations was now, after more than two centuries, to receive its first modification. Nor was the personal interest wanting: a great adventure in thought had at length come safe to shore."

The meeting of November 6, 1919, of the Royal Society also originated a myth that persists even today (though in a very much diluted version): "Only three persons in the world understand relativity." Eddington explained the origin of this myth during the Christmas recess conversation with which I began this account.

Sir J. J. Thomson, as President of the Royal Society at that time, concluded the meeting with the statement:

> "I have to confess that no one has yet succeeded in stating in clear language what the theory of Einstein's really is."

And Eddington recalled that as the meeting was dispersing, Ludwig Silberstein (the author of one of the early books on relativity) came up to him and said:

> "Professor Eddington, you must be one of three persons in the world who understands general relativity."

On Eddington demuring to this statement, Silberstein responded, "Don't be modest Eddington." And Eddington's reply was, "On the contrary, I am trying to think who the third person is!"

The myth that general relativity is a difficult theory to understand originated at this time. It is a myth which has done immeasurable harm to the development of the theory. The fact is that the theory of general relativity is no more difficult than many other branches of physics. General relativity, at the time it was founded, required familiarity with a mathematical discipline which physicists had not encountered before that time. But that has also been the case with several other branches of physics, including quantum mechanics.

I cannot conclude this account, relating to the verification of Einstein's prediction concerning the deflection of light by a gravitational field and how that was responsible for his becoming "a focus of widespread adoration," without remarking that history might well have been very different.

In 1911, Einstein had calculated, on the basis of his principle of equivalence, the deflection that light grazing an object, such as the Sun, will experience. The principle of equivalence correctly accounts for the slowing of a clock in a gravitational field; but it gives for the deflection of light only half the value predicted by general relativity. Roughly speaking, one might say that one-half of the predicted effect is the result of the slowing down of the time-measuring process; and that the other half is due to the spatial curvature of space-time. The latter effect is an essential aspect of general relativity.

The German astronomer, Finlay Freundlich, had planned to test Einstein's prediction of 1911 at an eclipse of the Sun which occurred in Russia in 1914. But the war intervened; and Freundlich was unable to make the observations he had planned. As Hoffmann and Dukas have said:

> "Suppose the war had not come and Finlay Freundlich had been able to observe the 1914 eclipse and had found a deflection of 1.7 seconds of arc at a time when Einstein was predicting a deflection of only 0.83 seconds of arc. Imagine how tame Einstein's 1915 calculation of 1.7 seconds of arc would have seemed. ... He would have been belatedly changing the value after the event, having first been shown to have been wrong ... and the deflection of light would have lost the tremendous impact that it had as a prediction."

But the war had in fact intervened; and the predicted deflection had been confirmed under circumstances described by Rutherford. And as Eddington wrote to Einstein in December 1919:

> "All of England has been talking about your theory. It has made a tremendous sensation. ... It is the best possible thing that could have happened for scientific relations between England and Germany."

Are we to conclude then that the unique place in physics which Einstein is accorded is an accident of circumstances? I do not think so. The testimony for his uniqueness comes from those who, serious students of science themselves, are caught in the web of the magic of

Einstein's theory and feel, as Hermann Weyl felt, that a wall obscuring truth collapses when we explore the richness of his theory.

In saying this, I do not wish you to conclude that those who marvel at the content of Einstein's theory form a cult of some sort. That would be the case if one's admiration for the theory was derived from a distance, as Born has stated, or as a "work of art," as Rutherford and J. J. Thomson have stated. The simple fact is that Einstein's theory is incredibly rich in its content and presents glittering faces at every turn.

Let me be specific and illustrate what I mean in a concrete way.

I am sure that all of you are familiar with the role black holes have been publicized to play in current astronomical developments. Let me say at once that I do not associate myself with those who consider black holes as exotic objects predicted by general relativity. Exotic means grotesque, bizarre: and there is nothing grotesque or bizarre about black holes.

We are all aware that a body projected from earth, for example, cannot escape the earth's gravitational field unless it is projected with a sufficient speed; otherwise, it will simply fall back. Once we grant that light is deflected by a gravitational field—as, indeed, we must—then it is a matter of simple arithmetic to calculate how strong the gravitational field must be if a particle, projected with a velocity equal to that of light, cannot escape. This calculation was, in fact, made by Laplace as long ago as 1798, even though he had no reason to suspect that light is affected by gravity.

We have seen that light grazing the Sun is deflected by the minute amount of 1.7 second of arc. But if the Sun with its present radius of 700,000 km should be compressed to a sphere of radius 2-1/2 km, then at that radius, the gravitational field would be strong enough to prevent light from escaping from the surface; and we should cease to see it: it would have become a black hole.

The contraction of a star with a solar mass to a radius of 2-1/2 km does not require us to postulate physical conditions with which we are not familiar. The mean density of matter at that radius is no different than in ordinary atomic nuclei. The physical conditions required for the occurrence for stellar masses to become black holes are, therefore, entirely within the realm of reason. The question is, rather, whether such physical conditions can be realized in Nature and in the natural course of events. Let me categorically state that very simple and quite elementary considerations relating to the last stages in the evolution of massive stars—i.e., stars with masses exceeding say five solar masses—require the formation, barring accidents in every case, of black holes. This is a story which is fascinating in itself, but it is not the story I wish to tell now. I want, rather, to turn to what the general theory of relativity has to say with regard to black holes.

One might think that if all that is required for black holes to occur are sufficiently strong gravitational fields, then black holes of diverse shapes, forms, and sizes should be possible. For example, the external shape and size that a gravitating object can have in Newtonian theory are infinitely diverse: they will depend on the mass, the stratification of density, and temperature in the interior, whether it is rotating or not; and if it is rotating, whether it is rotating uniformly or not, and how fast it is rotating; and a whole variety of other factors. But very remarkably, according to the general theory of relativity, black holes belonging to one family and of only one kind can occur. This family of solutions was discovered by Roy Kerr, a New Zealand mathematician, in 1962. Kerr's solutions provide the basis for an exact representation of all black holes that can occur in the astronomical universe.

It is not only that Kerr's solution is unique: there is an explicit formula which one can write for it. This is a startling and an unexpected consequence of the general theory of relativity. Besides, the geometry of the space-time around a Kerr black hole has many remarkable features. For example, in Kerr geometry all the standard equations of mathematical physics can be solved exactly; and we are also led to mathematical identities of a kind one had never suspected.

I do not know to what extent the foregoing remarks appear convincing to you. But the point I wish to make is that the general theory of relativity is incredibly rich in its content; and as I said, one finds a glittering face at almost every turn. Einstein was certainly correct in his prediction of 1915 that "anyone who genuinely understands the theory" cannot escape from its magic. If some remote and presently unforeseen development requires a modification of Einstein's theory within its own well-defined framework of validity, then we should indeed have reason to "be sorry for the dear Lord."

This lecture is the seventh in a series sponsored by the J. Robert Oppenheimer Memorial Committee of Los Alamos, New Mexico. The previous speakers and their topics were

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David H. Sharp
Chairman (1978)