

'THE CASE AGAINST EINSTEIN

WORKS BY DR. ARTHUR LYNCH

Science

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THE CASE
AGAINST EINSTEIN

BY
ARTHUR LYNCH

LONDON
PHILIP ALLAN

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PREFACE

BRAVE men find fascination in voyages of discovery ; few joys can be compared to the thrill and excitement when first the eye discerns on the horizon a faint cloudy strip that surges up, wonder on wonder, as a new land, full of untold possibilities of riches and delight. Yet greater still are the voyages of the mind, when trembling along the subtle lines of our thought, a new idea, a new discovery, reveals itself, that tells of a secret of Nature ; for secrets of Nature are never single nor barren, and once the source has been reached the way is won to a very continent of power, and the subtle speculations of a solitary thinker come to reality in the possessions that for ever afterwards enlarge the spiritual domain of man.

Is Relativity such a domain of new intellectual enlargement and strength ? There is something enlivening in the bold fresh adventure of thought, and to me, perhaps more than to most, there is a deep satisfaction in the break away from whatever is false or unwholesome in mere orthodox conventions. It is in the way of development of science to be daring in thought, even audaciously speculative ; yes, always with this proviso, that the speculations be tested with a courage, a desire for truth, no less inspired than the impulse of the first setting-out.

Another principle in science has imbued me with its

spirit, and that is to seek the general law in which the particular is contained, and then again to put this in a setting so that we see it in a still more general relation, until we reach a point at which we touch the controls of a domain of science. That was, according to Aristotle, the standard of value in thinking men. And so it is that the analysis of Galileo, showing the elementary forms of which all machines are built, leads step by step to developments that roll through the ages. And also in the discovery by Descartes, through his coordinates, of new modes of mathematics which in far-reaching results have given us mighty engineering works and the delicate mysteries of wireless, here again was an instance of touching the controls at a high level. The highest and most general of all has its own special allurements, to trace out, to the elements, the machination of the thoughts of man, to find the ultimate secrets of reasoning, and to show through the working of the minutest wheel-work the whole structure of the building of consecutive thinking; in other words, to establish the Fundamental Processes of the Mind, such that by their successive and varied combinations the whole world of thought may be built. If we achieve so much we have also an instrument of analysis, for on that basis the elucidation of all problems is possible.

The works of two men have in this regard filled me with keen anticipation: Kant and Einstein; the study of their reasoning has in turn left me with deep disappointment. To Kant, however, I owe, in somewhat inverted fashion, a debt of gratitude, for it was the failure of his system that sent me with impetus along the lines of research which have occupied the greater part of my intellectual life, no other in fact than the

analysis that led to the Fundamental Processes. With Einstein the disappointment has been real, but, I think, the chance of redemption less; his is not a valid system of science; it is a strange medley of metaphysical imaginings, mistaken interpretation of an experiment, unwarrantable assumptions in the building of a theory; while the intrepidity of thought is displayed, alas, mainly in the refusal to be daunted by the absurdity of the conclusions reached.

There are three domains of science of special interest in this theory of Relativity: that of psychology, of which Einstein rightly insists on the importance; that of physics, in which he indulges in tentative speculations; and that of mathematics, in which he disregards the essentials of clearness of vision and rigour of argument.

In the following pages I place his theory in its setting in each of these domains; in none of them, to my surprise, did I find originality of thought. His psychology is simply imitative, being derived from Kant as the fountain head, without discrimination or surety of comprehension, for Kant himself lacked that.

In the domain of physics, there is nothing new except the bizarre notions, the disconcerting conclusions. In the field of mathematics I have traced out successively from Descartes onward the source of his ideas—for here, too, there is nothing original—and I have shown that the magic, or the paradox, of his theory consists in giving to mathematical expressions which occur in the writings of others, notably of Riemann, and later of the exponents of Absolute Geometry, strained meanings, and the setting down as realities what are conventional modes of representing operations.

All this, however, has a dry, abstract appearance, and may produce that distaste which most men feel for the merely academic; but so far we have not touched on the real, even if already legendary, and divinely impossible, Einstein; that is to say, the Einstein of popular imagination; the man who has changed the qualities of the Universe by an intellectual turn of the hand; the man who has decreed the limits of velocity; the man who can curve our space from Euclidean to Lobatchewskian or Riemannian, from flat to spherical or saddle-backed, simply by fiddling with cryptic symbols; the man who tells us that bounded and infinite are just one, and that time is indistinguishable from space; the man who sees enveloping our world a finer transcendental world of which the freedom of the city is vouchsafed to the Relativitist disciples, and even—such is the power of this tremendous wave of thought—a few men of science.

Even during my own lifetime I have known many theories, famous in their day, overthrown. In the following pages I will show that Relativity may also be numbered among these ephemeral doctrines.

A. L.

LONDON, *May* 1932.

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INTRODUCTION

RELATIVITY has become a sort of new religion with Einstein for its prophet. It has the hall-mark of many religions that have swayed the minds of men in being founded on premises which at first sight seem dubious, and on examination by the profane—that is to say those who have not received a sort of mystical baptism—untenable, if not absurd. It also resembles a religion in this respect that when the truth of this statement is proved the faith of the believers remains unshaken. This book, therefore, is addressed mainly to those whose minds are still free and capable of separating the grain of real science from the chaff of impossible, even if delightfully mysterious, conceptions. The introduction is devoted to the consideration of those intangible but powerful influences, which, though completely unscientific, may determine reputations in science; and if I repeat myself, as I may do even to tedium, it is because I have continually run foul of the same falsities.

When I first began to look into the theory of Relativity and to criticise the arguments by which it was proposed to establish it, I was told that I must have 'great courage' to undertake such a task. I reflected. It is true that in every campaign of this nature I have provided myself with but one instrument—reason; and often, as I have looked upon the

embattled towers of authority, proudly flaunting the banners of error, I have felt how puny must have appeared that weapon in my hand. Yet I have marched onward, and the more I have espied the land, and the bastioned walls of my opponents, the more I have appreciated *their* courage, and particularly that of Einstein, in defending, with dazzling prestige and faulty argument, the fantastic edifice which he and his followers have built.

Yet the man of science has the right to be confident in his reason, for, if not immediately, yet eventually and surely, good reason based on the laws of nature will prevail. I have already had occasion to make this observation in the course of another series of studies which comprised the main lines of development of the principal sciences.* When Galileo dropped two pellets of lead from the leaning Tower of Pisa, and by that simple experiment proved the absurdity of the teachings of the Cardinals, his life was in danger; he was imprisoned and persecuted. The Cardinals raged in anger, and covered themselves in the virtuous indignation of noble persons, faithfully devoted to falsehood; but there, irrefragable, irrefutable, standing for ever as an index, was the evidence of Nature. Those two pellets drove a hole through a tawdry fabric of philosophy, a false system of civilisation.

Later,—and I take these out of a hundred examples of bad teachings buttressed up by names of authority—Maupertuis launched his theory of ‘Least Action,’ which was accepted by the learned world, and guaranteed by authorities more powerful than anything that Einstein can claim, for Frederick the Great him-

* *Science : Leading and Misleading.*

self sponsored the marvellous work. The doctrine of Maupertuis, for it really was a doctrine, had like that of Einstein a mystical and religious side, since Maupertuis proved the goodness, and therefore the everlasting existence, of God by showing that 'Action' in conveying energy from one point to another was always a minimum, and Nature was 'economical.' Maupertuis, anticipating our financial experts in their attempts to regulate a world ruined by their incompetence, gave a high spiritual value to economy. Voltaire, though not a mathematician, but a thinker whose wit was based on good sense, was one of the first to see through the nonsense of this pompous show of demonstration. He was followed by a young Irish mathematician, Count d'Arcy, who had taken service in the French army; d'Arcy had no lack of courage, but in confronting the imposing machines of Maupertuis he seems to have called rather upon his gaiety of spirit, for his reply is one of the wittiest expressions that ever found entrance into that austere domain. He showed that, according as a concave or convex mirror was interposed into the path of a beam of light, Nature, according to the arguments of Maupertuis, became the most parsimonious or the most spendthrift of mothers.*

Yes, you say, but what had the authority of Frederick the Great to do with this? Surely he was not an expert in mathematics? That is true, but amongst those to whom Frederick extended his patronage was the famous Swiss mathematician, Euler, who enjoyed a respect, rightly acquired, as one of the greatest

* There have since been numbers of studies of this question; one of the best is that of a German mathematician, A. Mayer. He speaks with high appreciation of Count d'Arcy's effort; "namentlich hübsch" (decidedly pretty), he calls it.

geometers of all time. Euler knew well the character of the faulty conceptions that had misled Maupertuis, but, in deference to the famous Frenchman's reputation, and in homage to the great Frederick, he blemished his integrity by covering them with his own authority.

I thought of that story on a certain evening when by gracious favour I was admitted to hear Einstein lecture on his principle of Relativity at King's College in London. He was the Maupertuis of his day, appearing there with a doctrine still more alluring, still more recondite, and, at the base of things, still more fantastic than that of the Gallic geometer. Frederick was represented by the professors sitting in their robes, those wonderful garments that might have proved rivals more serious than Solomon's to the lilies of the field.

I mention these details because I am dealing for the moment not with science, but with public impressiveness, and in this hierophantic display of the professors I have indicated the main source of their attraction for the public.

The whole affair was well stage-managed, just as I used to note in the House of Commons when Mr. Asquith rose to speak. Einstein appeared in modest guise, and I conceived a liking for the man, a liking since confirmed by M. Paul Painlevé—a man of genius, that—who lately spoke to me of Einstein's courtesy, equal indeed to his seriousness of purpose. Looking like a musician, as he is, with his crown of unruly, but now well-ordered, hair, his easy stance, his modest manner, and his short-sighted wondering eyes, Einstein spoke well. His exposition had an air, quite a deceptive air,

of lucidity, while now and then a lightness of manner and a witty expression reminded me that the learned professor was of the race of Heine, rather than that of Heine's professor Saalfeld.

But he spoke in German, of which language not ten per cent of his audience knew enough to follow him ; not ten per cent of these knew anything of physics or mathematics ; and of these again how many could understand the psychological assumptions that lay deep at the base of the doctrine ? I hesitate to answer because I do not wish to discourage anyone. At the University of Berlin it had fallen to my lot to study these three subjects, and it had been an essential part of my original work on psychology to free myself from the shackles of that Kantian psychology that, I perceived, still marked the limits of Einstein in that domain ; therefore I knew at least what he was saying.

The discourse was divided into three sections that had no visible connection with each other ; one dealt with those psychological, or metaphysical conceptions, the second with physical hypotheses, such as came within the Galilean scope of things, and the third with Einstein's tenuous theories and his tentative explanations, for example, of such phenomena as gravitation.

I listened in absorbed attention, sometimes delighted with his philosophical detachment, at other times disconcerted by the want of cogency of his argument. In this I was reminded of a spectacle of a few years previously, when I had listened to Bergson speaking to a similar audience. His exposition had at times an appearance of great clarity, at other times it astonished me by a sudden flash of intellect, and again left

me in stupefaction at the futility of the reasoning. I may add that with Bergson it was precisely these passages that enhanced his reputation ; and it was the evidence of this want of discrimination between the gold and the dross that settled my opinion for ever with regard to the value of professorial pronouncements. I cannot say that the obscure portions of Einstein's address had the greatest effect at King's College, because so few understood even the lucid passages ; but nothing prevented the full concurrence of the professors with what he was saying, nor indeed diminished the value of their judgment.

After the lecture I met a great pundit, one of the few whose works are—and deservedly so in his own domain—'articles of export.' Feeling my scepticism in advance, he glared at me through his polished glasses, but—such was my 'courage,' I had learnt not to be daunted by the foibles of the great—I asked him in all innocence what he thought of it. He was one of those who understood the German, but little else. "I think," he replied, with an air of wisdom such as no man ever really possessed, "I think that there is something in it, something in it that demands thought ; and that, where well founded, should be accepted !"

I never heard one of the professorial audience, either then or at a subsequent time, ask the simple question, as to what was the nexus, if any, between the three phases of Einstein's discourse, though they might plead extenuating circumstances in that none of them had observed that there were three phases in the exposition. The matter is, however, one of importance, for whereas the nudity of the whole doctrine would have become evident if any one of these were regarded as essential,

and for that reason submitted to a rigorous argument ; yet when they were all combined, no matter how loosely, they appeared to lend each other mutual support ; and, in the midst of the ambiguity and confusion so created, the unsubstantiality of the theory was veiled. Here perhaps a man of science may interpose : Granting all that, why do you lay stress on the popular reputation of Einstein instead of the position of authority he holds among the body of scientific men ?

My answer is that I would be pleased to deal with the theory of Relativity on the sole ground of its scientific value, but the main part of its strength does not lie in its acceptance by men of science, but in its popular fame ; and, further, many scientific men, even some on the councils of the universities, and certain journals and magazines of science, are far more susceptible to current popularity, or even 'boosting' of press agencies, than they would care to acknowledge. This is as conspicuous in the case of Einstein, as in instances of men, of far inferior intellectual quality, who have by sheer force of popular 'boosting,' stampeded the learned bodies. It is necessary to overcome in the first place the presumption created in the minds, even of thoughtful people, that there must be something of exceptional depth, or luminosity, in Einstein's paradoxes ; for he is cited by learned men all over the world, and invited to lecture with authority at the great universities which close their doors so jealously on any kind of philosophy that runs counter to their own accepted teachings.

We touch here on an aspect of Einsteinism that is of a subtle and interesting character, and that places him on a pedestal side by side with his great inspirator,

Kant. Neither of these men is an exponent of orthodox views. Their methods and their conclusions alike are at variance with the forms and traditions of orthodoxy, yet the great force of their influence is derived precisely from a feeling that they form a buttress against anti-orthodox attacks. Why? The answer is not easy to give, but I have been at some pains to search out to the source its psychological import, and I find as the result of my reflexions this state of affairs. The main religions of humanity are in a parlous condition with regard to their essential foundation either in philosophy, or in mere practical consistency with truth, or in their application to the needs of the people.

With respect to the orthodox religion and its by-products in the form of academic philosophy, the enemy is science, and particularly that mode of science which the universities themselves pompously designate with a capital: Thought. That was the ground of the strenuous objection to Locke when he began to show the light of common sense in psychology; and however inept seem now the objections of Stillingfleet, I say deliberately that we are, except for a transposition of terms, living in the same atmosphere.* Locke was succeeded by the audacious Hume, who did not scruple to apply the keen edge of his criticism to the positions of Locke himself; and then Kant, awakened from his 'dogmatic slumber,' arose to refute Hume, and in especial to defend certain doctrines already assumed as sacred. This manner of entering upon a philosophic problem was not scientific, and, incidentally, Kant did not refute Hume. Moreover, if he had

* I shall later return to this point, and indicate the scientific basis of the study of psychology, and hence of philosophy at large.

refuted Hume he would not by that feat have laid the basis of a scientific method of psychology, because that method was lacking in Hume himself.

Kant's *Kritik der reinen Vernunft* (Critique of Pure Reason) is not an orderly exposition, proceeding from sure bases, and arriving by close argument at definite conclusions; it is an exposition of various metaphysical notions of Kant derived ultimately from the lees of the old theological disquisitions of the Middle Ages; but this is masked by a style, both loose and obscure, and also by the fact that, following Aristotle, he had rightly perceived the conditions under which the true method was possible, though he had failed to work out this method scientifically. His main reasonings, moreover, are not based on the faulty premises he had established.

All this does not at first sight seem a promising form of protection for authority, but orthodoxy has reasons that reason cannot know, and Kant became the philosophic high priest of much that he condemned. The great point in his favour was that he was the opponent of that deadly criticism which was beginning to sap the teachings of these same Middle Ages; and the circles of the cardinals, whether Roman or Protestant, joined together and hailed him as the enemy of their enemy. And so it happens that in Oxford of to-day, for instance, Kant is less a teacher of a living philosophy than an enthroned demi-god whose name is flung *ex cathedra* to stifle the efforts of intellects that have still remained intact.

Now all this is not remote from Einstein. On the contrary it not only explains the vague but powerful influence of his theory, but it affords the clue to some

of the philosophical, or psychological, conceptions that underlie his doctrine. The influence of Kant is still extraordinarily pervasive amongst scientific men, particularly in Germany and this country ; and in reading a scientific lecture intended for the public, such for instance as the official address of a President of the British Association, I have been accustomed to look for the tag of quotation either from Kant or from one of his disciples. These quotations seldom have any connexion with the discourse, and the references by the learned President do not necessarily indicate the veritable philosophic spirit ; they resemble rather the oratorical tricks of the statesman who, after composing a sophisticated speech, searches in Bartlett's *Dictionary of Quotations* for a more or less appropriate embellishment.

There is another reason for the popular appreciation of such a theory as Einstein's. The public delights in wonders, and the press, which offers a fair reflex of the public mind, encourages them in that predilection. It is difficult, as a rule, to obtain the insertion in a newspaper of a scientific article, and even when, on State occasions, the papers give some space to science they either invest the news with something that enhances it—the patronage of science by a titled person—or they add something astonishing, or fantastic, to eke out the news. The same difficulty arises with any reasoned form of philosophy. If it reaches sane conclusions it passes into the light of common day, and ceases to be of interest ; but if it asserts that a young man in Cambridge has created life in a test tube, whereas he has simply mistaken objects under his microscope, then a wave of enthusiasm traverses the world ; and devout

people, whose chief beliefs would be jeopardised by this discovery, are loudest in their hymns at the wonder.

And so with Einstein. It was not merely that he had improved on Newton; for when Henri Poincaré published his *Nouvelles Méthodes dans la Mécanique céleste*, which added refinements to Laplace's system, the man in the street remained indifferent to the scientific achievement. But Einstein! Ah, there we have the real journalistic tang, the marvel, assured of popularity. The man who can dispense with the ether itself, who can enable us to talk of space that is both bounded and limitless, and who can assure us that the relative motion of two objects has no effect on their relative velocity; that is no ordinary man! The attractiveness of these doctrines was also enhanced by the fact that they could be imbibed without the fatigue of study. I have heard everywhere, at Labour meetings, in theatrical circles, in the halls of the great, the chatter of Relativity among men and women, who had become 'advanced,' and emancipated from the tyranny of coherent thought.

Now this is absurd, I hear a serious person exclaim. Agreed. That is how it strikes me, but changing the terms a little, that is not how it strikes either the public, or the press, or the learned body of scientific men. The paradoxical character of his statements has been as much a cause of Einstein's fame as it has been of that of George Bernard Shaw, and an attentive, dispassionate examination of the facts has convinced me that in both cases the main factor has not been the deep intelligence, or the lambent wit, but the folly of the utterance.

When a journalist writes, for instance, that Freud is

the Columbus of the Mind, or that Einstein is the greatest thing that has happened since Christ, it should not be readily assumed that the author of the statement has examined all the philosophies, or even that he knows much of Columbus, or of Christ; he may simply be a young newspaper man, accustomed to the study of 'captions' and persuaded that the chief literary faculty is that of finding smart sayings. Yet I repeat, for there is no paradox here, it is the accumulated impact of such sayings that has at length forced the doors of the men of science.

There is still another cause that has contributed to Einstein's fame. He is a Jew, and I have been told by men of science in various countries that this fact has had great effect in bolstering up his reputation. If so, Einstein is not singular in this regard. In nearly every country scientific authorities extol, sometimes unduly, the virtues of their great representatives. It is accordingly quite possible that the pride of the Jews has found vent in an attempt, if not to understand Relativity, at least to amplify the mysterious boon it has offered to the human race. The Israelites have been eclectic, for they have emphatically rejected the opportunity of glorying in the fame of one of their race still more universally applauded, even if as little followed, as Einstein.

Let this be said, however, with no prejudice against this people, for without constraint of any kind I have admired the work in mathematics,—since we are dealing with that subject—of children of Israel far more gifted than Einstein; for instance, Jacobi and Sylvester, and Hertz. I once mentioned this to a Jew, one of the best read men in Parliament, and discovered

that he had never heard of Jacobi. But if the works of Jacobi—monumental in volume, delightful in the elegance of demonstration—were beyond the cognizance of my learned friend, what were they to the man in the street? What can the more recondite speculations of Einstein mean to the average man? The remark, sometimes made, that the popularity of Einstein is a hopeful sign of the interest of the people in the high feats of intellect is sheer humbug, if the use of so vulgar a word be permitted. What interest has the average man shown in the works of Cayley in this country, or of Hamilton in Ireland; or of Cauchy, or Poincaré, or Hermite; in the marvellous deep thrusts of insight of the young genius, Evariste Galois, or in the delightful elegance of the expositions of Hesse, or the profound searchings of Sophus Lie? Sophus Lie by the way hailed from Norway, but when I once mentioned his name in admiration to a Norwegian he thought that I had meant to allude to Jonas Lie, a popular novelist. Yet Norway is one of the few countries I know that has erected a statue,* and a fine one, to a man who was a mathematician pure and simple. Not far from the University of Oslo stands a remarkable monument destined to glorify to the world the intellectual prowess of the Keats of mathematicians, Niels Abel. But does the man in the street read Abel? Would it please a London music-hall audience to hear an allusion, by way even of an appreciative gag, to the study of hyper-

* In the grounds of the University of Kazan there is a bust of Lobatchewski, and thither one day, even amidst the appalling famine time, I wended a pious pilgrimage. There is a bust of Newton in Leicester Square, but Newton was not merely a mathematician; he was a member of Parliament, and he occupied official positions. But Abel died young, neglected, and, at his death, almost unappreciated.

elliptic functions? No. Then, let us clear our minds of cant and recognise what 'balderdash' is the current admiration of Einstein, even though heralded by Lord Haldane and trumpeted by Bernard Shaw.*

"Be serious," said Grotius; and that saying of his I admire as one of the great aphorisms so much that to

* The disciples of Einstein insist that no criticism of his theory is of any value unless it be undertaken by an expert mathematician. That may be true of the mathematical exposition of the theory, though, as I shall show, the mathematics owe hardly anything to Einstein himself, while those who have particularly developed this branch of the subject have not been Relativitists. Further, two of the chief sponsors of the theory who have most contributed to its popularity have known nothing of mathematics. Lord Haldane held a position of high authority in philosophy during his lifetime. He was the type of academic Cardinal, for, as may be seen now, he contributed nothing of real originality to the subject, and his fame was based on a translation of a work of Schopenhauer's (*The World as Will and Representation*), which is itself devoid of scientific basis. The influence of such thinkers as Lord Haldane, no matter how well-intentioned they may be, is worse than useless, for they encourage the merely conventional values admitted at the Universities, and close the doors even to great work if it runs counter to the academic teaching.

As to Bernard Shaw, what shall I say? Here we have an extraordinarily alert and bright mind, but in matters of philosophy without education on right lines, though packed with misleading learning. My great admiration for his wit, and for the salutary effect of his social criticisms, has made me tolerant even of his foolish paradoxes though I have noted at times with pain that these are the chief sources of his celebrity. Except in the sphere in which he so bitingly shows the social inconsistencies of the self-righteous, he has little original thought, or he would not have submitted so tamely to the impress of minds so far from first class as Ibsen, Sydney Webb, and Bergson; I will not say Einstein, for though he is a champion of the master he may be forgiven, for he knows not what he says. He is here simply, ah—no, I will not call him a half-wit, I was about to use the word but, if an Irish bull be permitted, I withdraw what I have not said, out of sheer homage to the man who has given us so many flashes of real wit, and whose goodness of heart is so appealing. As Dr. Johnson said of Swift: No bad man could have written so much nonsense. But in regard to Einstein what I have meant to indicate was, that of the resources of his intellect Shaw has not brought to bear more than the moiety either to the consideration of Einstein's theories, or to the actual expression of that theory which he has broadcast to the world.

his statue in Delft I also paid a pious pilgrimage that I might gaze upon his thought-laden countenance. But all through this discourse I have been entirely serious, at least in intention, for in combating Einsteinism, which is already being erected in a sort of mystical cult, I must candidly face the main forces of the enemy. These are not to be found in the scientific side of the theory, but rather in the nebulous cloud of intangible conceptions that his name evokes; and I must repeat this at every turn, these psychological forces have greatly influenced scientific men.

But now there rises within me the cry of the passionate desire for truth, the searching for the reality of things, the love of science, all that has been the driving force of my efforts in the world of ideas. There was a time when I thought that science itself might be an ethical motive so wide and high as in itself to satisfy our conception of an ideal to which one's life might be devoted. But since then I have been intimately acquainted with the inner wheels, and seen the idols of the men on whom the flame of science was bestowed, and known the limitations of their morale. I have found so repeatedly that the forward march of science has been blocked by scientific men, that now I strive to clear the ground, to let in the light into the falsities of outworn systems, to breathe pure air in the philosophy of things; for I believe that the value of science cannot be measured even by its vast utilitarian products; but that rightly conceived its greatest mission, and its veritable reward to the scientific mind, is found rather in a quality that, if the word had not already been so misused, I would call 'spiritual'; that sense of inward enlightenment, of the strengthening of the highest

faculties, and ever as the frontiers of our knowledge are pushed towards wider horizons, the vision of the world opening up to new wonders, new alluring prospects, and new rewards of the aspirations of man.

These visions are clouded by the spirit that makes of certain scientific circles merely close boroughs of material interests; by the lowness of ethical conception that makes science a stepping-stone to social vanities, titles and honours; that makes it more advantageous to a man of science to flatter the great than to give twenty years to noble work; that makes these men conform, log-roll, and play the sycophant, or on the other hand invest themselves with the *morgue académique*, and apply to science what may be called sectarian or political tests. Yes, and often I hear the reply: that may have been the case once but not now; we, the dispensers of merit, may wrap ourselves up in the robes of self-righteousness, for we are, if not free from fault, certainly above criticism.

The atmosphere that I have indicated, however, is stifling in the universities, and the spirit of sectarianism, and of false patriotic glorification, vitiates the work of the British Association, and permeates the scientific journals; and as one consequence in this country, more than in any other, it has made Einsteinism orthodox. How mean all this will seem in future times! How mean therefore it is now.

And withal the Authorities are not on the side of Einstein. A distinguished member of the French Academy of Science told me that when it was proposed to elect Einstein as a corresponding member only thirteen votes out of over a hundred were cast in his favour. It is true that the thirteen comprised some

eminent mathematicians, but I will reserve comment on this point when I come to consider the real connexion between mathematics and Relativity.

One of the most noted mathematicians of Europe, M. Picard, has delivered a lecture which in brief space indicates the nature of the mathematical aspect of Relativity. M. Picard; who is M. Picard? He is, I grant, not as well known as Einstein, but his accomplishment in mathematics is vastly superior. I am still seeking in fact for anything that Einstein has added to mathematical knowledge. M. Picard, one of the keenest in analysis and most lucid in expression of the mathematicians of our time, has published his lecture, and I know few works so keen in its irony as this brochure. I found the charm of that style so great that I would like to have kept within the same vein, but I found that by the very deftness of the subtle play of his rapier he failed to make the gross impression necessary in dealing with popular delusions.

Three great French mathematicians, Henri Poincaré, perhaps the most celebrated of his nation since Cauchy, G. Darboux, who received the Nobel prize as the most widely versed of all the mathematicians, and with him, P. Appell, of vast learning and great clarity—to mention but a few—all had the opportunity of examining Einsteinism, but none of them accepted it. Klein, in Germany, the friend and in part the collaborator of Sophus Lie, both of them disciples of Plücker, was critical of Relativity from the point of view of mathematics. M. Paul Painlevé indicates the arbitrary assumptions in Einstein's theory of gravitation. M. LeRoux, by his persistent examinations of different aspects of Relativity, searching examinations that read like attacks, is the

most devastating critic of all. M. Bouasse, most learned of physicists, who is also an accomplished mathematician, writes of Einsteinism with a vital common sense that warms the cockles of my heart like a workman's stove fire amid a London fog. M. Guillaume, of the Academy of Science, is still more downright. The Italians Ricci and Levi-Civita, who have done most to develop the mathematical instrument used by the Relativitists, had no concern for Relativity.

The American physicists and mathematicians are in general the most critical of Relativity and this is true also of the framers of the case which is the corner stone of the theory, the Michelson-Morley experiment; Michelson rejected the Relativist theory.

Amidst all these there are the upholders of Einstein, some of them Frenchmen of science whose writings have given me delight for that especial French quality, lucidity—M. Langevin, for instance, and M. Nordmann, a more popular writer; but I have noticed this point that when they write of Relativity their lucidity vanishes, they have all the style, both heavy and obscure, that characterises a certain school of Germans.*

To the elucubrations in Relativity of some of the English physicists, Sir Oliver Lodge, for instance, and others, whom I will touch on later, I attach no importance. They talk like the Cardinals without their mystery, or Bernard Shaw without his wit. Professor

* Germany has produced some very great mathematicians, who, in addition to their national characteristics of thoroughness of research and completeness in exposition, have displayed also the captivating qualities of quick, keen-sighted intuition, and imaginative construction, and originality of method. It is a delight in the exercise of intellect to read Jacobi, Hesse, and Klein at their best; while there is a profound originality in others, Gauss, van Staudt, Riemann, in all of whom, though expressed with more sanity, will be found the germs of Einstein's theory.

Sir Arthur Eddington is much more serious, and I deal with him in detail though not in more tender spirit, for he has shown intelligence enough to have enabled him to escape from the web in which he is entangled.

Here I desire particularly to declare, though I have descanted on the question of authority, that is always to my mind the most wretched of pleas. Even if all the thinkers whom I have cited were—let us imagine it—on the side of Einstein, my own judgment would be unshaken, for I have formed it on grounds deeper than any advanced in the theory itself, and particularly in the close examination of those psychological questions which are involved in the doctrine.

Therefore, laying aside all authority, and only citing names, where I do cite them, in order to indicate the situation of any problem in regard to the survey of the whole theory, I will now proceed to a closer examination. In the first place I lay down, even laboriously, certain standards of judgment in psychology, for continual reference will be made to that aspect of the matter. Then I show and criticise the first form in which the doctrine of Relativity appeared; and I demonstrate its baselessness. I then indicate the germinal ideas as expressed in the first instance by forerunners of Einstein, but expressed by them without his implications. Incidentally I trace out in the main lines the course of development of the mathematical apparatus employed by Einstein and his pupils in the elaboration of the more extended, or generalised, form of his doctrine. I show that Einstein has contributed almost nothing to this part of the work; and I make it clear that the conceptions and theories of Relativity, and the

development of the special calculus taken over by the Einsteinists, have no mutual dependence.

Then I consider the so-called 'proofs' or tests of the theory. I show that these that have been invoked, up to the present, give no support at all to Einstein's positions. The correspondences cited are really not correspondences, and the methods by which they have been obtained are fallacious.

CHAPTER I

THE PHILOSOPHICAL BASIS

THE process of reasoning is in itself one of the greatest marvels of human life. It is true that it is not usually so esteemed, for it has become so familiar to us that we regard with equal nonchalance loose asseveration and rigorous argument, or even with an inclination to the first if it should accord with our desires. Good argument demands a close attention, a rare integrity of mind, and at times the compulsion of agreeing to something that clashes with our interests.

And here once more I remark, I am not straying from Relativity, nor even deserting the circle of learning. Demonstration! That is not a commonplace, however frequently met; it is a perpetual miracle, and to the philosophic man, the more he ponders on it, the deeper appear the sources from which arise the questions he asks himself. We start from certain premises, and we proceed, step by step, to certain conclusions, and when we arrive at these we have often lost sight of the intermediate steps; yet there are cases when justifiably we accept the conclusions, and cases when, unjustifiably, we hold to them with intense fervour.

But in that procedure already there is a world of meaning in the word 'step,' the step from one position

to another. What are the sanctions, the veritable grounds of our confidence, in so proceeding? And what is the final meaning of acceptance of our premises? This problem might in one aspect be called that of the analysis of reason, and I have found so much in it that I have devoted to it a great part of my intellectual life, one potent cause of difficulty being to clear the ground from the mass of false learning on the subject that constitutes the intellectual assets of our universities in the domain of Philosophy.*

At first the problem seems so extended in scope, and so peopled with myriad forms, and yet so abstract and elusive, that no sure beginning even can be made. But with attention we see modes of simplification. The material world is extremely various, yet it is built up of a few elements in certain forms of combination. Is there nothing analogous to this in the mental world? Yes, strikingly so. In the first place we should make a separation between the immediate impressions of the world of things that we observe, and the subsequent combinations of these impressions that we build up in our minds even when the first stimulus is absent; that is to say briefly—for right through in this chapter I am condensing the matter of a big volume—the distinction between objective and subjective forms.

Having observed the manner in which the impressions of the objective world reach us—that is to say by way of the senses, including all the senses—and having ascertained that the subjective images derive from the

* Rodin, the famous sculptor, once said to me that forty years of life were required before a sincere artist had debarrassed his mind of the academic teaching. If this be true of a domain where the objects of contemplation are concrete and graphic, how much more when they are recondite and impalpable,

objective source, we may, for our purpose, and without loss of scope or exactitude, deal with the objective world. Now, by virtue of its being built up of a few elements, we find incessant repetitions in the objective world, and moreover we see at length that even where certain objects are different—say a red disc and a blue disc—yet the act of looking at one of these has a part in common with the act of looking at the other. In other words we can make a separation between the pictorial or graphic appearances of objects—the ‘qualities’ of the old philosophers—and the quite distinct act, or process, of directing attention, as by looking at them.

In this way we are led to find that we can reserve our study to the examination of these ‘schematic’ processes in themselves. Then we shall observe that mental operations even of a very complicated kind are built up by combinations of such processes. Then at length we shall be brought to the point where we definitely pose the central problem of psychology: What are the final elements of mental acts, or as I have elsewhere expressed it: What are the Fundamental Processes of the Mind?

This inquiry involves also that which I mentioned at the beginning of this suggested analysis, the critical investigation of the meaning of acceptance of any premises such as the axioms. I cannot here give even a clear indication of the various steps of the analysis by which I sought to trace them down, and still less that of a very critical and difficult part of this work, the demonstration, in all rigour, that the Processes so formulated were not redundant and that they were so complete, in other words that, in the mathematical

phase, the Fundamental Processes presented were necessary and sufficient.*

To reach definite and certain conclusions in this other matter it was necessary to seek for new modes of analysis, and finally to arrive at sure principles of classification.†

Once in possession of the system of the Fundamental Processes we have in hand a powerful instrument in the analysis of any psychological problem. We shall discover, for example, that since these processes, and no others, are involved in all mental acts, then they apply to the methods not only of psychology but of all the sciences; and that is the meaning of the statement I have made elsewhere that psychology is the matrix of all the sciences. These sciences differ, in fact, not in the schematic forms of the operations necessary for the elucidations and developments that constitute them, but in the different objective forms to which these operations are directed, and in the wider associations of applications and of interests in which the study is immersed.

The Fundamental Processes should not be represented as a list; the proper image is that of a working machine, and the psychological aspect of that machine is correlated to the definite structural form of the

* The whole investigation is given in my *Principles of Psychology*.

† By no other means than by a principle of classification, in whatever form it may be exhibited, is it possible to be sure that the whole field had been covered. Euler, for instance, pointed this out in regard to Newton's list of the curves of the third degree—"he lacked the principle of classification." That principle of classification was afterwards supplied by Plücker, and the defects of Newton's system became evident. And so I say, in complete objectivity, the classification that I here indicate will show the lack of validity of any system in disaccord with it.

brain, and at the same time to the dynamic activities of the brain.

With this proviso I cite as the Fundamental Processes of the Mind the following :

- | | |
|-----------------------------|----------------------|
| (1) Immediate Presentation. | (7) Impulse. |
| (2) Sense of Unity. | (8) Sense of Effort. |
| (3) Association. | (9) Hedonic sense. |
| (4) Memory. | (10) Negation. |
| (5) Agreement. | (11) Time. |
| (6) Generalisation. | (12) Space. |

This summary in itself offers but a partial account of the meaning of these processes ; for instance, the conceptions of Time and Space are certainly Fundamental Processes, but that arises in virtue of Time and Space being essential and universal conditions of all perceptions.

The bearing of these principles of psychology in such branches of science, false or true, as Relativity, are now fairly obvious ; but the influences I will touch upon are twofold ; the first is that of the legitimate use of the instruments of analysis so provided, and the second is the influence of the doctrines of Kant. These either directly expressed, or tacitly assumed, are dominant amongst the Einsteinists. I can see the shadow of Kant whenever they touch on psychology. Therefore it is necessary to deal with Kant. The origin of Kant's work was not a philosophic contemplation of reason as a field of scientific study, but a desire to save certain dogmas derived from the old theological disputes. In particular he saw the comfortable form and sonsie countenance of David Hume furnished

with the accoutrements of Auld Cloutie ; the horns and hoofs were there.*

In the course of his meditations he saw the necessity of discovering and establishing what he spoke of as the Categories, but what I have called the Fundamental Processes. In his endeavours, however, to get a grasp of these the reader will nowhere discover any sign of such engines of analysis as I have indicated. He had found no principle of classification. He had not only not found a true principle of classification, but he had not even a method of search. All that he did was to grope in a tentative way, and finally to paraphrase Aristotle.

This leads me in turn to consider Aristotle himself. I never approach the great Stagyrte without a salute of deep respect, even though throughout the scope of his wonderful work one meets with lacunae, misconceptions, even gross errors. Yet if I could find among the moderns in this country even a fair leaven of that spirit of truth, the single-minded devotion to science as science, I should not be compelled to speak of our learned Societies and universities in the tones of asperity which the false spirit has wrung from me.

Aristotle, whose main object in this field was to raise up the great structure of his Ethics, saw clearly the necessity of establishing the psychological foundation according to such terms as I have already indicated. In science he was primarily a biologist, not a psychologist, and moreover he could not afford to spend much of his time in recondite studies of which he would not perhaps be able to win the scientific fruit. He therefore did the best he could, without a complete and

* Kant, it should be mentioned, was of Scottish origin, the name of his Scottish grandfather having been Cant.

exhaustive analysis, and, though in a tentative way yet with a mind of rare judgment and perspicacity, he set down his 'Predicaments.' It has been said that he found the suggestion of these in current works on grammar. This is absurd; Aristotle was far ahead of his contemporaries in such speculations; his predicaments, though imperfect, represent an epoch in this study.*

Kant is said to have taken his inspiration from works of formal logic; but that seems to me as wide of the mark as the ascription of Aristotle's feat to the grammars. All that the Königsberger did was to transform Aristotle's list and to express it in a special language. He himself did not build on the foundation he thought necessary to establish, but for reasons already given his influence spread through Europe, and still remains resplendent in the psychological side of Einstein's errors.†

* It is to be noted that in default of the solution of the central problem, all study of psychology must be tentative, and except in details, futile; yet after Aristotle I find precursors only in Kant, and at a later date Renouvier and Hamelin. I have received this indication from M. Dugas, formerly professor of philosophy at the University of Rennes, whose own work in philosophy is large in scope and lucid in expression, while the style in such studies as permit a less formal manner has all the Pascalian charm.

Both Renouvier and Hamelin have improved on Kant; but they have derived from Kant. I too owe to Kant a real service: both in his *Kritik der reinen Vernunft* and in his ethical works—the first reeking with the fumes of sterile Schoolmen disquisitions, the other ending in worship of the Hohenzollern conception of the State—he repelled me from all respect for such methods and drove me to rely resolutely on my own resources.

† Not in the mathematical part. Kant has, amongst philosophers, a great reputation for mathematics, none the less that few if any of the philosophers who have offered their comments have been mathematicians. Among the mathematicians, his philosophy is especially praised. I have looked into both sides of his work, and having touched on the philosophy

The most famous of the Einsteinian derivations from Kant is that which has led to the confusion of Time and Space, while his pupils, accepting this confusion, have also confused Space, and separately Time, with the modes of measuring space and time.

Here then it is necessary to clear the ground definitely. The conception of Space, apart from the definition of any particular form of space, is so elementary and inevitable that it might seem unnecessary to expatiate at length on the matter; but owing to the influence of the studies in Relativity, I recently met with the proposition that the conception of Space was really a deduction from the branch of mathematics called Analysis Situs, as illustrated, for instance, in Riemann's theory of surfaces of complex connexity! The critic in this case was a serious and highly intelligent young wrangler, Maurice Kendall, and the criticism had therefore to be examined.

I say that an infant, innocent of Analysis Situs, or even of Riemann's speculations which Gauss appreciated as 'profound,' still has, and must have, a conception of space, the moment that he opens his eyes, and sees two separated objects. He must, if he has vision at all, see that the objects do not coalesce, and the meaning of that separation contains within it the rudimentary conception of Space. The objects may move, but something remains of the total conception that is not affected by the nature of the objects, nor by

already, I say that the mathematical part is not great, even as applied mathematics. He used this instrument to study the question of the tides, but he simplified the conditions so as to fit in with his inadequate mathematical resources. He was not content with his own results, for from time to time he changed the argument and arrived at different results.

the extent of their separation. It is true as my friend pointed out that you may have various forms of bounded space, or volumes of space defined by various forms of enclosure. These may, in their very first admittance to consciousness, be what I have called Immediate Presentations * ; but when we come to observe, and still more so, when we come to define the limiting boundaries, we are already away from elementary conceptions ; and even arguing on these lines, there must be something, comprehensible, about which we argue in discussing the form of its boundary ; that is the rudimentary form of conception of that Space which is indeed a condition of our faculties. It is true that the measurement of space introduces us to a new problem, and it then appears clearly that all that is implied in the 'common sense' view of objectivity involves exceedingly complicated factors.†

Already at the threshold we see that a grasp of psychological analysis may be necessary for a true understanding of what are called simple matters. Let us touch here on the measurement of Space, and through the Cartesian coordinates we shall soon be led to the celebrated 'Fourth Dimension.' ‡

* Cf. *Principles of Psychology*.

† In a chapter on Externality, in *Principles of Psychology*, I have entered into this with extreme minuteness.

‡ Lord Kelvin, who was impatient of psychological questions, and proud of being 'practical,' declared that science did not begin till measurement was introduced ; and the scientific world said, Amen. Once more I dissent. As we cast our eyes over the range of science we find again that the most notable epochs in the advance of science have been those marked by the discovery of 'qualitative' facts, apart from quantitative relations.

When Empedocles, more than two thousand years before Darwin, offered a clear statement of the principle of Natural Selection, and when the author of *Origin of Species* gave a more explicit and detailed account

Space, as space in the immediate, and inevitable, rudimentary form of our conception has no dimensions; that is to say no resort to consideration of dimensions is necessary in order to give the impression conveyed to us. That is evident from the fact that the conception is immediate and inevitable, whereas notions of dimensions and measurement imply some complications and experience, and an intellectual development that is not essential to cognizance of space.

In a later chapter I shall indicate rapidly the development of mathematics which has led to the form generally employed in the Relativity theory; but for the present it may suffice to say that, given a framework such as the three walls of a room meeting at a point—

of the theory, they opened new fields of science without the aid of quantitative relations. The first enumeration even of Mendel's law required no mathematics except the most rudimentary. When Harvey demonstrated the circulation of the blood he required no quantitative expression, nor did Malpighi when he added precision to the theory by showing the circulation through the capillaries. Bell gave an immense impulse to physiological science when he made clear the distinction between sensory and motor nerves, but he had no need for mathematics in the case; nor did any of the research workers who developed our knowledge of the structure and the functions of the brain; and indeed a similar statement might be made in regard to the essentials of physiology. The fact of the existence of gravity was known to the Greeks, and later to Galileo, who indeed sought to obtain the law; but the essential point was the recognition of such a force.

The observations of Rumford, on the heating of the barrels of guns by friction, which led to the science of thermodynamics, were of the qualitative order. When Davy produced potassium by electrolysis he obtained his result without calculation. The first conception of the atomic theory was of a qualitative kind. The discoveries of the thoracic duct, the lymphatic system, the Eustachian tubes, the Fallopian tubes, the Graafian vesicles, the development of the cell, and scores of other great achievements in physiology and biology, from digestion as observed by Pawlow to parthenogenesis studied by Yves Delage, had no dependence on calculation. Nor did the principal observations in bacteriology, from Spallanzani to Schwann to Pasteur and Koch, who first demonstrated the existence of the tubercle bacillus. The same tale is true in scores of researches of great value in medicine; the study of the func-

which may be called the origin of coordinates—we may define the position of any point in space by virtue of its distance from the three walls. The numbers of units of measurement corresponding to these distances represent the coordinates of Descartes. It is evident that any point in space may be indicated by these coordinates. If we are dealing with points on a flat surface, a plane, it is only necessary to have as a frame two straight lines at right angles, their intersection being the origin of coordinates. In this case only two coordinates, defined by the distances of a point from the straight lines, are necessary to determine that point. This case is a simplification of the first, and for the

tions of the various glands, and the work of Manson and Sir R. Ross on the cause of malaria.

Coming to Lord Kelvin's own subject, Ampère was a profound mathematician, but in his path-opening experiments in the reactions of electric currents he did not require calculation, nor did mathematics enter into the subsequent discoveries of Oersted and Faraday and Henry. Faraday, whose researches led to immense developments in the applications of electricity, was not a mathematician. One could cite throughout the whole scope of the sciences hundreds of instances of the kind, but I will point the matter still more definitely by saying that, even in mathematics von Staudt, who detested symbols, established a method without their aid, and when Descartes founded his system of the coordinates, thus bringing into activity a new era of mathematical science, he required, in the first genial conception of the principle itself, no aid from calculation.

Heaven knows I do not mean to disparage mathematics; I simply desire to show that the pronouncements of great authorities should be subject to, and should be able to stand, the test of examination. Lord Kelvin's own work that bulked so largely in his lifetime consisted almost entirely in the application of known principles, and, owing to a lack in himself of that speculative, 'unpractical,' quality which he despised, he has added nothing, as did Ampère, Oersted, Faraday and Henry, to our knowledge of the nature of electricity. Calculation is, however, of the greatest value in making our knowledge precise in regard to the reactions of forces, for, as Darwin remarked, mathematics seem to give an added sense. Finally there are cases, though relatively few, where, as the result of mathematical investigation, new principles have been brought to light; and these are amongst the most fascinating passages in the whole history of science.

plane two coordinates are sufficient and necessary ; for space in the more general case, three coordinates. These coordinates may be called x, y, z .

So far we have supposed the point at rest, but if it be in motion, and if we know the law of its motion, which will depend on time, then starting with the point in a defined position, and then considering it moving according to its law of motion, we can assign its position at any instant of time denoted by t , t being simply a measure of time dating from some chosen zero of time. We have now three coordinates x, y, z , and a time measurement.

Lagrange in an excellent work, *La Théorie des Fonctions*, calls x, y, z dimensions, and having introduced the term t , he speaks of t as a dimension, the fourth dimension.

All this is crystal clear, and, certainly, there is no mystery in it ; but even before the Relativitists got to work, philosophers and mathematicians reasoned in this style : to two dimensions corresponds a plane ; to three dimensions corresponds a volume ; what corresponds to four dimensions ?

To this question fantastic answers have been given ; my own statement in reply is lamentably tame. There is no correspondence, for the question has no real meaning. I will give the reasons for that reply, but in the meantime I will discuss some of the imaginings of the philosophers. Felix Klein, a brilliant German mathematician who died recently, said that in a fourth dimensioned world it would not be possible to tie a knot. He did not give his reasoning, I believe, but it is possible to supply an argument. In the plane, if an obstacle be interposed across the line which a moving

point must take, the point would run into the line. But if the point were suddenly granted the freedom of a three-dimensional world it could hop into space unimpeded by the line. If now in space a moving line, as a cord, were obstructed by another line or cord, the lengths crossing it would not be able to avoid the contact; but if we brought the whole system to a four-dimensional world, then by analogy its course would not be checked. We could not therefore tie a knot.

All this is difficult to conceive; it is all imaginary, as Klein, who was not a Relativist, would admit. It is in fact impossible to conceive. A suggestion as to how a conception of a fourth dimension may arise is sometimes given thus. Imagine a minute animal perfectly flat on a plane; it could move on that plane and would suppose that no other world was possible. Yet if it could be transported to another plane from which it could view the first, it might gain a conception of a third dimension. All this is fantastic, for no animal can even be imagined as consisting of breadth without depth, and depth is already a third dimension; and moreover no animal could move on a plane without the use of a mechanism, and that implies a third dimension.

Then comes in this suggestion that I heard offered by an eminent man of science: Imagine an extremely flat animal; its depth could be expressed as an infinitesimal quantity, and it would have only a rudimentary notion of a third dimension. Now is it possible that we have a rudimentary sense of a fourth dimension that may be capable of development? Thereupon the savant,—he was French—imagined various forms of space which seemed to admit as by a crevice some escape from our common-plane space. For example,

if two prisms have all their sides and angles equal it may be expected that they could be brought to perfect concordance, but suppose you put a prism before a mirror and taking the reflexion of the prism as a second structure, you could not bring these two prisms to concordance; the left side of one would reproduce the right side of the other. But take this experiment in its elementary form. Place before the mirror a thin straight rod touching it at one point, but with an inclination away from the mirror. The image will touch the mirror at the same point, but the inclination will be away from the mirror in the reverse direction, and the two rods could not be brought to coalescence unless the inclination from the mirror vanished.

Now all that is interesting, but it is all explicable on the terms of ordinary space. For this instance you draw a horizontal line as datum, and then above it an inclined line, and then plot out another line below it at the same distance in each pair of corresponding points, you have the effect of the mirror, and for the same reason. Devices of this sort will not enable us to escape from our familiar world. But—I have the story from the same distinguished savant, M. Chaumat, President of the Society of Electrical Engineers—M. Pierre Curie, who helped the famous Madame Curie in her researches in radium, thought of building a room arranged all in mirrors in such a way as to destroy even the sense of up and down. He argued that our habitual modes of thoughts become so incrustated into our system that they produce a kind of intellectual ankylosis,—a stiffening of our mental joints. If we could get rid of this arthritic condition we might find movements that would lead us to the fourth dimensional Heaven.

Curie experimented, but after a time discontinued the experiments. "They were leading me to mental aberration!" he said.

"But," cried my friend, "it is just there that you should have persisted!"

Now, many of the Relativitists have persisted.

There is still another theory current, generally ascribed to Minkowski, and that is, that time is the fourth dimension, not in the sense of Lagrange, cited above, but in some transcendental conception. In this form I have traced the suggestion to a physiologist and psychologist, Czermak, who, however, was not a Relativitist.

Let us therefore, apart from all this, consider Time; that is to say the conception of time as one of the Fundamental Processes, or rather as a universal and essential mode involved in all the Processes. If there be a faculty of perception at all, and the most rudimentary form of thought, then we have the Immediate Presentation of some sort of image or impression, then another, the second rising into clearer mental comprehension as the first begins to sink. If there be Memory—and without Memory there is no thinking—these two appearances are linked in Association. The very fact that there is such a linking of this immediate and inevitable character implies Time. We say that time flows, or that there is a sequence, and even if these terms be contested, we should only have to seek other terms to indicate that which it is beyond our power to destroy; the peculiar nature of the association of one, and then the other, occurrence, whether it be image or impression or anything else. All this has a relation to the measurement of time similar to that of the relation of rudimentary space to the measurement of space.

In the developed and complicated forms in which we usually carry on our thoughts, relations between measurements of time and space are continually being noted, but the two concepts are essentially distinct.

I have insisted on this, simple as it is, because I have met with definitions of time just as complicated and remote from the first natural conception as Riemann's connexities of space from the simplest concept of space. They have in fact both proceeded from the same cause, the influence of a high education in special branches of mathematics. One definition that has been offered is this: Conceive of two skew curves in space, and imagine them meeting and crossing at a point; then if their mutual relation be expressed in the form of a function, the differential at the point of contact is the measure of time. I submit, however, that even if this be so, it is more difficult to seize than the ordinary notion of time; a child of three speaks quite intelligently of time, but not every child of three could grasp the above definition.

Having now cleared the ground in this way I will come to closer grips with the Einsteinists with regard to their confusion of Time and Space. In the first place I remark that the explanations I have offered are brief abstracts of the modes of thoughts by which I have established these positions. On the other hand I ask the Relativitists, or their disciples, where, either in their own writings or in those of Kant, is there anything at all to show that they have undertaken such analysis. There is nothing except mere assertion, and I, in turn, have made clear that if a philosopher confuses Space and Time, or attempts to make these concepts coalesce, or fails to distinguish between them and

the means taken to measure any volume on the one hand or duration of time on the other ; then such a philosopher is either superior to humanity, or he is deficient in the required acumen in this domain ; and I see no signs of superiority to humanity in the expositions of the Relativitists.

That being said, let us return to the contemplation of the Fourth dimension. Apart from the lucid expression given by Lagrange in taking time simply as a fourth parameter, or index of measurement, there is no fourth dimension. I interpolate here, however, that, as we shall see in the discussion on the development of mathematics, there is a manner, avowedly a conventional form of speech in the view of the authors * who introduced it, by which we can speak not only of a fourth dimensional space, but of a space of any number of dimensions.

But there is no disability of our faculties implied in our failure to conceive a fourth dimensioned space ; our faculties may be perfectly healthy and brought to the highest pitch of their efficiency, but in our whole structure as human beings we are limited to certain modes of thought ; the Fundamental Processes, for example, are not matters of our choice, they are inevitable.

When a wave of ether strikes the eye it sets up a train of events of which one result is a sensation, for instance, of a certain colour. If that wave struck a photographic plate it would produce an impression of another sort ; and if it struck a mixture of hydrogen and chlorine it might result in an explosion. But to ask the question, what would we think of blue colour if

* For example, Plücker, Sophus Lie, and Riemann,

we were a photographic plate, or if we were a test tube full of hydrogen and chlorine, has an air of the absurd. Yes, but to search for conceptions in our organisms that appertain to something essentially different does not seem to me more reasonable.

We now come to a conception which has had important effects in the theory of Relativity, that of the Space-Time continuum of Minkowski. I do not believe that the phrase has even any meaning. It is true that in a mathematical investigation we may group together the quantities that measure disparate things. Descartes, the great eponym of modern mathematics, tried to form an entity of the product of force and momentum, but this proved to be the least fortunate of his inspirations. In the search for solutions of algebraic equations also a school was formed which advocated the 'combinatory' system, in which simplifications were sought by the grouping of various terms. Now, the instances could be multiplied in which, in certain circumstances, these devices are successful; but it is both unscientific in itself, and contrary to the spirit of algebra, to stereotype these forms at the expense of the simpler components of which these complex terms are built up. It is as if, instead of using an alphabet, we insisted on building up a written language by signs representing phrases; these are useful at times, but at others their employment is not possible, whereas by the continual combination of the elementary forms we can compose any forms, including of course those which it is sought to stereotype as fundamental. What is said here of the alphabet is, of course, true of the Fundamental Processes. That being rendered clear I reserve the Continuum for a further discussion.

CHAPTER II

THE EMPLOYMENT OF MATHEMATICS

THIS chapter is preparatory to a later discussion on the mathematical apparatus employed by the Relativitists; in the course of the story certain ideas will be noted from time to time as showing the germs from which have arisen the more paradoxical forms which we meet with in the theory. As usual in all research for the origin of ideas that have become potent in the civilisation of the world we come to the Greeks of old. They were certainly the first great mathematicians, and they cultivated this science assiduously and with wonderful success from Pythagoras to Archimedes. Pythagoras, Euclid, Apollonius, Eratosthenes, Diophantus, Archimedes, Hipparchus; these are great names. It was to Archimedes that Galileo turned when the first rosy fingers of the dawn of the Renaissance swept away the thousand years of night that had overshadowed the Greek culture. Archimedes had reached a point where the modern forms began to appear clearly, for he had already worked in the spirit of the infinitesimal calculus.

Yet there were serious defects in the Greek system. In the first place they lacked a good method of notation, for the Arabic system which we employ was unknown to them. That is a very important point.

Then they devoted themselves, though with notable exceptions, too exclusively to geometric figures, and they kept their modes of demonstration too much within limiting grooves. The Greeks were curiously uninterested in mechanical instruments, and in invention; the reasons for this are to be found in part in ethical grounds and social habitudes, but in any case—since every man and every nation pays for defaults of nature—these deficiencies had serious consequences on the development of their mathematics.

During the Middle Ages the Arabs, who were the principal repositories of science, had developed their algebra. Here I may say that the distinction between different branches of mathematics is somewhat artificial. Geometry deals with spatial relations, and enumeration depends on the concept of time; so that here again we find these fundamental forms. Algebra is a generalised arithmetic, but what essentially characterises it is that in its methods it dissociates itself from all except what is essential to the problem; and it is the same principle that essentially characterises the modern branch of calculus used by the Relativitists. The study of algebra had been systemised and extended, principally by Vieta in Italy before the time of Descartes, but geometry and algebra had remained apart as distinct branches of learning.

Then one fine morning in Holland Descartes lay in bed meditating, as was his wont, when all of a sudden came that flash of genius which has gone far to change the face of our civilisation. He had been struck by the fact that he was looking at three walls, or planes,—the two side walls and the floor—which intersected at a point. The next step of observation was that a cer-

tain object in the room stood at measurable distances from the three walls, each in turn ; and then came the suggestion that, given these three distances, one could determine the position of the object. All this was so wonderful that Descartes leapt out of bed in an ecstasy; he had discovered the principle of his system of co-ordinates.

The lines of development became clear at once. For instance, imagine an ellipsoid. Take its centre as the origin of coordinates, that is to say as the point of intersection of three planes symmetrically placed with regard to the ellipsoid. Now to any point on the ellipsoid certain measures could be assigned, these being the distances of the point from the three planes. The measures could be expressed in numbers which, in a generalised manner, we could call x , y , z . These numbers stand for the coordinates. But as the ellipsoid is a symmetric figure one would expect a regular relation to exist between the numbers, and so it is found. We can reduce the relations to a form of this kind :

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} + \frac{z^2}{c^2} = 1$$

where a , b , c represent the length of the lines, symmetric to the ellipse, drawn from the centre in three directions at right angles.

Now the expression above is an equation, and this equation forms in algebraical language the very definition of the ellipsoid. In other words whereas the Greeks had employed geometrical forms in the study of their spatial figures, and the Arabs had regarded their algebra as a special branch of mathematics, Descartes indicated how algebra could be utilised in the

study of geometry ; and as algebra had become fairly well developed he was able to bring this powerful apparatus to his aid in the study of all spatial forms.

Naturally the impulse given to mathematics was immense ; but here it may be mentioned as a fact for deep and curious consideration that Newton when he first looked into the system of Descartes found nothing of special interest in it, although it was destined to be the foundation of a great part of his own work. Descartes is, however, doubly interesting to us in the discussion of Relativity, for at one time when the Inquisition was becoming uneasy about his scientific researches, he gave them a reply that satisfied them, or perhaps he merely gained time, which was long, while they were trying to understand his meaning. He declared that the sun went round the earth, and that when he said that the earth revolved round the sun that was merely another manner of expressing the same occurrence.

I met with this saying first from Henri Poincaré, and I thought then that it was a witty, epigrammatic way of compelling thought to the question ; but on reflexion I saw that it was a statement of actual fact. The movements of the two bodies are relative one to the other, and it is a matter of choice as to which we take as our place of observation. When, however, we consider the whole solar system, then, taking the sun as our standpoint, or origin of coordinates, we get a much simpler expression for the relations sought than if we take the earth. We get the expression of a law of nature.

And now I touch the pith of the matter, where I am able to offer an excuse at least for not immediately

accepting Poincaré's expression. Those who regard it as indifferent to say that the earth revolves round the sun, or the sun revolves round the earth, are apt to proceed further to the assertion that 'laws of nature' are mere convenient, generally mathematical, phrases. There I join issue, for I believe the confusion is due to what we have already noticed, the fault of considering that the measures of objects or relations can be identified with these objects or relations.* The law of nature operates whether we know of it or not, and when at length we discover a mathematical mode of defining it the law does not disappear.

Poincaré himself was never in error by reason of such confusion, and I accept what he said without reserve; but the Relativitists take pleasure in insisting on their erroneous view. So also, although we may all accept the Cartesian view of Relativity in this particular, yet the tendency of the Einsteinists is to force these expressions into a meaning that the mathematicians who originally propounded them did not intend. Here it may be advisable, to avoid misapprehension, to say that I am not writing against relativity at large, for all motion is relative, and all mutual interactions of bodies are relative; I am only applying my criticism to the doctrine that Einstein and his disciples designate as Relativity.

Descartes did not himself pursue very far his work on mathematics; he cast his seed, as he wittily

* I find the fault in its most subtle form in an excellent book, very serviceable in the study of the mathematics of Relativity, Otto Veblen's *Invariants of quadratic differential forms*, where he seems to identify a vector with the measures that serve to determine the vector. There is always a real difference between the qualitative thing and its quantitative assessment.

expressed it, into the garden of others. These others threw themselves with great ardour into the study, and we soon had an era when mathematics became the fashionable, or the sacred, study.*

Euler, whom we have already met, gave an enormous impetus to the study, so much so that when he dallied with philosophy, or metaphysics, Daniel Bernoulli reproached him, and besought him to return to those 'divine things,' mathematics.

Soon after the introduction of the Cartesian coordinates, an event in scientific history took place of great importance—the invention, or discovery, to use the favourite term of Hermite, of the infinitesimal, or differential, calculus. This subject has always been kept apart as something not only important but mysterious. I have no doubt that it impressed its contemporaries in the early days as wonderfully as Einsteinism does in ours, except that it laboured under the disadvantage of being intelligible and real. Be that as it may, it soon set the learned world raging with passion, and the bitter quarrels over priority have their echo even in our own time.

It is considered patriotic to assert with vehemence, but not necessarily with knowledge, that Newton invented this calculus. The invention was, however, gradual. Archimedes laid the foundations. Pascal had the principle but just missed the necessary forms of enunciation †; but previously, according to Lagrange,

* These fashions in science are really as marked as those of Spring bonnets. Once it was Evolution. Now it is Einsteinism, and simultaneously the most unscientific form of perversion, psychology *à la* Freud.

† Pascal when he devoted himself to religious speculations looked upon mathematics as of comparatively trivial interest. A great genius was lost here.

Fermat, who was contemporary with Descartes, had a clear vision of the matter. But the great rivals were Newton and Leibnitz, and their respective claims excited the multitude as in a later day the emulation of Sayers and Heenan, and the language used was hardly less violent. Certain it is that the notation of Leibnitz prevailed, because it had a more general reference, and was more directly expressive of the realities concerned than that of Newton, whose notation, however, has been universally adopted in special expressions involving time.

Mathematicians still invest this Calculus with mystery, and perhaps excusably, for it differs from algebra essentially in the employment of limiting forms of 'infinitely' small or 'infinitely' large quantities; and, since the veritable meaning of 'infinity' in this regard required a special analysis, and the recognition of the discontinuity of the operations of the mind to which allusion has been made, while few of the mathematicians entered upon such considerations at all, the instrument must have had a mysterious air even to those who used it deftly.

Is it possible, considering that mathematicians sometimes study for long without clear apprehension of the principles of the differential calculus, to give in a brief space to the intelligent layman an idea, however rudimentary, of the principle of its action? This is advisable for the consideration of some of the subsequent chapters; therefore I will, after commending myself like Esther to a tutelary power, and facing the worst—"If I perish, I perish"—make the attempt.*

* Those who have no time to follow an explanation which asks for close attention are invited to skip the following pages up to p. 34.

Suppose that we had to add an infinite number of things of finite magnitude, even if they were so concrete as loaves of bread, we should simply get a vague and undefined infinity. Yet it is not in all cases hopeless to add together an infinity of magnitudes. If in the first place we were to divide a loaf of bread into ten parts, and then add these ten parts, we should get our loaf again. The same would be true if we divided it into a thousand parts and added the thousand of them. And the same would be true no matter what number, no matter how large, we took in place of the thousand. So far we are on the earth. But now comes the eerie flight. Suppose that we had an infinite division and took an infinite number of the parts! Would the same result be obtained? Yes, providing that the 'infinity' itself were defined, in the division process, and that the corresponding definition held in the summation process. Well, there in the most rudimentary form you have the principle of the infinitesimal calculus, or as it is called, in reference to two operations, the differential calculus and the integral calculus. The integral calculus is the operation of summing together an infinite number of infinitely small things.

Hitherto we have supposed the small parts to be of equal magnitude, but it is possible to effect a sum of quantities which are not all equal; for example,

$$1 + \frac{1}{2} + \frac{1}{4} + \frac{1}{8} + \text{etc.}$$

When this series is carried on to infinity the sum is 2, for if we take the first two terms $1 + \frac{1}{2}$, the deficiency from 2 is $\frac{1}{2}$; if we add the next term $\frac{1}{4}$, then the deficiency is $\frac{1}{4}$, and so on. Therefore finally the

deficiency from 2 can be made as small as we please, and when we 'proceed to the limit,' or conceive an 'infinite' number of these terms, we take the limit as 2. This simple example suggests the attempt to sum up other infinite series of terms, when the successive terms diminish in value. Of course the terms should be defined, and the manner in which this diminution in value takes place should be defined, otherwise we should not know with what we were dealing. There must be a 'law' defining the terms.

To revert to our concrete example. We have seen that it is possible to effect a sum of an extremely large number of extremely small, but equal parts, of a loaf of bread. Suppose now the parts not equal, we may still be able to obtain the sum. For if the first slice represented the difference between two nearly equal loaves—that is to say a full loaf, and a loaf from such a slice had been taken—we could express its quantity by $A_1 - A_2$ where A_1 is the quantity of the first loaf and A_2 of the second. Now if the law of decrease of the slices were such that the second slice could be represented by $A_2 - A_3$, and the third by $A_3 - A_4$; and so on; then the sum would be

$$A_1 - A_2 + A_2 - A_3 + A_3 - A_4 + \text{etc.};$$

and a glance at this series shows that, taking together the terms $-A_2 + A_2$, and then $-A_3 + A_3$, and so on, all these terms become cancelled; and we are left with the first term A_1 and the last, which in the case assumed would be 0. In other words we have effected the summation, the result being A_1 . That is a truly wonderful result, the summation of an infinite series of terms which are not all equal, but which follow a

'law.' We may therefore be encouraged to proceed, for new wonders are in store.

Instead of speaking of a sum of terms we may place the letter S before the series, or so as to prevent misapprehension we may take the Greek S, Σ .

Thus in algebra $\sum_1^n a_n$ would mean a sum of terms where n is successively given the values 1, 2, . . . n ; thus, $a_1 + a_2 + \dots + a_n$.

The a 's are defined according to some law of progression.

Now the infinitesimal calculus differs from algebra only in dealing with the infinitely small, and infinitely large, as in the numbers of the terms, and in order to mark this distinction we use a differently shaped S; thus, $\int f(x)h$ represents a summation of infinitely small quantities represented by $f(x)h$. It was a stroke of genius to invent this \int , for nothing that I know has so effectually scared away intruders and kept the calculus unspotted from the world. Even Bernard Shaw, the bold champion of Einstein, has covered his wing before this symbol, for he has never ventured to attack it, or even to applaud it. It is to the profane more awesome than the Sphinx; and yet, here I say, it simply means S, for summation.

Now one or two efforts, and a little patience, and we gain a vision of the promised land. I have represented the infinitely small quantity by $f(x)h$. f simply stands for the word function. $f(x)$ means function of x . The word function is of very general application, but in technical language we say that if x be a quantity, $f(x)$ is a quantity that, as x varies, varies in accordance,

and remains constant while x is constant. Thus one's income tax is a function of one's income, and one's muscular fatigue is a function of one's muscular exertion. One of the simplest forms is x^2 as a function of x ; x itself may be called a function of x .

Suppose then that we put x in place of $f(x)$ in the integral $\int f(x)h$ we get $\int xh$. Now xh is an infinitesimally small quantity, and since x may be any finite quantity h must be infinitesimal. Note that we had taken $\int f(x)h$ as the sum of a series of quantities diminishing step by step. They diminish, step by step, by reason of a variation in the magnitude of x itself, since $f(x)$ is a function of x . Consider the case when the function increases as x increases, or as we say, when we give an increment to x ; in this case evidently the function diminishes also as x diminishes. The notion of an increment is familiar. If a man's income tax is a function of his income, then a certain increase or increment of his income would produce an increase in his income tax; and if we could suppose a state of affairs where the slightest increase of income was followed by a correspondingly slight increase of income tax, we would have such a case as is ordinarily dealt with in this calculus. All this explanation may be tedious but it is not abstruse.

Now if we give to x a slight increment, and call the increment h , the function will become $f(x+h)$, which we have taken greater than $f(x)$; and if we diminish x by the quantity h , we will have $f(x-h)$ which is less than $f(x)$. So that $\int f(x)h$ here means the sum of the series $f(x)h + f(x-h)h + f(x-2h)h + \text{etc.}$ The symbol h which

means a small increment to x , while $-h$ stands for a decrease, may now be replaced by dx , which has no other meaning than h , but has the advantage at times of indicating its relation to x ; so that now we may write: $f(x)dx$.

There is only one small addition required to give the symbol the form in which it usually appears. We may take the series as beginning with a certain value of $f(x)$, and then continuing with terms diminishing in value till we reach a term either 0, or differing from 0 by a quantity as small as we desire. But it is also possible to imagine a series beginning with a value of $f(x)$ obtained by making x equal to an assigned quantity b , so that $f(x)$ becomes $f(b)$, and ending at a term $f(a)$, where x has the value a . Under these conditions

the sum is represented thus: $\int_b^a f(x)dx$. If x decreased from b to a we should give the negative sign to dx , and this may be placed thus: $-\int_b^a f(x)dx$.

But how can we obtain the value of such sums, or, as we may call them, integrals. There are cases in which it is possible. Let us return for a moment to our concrete illustration of the loaves of bread. Here we may replace $f(x)$ by the quantity of the loaf; and since to divide a quantity by an extremely large number is just the same as multiplying it by the correspondingly small number—for instance to divide by 1000 is equivalent to multiplying by $\frac{1}{1000}$ —we may represent our former small slices by the symbol $f(x)dx$. We found it possible to effect a summation in that case, if we could represent the slice as the difference between the loaves, thus $A_1 - A_2$. Therefore the question arises, can we

apply this method to other quantities? In other words can we find another function, which we may call $F(x)$, such that $F(x) - F(x - dx) = f(x)dx$.

We cannot always do so; but in certain simple cases we can, and with these as data, we can by reducing the complex cases to more simple ones often effect the operation in the complex cases. Let us then try the

simplest of all cases where we have $\int x dx$; can we find a function $F(x)$ so that $F(x) - F(x - dx) = x dx$? Here for the purpose of a slight simplification in appearance we will again call dx , h ; and instead of $F(x)$ we will begin with a function of $(x + h)$, for which we may still use the same letter F ; so that, finally, the problem is, what is the function $F(x)$, such that $F(x + h) - F(x) = xh$; but that is the same problem as is expressed by

$$x = \frac{F(x + h) - F(x)}{h}.$$

To anyone who has become thoroughly familiar with the preceding explanations it will be possible to make a guess so as to obtain $F(x)$, but that process seems too hazardous. But since the infinitesimal calculus is a special form of algebra, and algebra is but generalised arithmetic, and the ordinary processes of arithmetic are susceptible of being analysed into simple processes, let us take a hint from arithmetic. If we wish to divide say 21 by 7, we are enabled to do so by having previously ascertained that 3 times 7 are 21. We store up a little repertory of results of multiplication, and we make use of these to obtain divisions.

Suppose then that, adopting this method, we put aside our problem at the point to which we have brought it, and exercise ourselves in the inverse problem,

which might be expressed thus : Take $\frac{F(x+h) - F(x)}{h}$, and give to $F(x)$ various values, and see what is the value of what we have called $f(x)$ in this case, h being taken as infinitesimally small.

If $F(x)$ were taken as x , then $\frac{F(x+h) - F(x)}{h}$ would become $\frac{(x+h) - x}{h} = \frac{h}{h} = 1$.

Here we have performed the famous operation of what is called differentiating. The limit value, when h is taken as 0, of $\frac{F(x+h) - F(x)}{h}$ is called the differential coefficient of $F(x)$; and we have found that the differential coefficient of x is 1, though in this special case the proviso that h should be reduced to the limit, 0, is not necessary. We have got a result but that is not the solution of the problem we had posed. However, take another case.*

Take now $F(x) = x^2$. Then

$$\begin{aligned} \frac{F(x+h) - F(x)}{h} &= \frac{(x+h)^2 - x^2}{h} = \frac{x^2 + 2hx + h^2 - x^2}{h} \\ &= \frac{2hx + h^2}{h} = 2x + h; \end{aligned}$$

or when we 'proceed to the limit,' where $h = 0$, we finally obtain $2x$. $2x$ is the differential coefficient of x^2 .

A very simple trial will show that x is the differential coefficient of $\frac{x^2}{2}$. Now revert to the problem which we posed: what is the value of $\int f(x)dx$ where $f(x)$ is x . We

* It should be noted, of course, that in this $F(x)$, the F has not the same significance as when $F(x) = x$; the F is a general symbol simply signifying function,

found that to answer this question we had to find a function, $F(x)$ such that $F(x+h) - Fx = f(x)h$, in the case where $f(x)$ is x ; and that is the same as finding a function, $F(x)$ such that $\frac{F(x+h) - F(x)}{h} = x$; and we have discovered that the function $F(x)$, in this case, is $\frac{x^2}{2}$. So that $\int x dx$ can be expressed as the sum of $\frac{1}{2}[(x^2 - (x-h)^2 + (x-h)^2 - (x-2h)^2 \dots)]$, the series being carried on till the last term becomes 0; and as all the intermediary terms such as $-(x-h)^2 + (x-h)^2$ become cancelled we get as the final result $\int x dx = \frac{x^2}{2}$; or if the summation be taken as that of the terms beginning with the value b for x , and ending with the value a for x ; we have, using the negative sign, since x decreases from b to a ,

$$-\int_b^a x dx = -\left(\frac{a^2}{2} - \frac{b^2}{2}\right), \text{ or } -\frac{1}{2}(a^2 - b^2)$$

In other words we have effected the integration, $-\int_b^a x dx$, or found the integral $-\int_b^a x dx$. This integral is equivalent to $\int_a^b x dx$, for x increases from a to b , and dx becomes positive. The value $-\frac{1}{2}(a^2 - b^2)$ is, of course, equal to $\frac{1}{2}(b^2 - a^2)$.

If this explanation seems to make undue demands on patience, I may plead that, from the early suggestion of the problem, as contained in the researches of Archimedes, to the solution as given, some two thousand years elapsed, and that even when the problem was taken up by the keenest minds from Fermat to Pascal, to Newton and Leibnitz, it required many years

to grasp its significance; and further, that the exposition that I have given is far from that of the historical development.* But what I have had in mind as of special importance in this connexion is, not only to give an idea of the nature of such operations as integration, but to rob the processes of the air of mystery in which they are invested. If any process of mathematics seems mysterious it does not mean that a great genius is at work; it means that there has been a failure to understand intelligible operations. Once the faculty of integrating fairly simple functions had been obtained the field became opened up enormously. This work was linked on to that of Descartes, and the areas, and volumes of geometrical figures became ascertainable by integration, and the ratio of the circumference of the circle to its diameter became expressible with as much precision as desired.

The new instrument was also turned to the solution of problems in mechanics, in astronomy, and in fact in all the sciences where phenomena were susceptible of measurement. All this is germane to Einsteinism, for

* Newton is spoken of as the man whose mind 'wandered through eternity' and this phrase has given a false view of the working of a mathematician's faculties. I much prefer the saying of Gauss that it is the obscure little corners of an exposition, or incomplete solution, that should be most searchingly explored, for therein lies the chance of discovery. Certainly imagination, a fertility of resource and invention, are excellent in a mathematician, but these, and their products, should be controlled, and subjected to the most rigorous examination; the concentrated light of the intellect should be directed especially to the narrow places where one step is linked to another. The mathematician should be as sure of what he is doing as the carpenter, or machine maker. When a mathematician produces 'mystic' results from his formulæ, he is not a super-mathematician, he is lacking in the acumen necessary for his work. This leads me to say that though great results were obtained by mathematicians whose conception of infinity were not clear, yet the theory could not be complete without the required analysis of the conception, infinity. Then one may proceed with certitude.

out of the consideration of infinity, a number of conceptions arose which have pervaded, in a false aspect, the theory of Relativity. One of these is the theory of parallel lines, and another allied to it, that of the curvature of space. The *Ausgangspunkt* (the Outgoing-point, or source), as the Germans call it, of many of the new speculations was the work of Gauss and his disciples. It was he who first definitely grappled with the question of parallelism, and the Hungarian Bolyai, the son of a pupil of Gauss, and the Russian Lobatchewski, set on foot the non-Euclidian geometry, which, interesting in itself, has been the subject of much amusing comment.

It is evident that if two lines are parallel, in the sense of keeping always the same distance apart, then if they be produced ever so far as straight lines on a plane they cannot meet. Gauss, one of the keenest visioned and most exact of all the mathematicians, never supposed they could; but a non-Euclidian geometry was possible. Briefly the matter might be expressed thus. If parallelism be defined as that relation between two straight lines that prevents them meeting, then it is a truism to say that they will not meet; but if the question be asked what are the conditions which make parallelism possible, we may take certain of these conditions and ask whether on any surface two lines that respond to these conditions will meet; then it might be possible.

For instance, imagine a globe with two lines starting, with an interval between them, on the equator and running as parts of grand circles to meet at the poles. These lines are each at right angles to the equator. It is true that they are not Euclidian straight lines, but

from point to point on those lines where the points are close enough the direction of the line is that of the shortest distance between the points.*

The non-Euclidian geometry of Bolyai and Lobatshewski was not an incursion into the realm of paradox, it arose from the legitimate and admirable desire to widen the scope of mathematical instruments in such a way that Euclid's system appeared as a special case of more general relations. Gauss himself worked always both towards wider generalisations and in search of the deepest foundations of the science ; and he gave the impulse to many developments that at first sight seem to have no connexion. It is one of his disciples von Staudt to whom is due the conception, which the Relativitists have played upon, that space is curved.† That notion is associated with a convention of mathematicians about parallelism, and it involves again the conception of infinity. It may be accepted that if two straight lines in a plane have an infinitesimal inclination to each other they will meet at infinity, and, as, usually, infinitesimally small is accepted as synonymous with zero, then it is assumed that two lines which have no mutual inclination, that is to say parallel lines, will meet at infinity.

In this apparent paradox there is a misapprehension

* This subject is very clearly expounded, on the basis of the work of Ricci and Levi-Civita, by Professor A. N. Whitehead. I am glad to be able to render him that homage, because as he is a Relativitist I differ from him on many grounds, but even my severest criticism of these theories has no personal animus. Finding the writings of men like Professors Whitehead and Eddington so acute at times, I feel inclined to say, what might they not have done if they had studied the theory of the Fundamental Processes of the Mind ?

† It is always hazardous to say, 'the first.' I have found the suggestion in von Staudt's writings, but have not been able to trace it further back.

of the meaning of infinity, for as we have already seen infinity cannot be used blankly as a multiplier or divisor; infinity is the limit of a sequence and the form of the sequence should be defined in each case. If then we are to multiply by infinity we cannot assume that the infinitesimally small—the result of division by this same infinity, is 0 absolutely.

Suppose that we have a horizontal straight line in an indefinitely extended plane. Then fix a point somewhere above that line, and draw another line of length unlimited, through the point, so as to cut the first line. Mathematicians, for the purpose of classification, say that one straight line must cut another in one point, and that it can cut the other only in one point. Now suppose the second straight line to be turned about the fixed point so that, let us say, the point of intersection of the two lines moves eastward, the less will be the inclination of the second line to the first. Continue the process till the second line becomes parallel, in the absolute sense, to the first. The mathematicians say that, by virtue of their law, the second line must still intersect the first. But where? At infinity. Infinity is here somewhat in the remote East. But, since the second line is now parallel to the first it is parallel also towards the West; and by the same reasoning it must intersect the first line, to the West side, at infinity. But our straight line can intersect another only in one point; at infinity, therefore the two points to the East and to the West must be identical. This could happen if, and only if, the two lines that we called straight were, or had become, curved. Think of them, for example, as two great circles, of an infinitely large globe, meeting at a pole of the

globe away at an infinite distance at the back of our plane.

But this remarkable state of affairs happening altogether against our will could be due only to a curvature of space. Here indeed is a wonderful discovery and all due to abstract science !

My own explanation is, compared with this refinement, of a merely vulgar nature. The result is not due to curvature of space, but to curvature of mind, a sort of intellectual scoliosis in the intellects of the mathematicians which has made them give crooked reasons. It certainly adds to the neatness of mathematical expression when we use such conventions—and there are many equally untrue in mathematics—that two straight lines in a plane must intersect. But Nature has a little kink in this direction, she does not bow to our etiquette, and when two straight lines are parallel she affirms tacitly that they neither intersect nor meet at all. I take my stand with Nature and against the Einsteinists.

Let us pursue a little further this conception of curvature of space, which is associated again with an idea derived from another disciple of Gauss, Riemann, that space is bounded but unlimited. What a God-send that was to Einstein ; for Riemann was a truly great mathematician, so great that he never allowed to his fancy the absurd twist given to it by the Relativitists.

The astronomers who have explored the Universe tell us that the actual bodies therein, organic or inorganic, are in bulk, compared with the greatness of space, like five flies in Europe ; then imagine, if you can—I cannot—any means of defining thus the contour or boundaries of Europe, the five flies not being

supposed to be on the boundaries. What is the meaning, if there be a meaning, of space being curved? Most of the Relativitists, be it noted, deny the existence of ether. Their rejection of this entity was perhaps too precipitate, though such fantastic nonsense has been 'demonstrated' by scientific men with regard to the ether, that any liberty is here permissible. One might imagine the universe as a sort of gigantic onion with successive layers of curvature; but even then we should not have to go to infinity to get our straight lines curved; they would be curved on our doorstep. Further it would not be space, but a material substance that is curved. And finally the troublous question would be forced upon our minds as to whether it were only straight lines that were curved. Could we use curved lines and by putting them into space get them straightened out, rescuing them at the moment they threatened to become curved again? I cease, lest my brain reel. I have not the courage to go on. There are regions of nonsense which I dare not enter; I am not a Relativitist.

We shall have occasion later to consider certain of the developments of the mathematicians that have a direct bearing on 'generalised Relativity.' Two of these, however, have a special interest at this point, Jacobi and Riemann. Jacobi, who was one of the most brilliant of mathematicians, said that there were two kinds of demonstrations that held possibilities of error: those that were so short that their cogency could not be tested, and those that were so long that the possibilities of false steps became multiplied. We shall have ample opportunities in the study of Relativity of meeting both these carriers of falsity.

CHAPTER III

THE MICHELSON-MORLEY EXPERIMENT

THE main part of the theory of Relativity depends on the experiment conducted by two American physicists, Albert Michelson and E. W. Morley, first in 1887; or rather the theory depends on a certain interpretation of the results found by these experimenters. If space permitted it would be of interest to consider this experiment in full detail, from all the points of view presented.

The suggestions that gave rise to it had been in the mind of Michelson for several years before he carried out what he believed would be a crucial test. The earth moves round the sun at a varying velocity, but it will suffice for illustration to set it down at 30 kilometres a second. The earth also rotates on its axis, and this rotation gives a velocity at the surface of about 465 metres a second at most. But as it is difficult to detect any effect, by the means adopted by Michelson and Morley, for a velocity of 30 kilometres a second we may leave out of account at present the tangential velocity due to rotation, for it is less than the sixtieth part of that due to translation.

Now suppose we were travelling on a broad decked boat at the rate of ten yards a second, and that we rolled a ball at the speed of ten yards a second along the deck, ten yards broad, in a direction at right angles

to that in which the boat was moving. The ball would reach the other side in one second. But if there were a buoy ten yards ahead in the direction of the movement of the boat and, from the same starting point, we rolled another ball in this direction at the speed of ten yards a second; then, with respect to the buoy, its speed would be that of the boat added to the speed which it had relative to the boat, that is to say twenty yards a second; and it would come level with the buoy in half a second. We might suppose in each case that the ball met with an elastic obstacle—in the second case outside the boat—which returned it on the previous line in the reverse direction; it is evident that the balls would not return to the initial point at the same time.

This is a very rough indication of the elaborate and delicate means employed by Michelson and Morley to ascertain whether an 'ether wind' existed or not; but the main principle applies. The object of their apparatus was to send two beams of light from the same source along routes equal in length, but such that in one case the route was at right angles to the direction of the earth's motion and in the other along that direction. The light waves were reflected by mirrors in such a way that they were brought together at a point, and here it was possible to ascertain whether one had traversed a longer path than the other. The wave form of the transmission must be kept in mind. Suppose, for instance, that two light-waves start at the same moment from two adjacent points, the waves being at the same phase, then if they reach a distant point together, after traversing equal lengths, they will be in the same phase when they arrive at this point; but if

the length of one route were longer than that of the other by, say, half a wave-length, the phases would be in disaccord, one wave beginning to fall while the other was rising. The difference can be detected by appropriate instruments which show rings of 'interference,' interference meaning that one wave in a certain phase

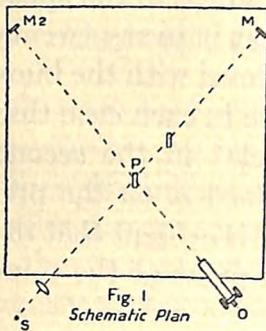


Fig. 1
Schematic Plan

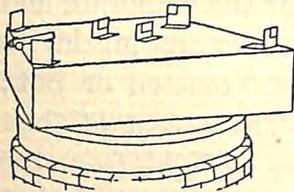


Fig. 2
Rough General view in Perspective

has balanced the other wave in the opposite phase.

Briefly the description of the apparatus of Michelson and Morley is as follows: From a source of light, S , a parallel-sided beam of light is produced by an arrangement of lenses, and this beam is directed to a small glass pane, P , silvered on the bottom part and transparent on the upper part, the pane being set at an angle of 45 degrees to the direction of the beam of light. The part that is transparent allows a corresponding part of the beam to pass through to a mirror at some distance away. This direction of the beam is at right angles to that of the earth in its orbit, and it strikes the face of the mirror M , at right angles or nearly, and becomes reflected directly back to the pane, P , and is reflected by the silvered part of this pane to a sort of opera-glass arrangement of lenses, O . The other half of the beam which first strikes the silvered part of P is reflected in the same direction as that of the earth, and in this new direction it strikes a mirror, M_2 , at a distance from P equal to that

of M . The beam strikes the face of the mirror M_2 at right angles, and is directly reflected in the direction, reversed, by which it arrived at M_2 ; and it then passes through the unsilvered part of P , and along the same direction as that taken by the other part of the pencil after its reflexion at P . The two parts of the pencil arrive together at O , and if their paths have been equal they should be at the same phase; or if the paths have been unequal they should be at different phases and this would be shown by interference, and the amount by which one has fallen behind the other would then be measured by the interferometer.

Such being the general principle, Michelson and Morley paid great attention to the details of construction so as to avoid causes of error. It was necessary that the whole apparatus should not be subject to deformation. The foundation on which the mirrors were placed was a stone slab resting on mercury in a large basin. Then it may have been noticed that in the paths pursued by the two parts of the beam of light, that in the direction of the earth's orbit had to pass through the thickness of the pane, P , to reach the surface of the silvered part by which it was finally reflected, it then emerged after traversing this thickness, and once more on its return it passed through the same thickness; a pane of unsilvered glass of like thickness and set at the same angle to the beam was accordingly inserted in the path of that part of the beam whose direction was at right angles to the direction of the earth's orbit, so that this beam also traversed that thickness three times. Further, the effects were amplified by successive reflections from different mirrors. The delicacy of the methods required may be

judged by the fact that a variation of a very small fraction of a degree in the temperature of one of the branches of the apparatus produced an error in the displacement of the interference bands which gave a miscalculation of several kilometres a second.

Experiments were carried out in great numbers and at opposite points of the earth's orbit, taken also at various angles of inclination of the first beam to the orbit, and the result was that no 'ether wind' was discovered above 5 kilometres a second.

During the years 1904 and 1905 a fresh series of observations and experiments were carried out with even greater care by E. W. Morley and Dayton Miller, the paths of the light-beams being considerably extended in length, and the results went to show that an ether wind of 3.5 kilometres a second was improbable. The foundation of the apparatus had been varied from time to time, stone, wood, steel having been tried in succession, and these were found to have no effect on the result.

In 1921 Miller again returned to the experiments, and installed his apparatus on the summit of Mount Wilson at a height of nearly six thousand feet, and out of a great number of observations giving rather widely divergent indications, he concluded that an ether wind of 10 kilometres was ascertained. Miller himself pointed out various causes of error: the difficulty of securing constancy in the parts of the apparatus; the effect of the 'personal equation,' influenced also by the great fatigue caused at this altitude by the strain of attention to all the details and to the precision of the measurements. Many thousands of experiments were undertaken between the years 1921 and 1925. At

Cleveland only about one tenth of the supposed ether wind was discernible.*

Dayton Miller, in a letter dated 4th October, 1930, says that "It is true that nearly all the writers at the present time interpret the experiments as giving a definite null effect, and most of them assume that it is final. The truth of the matter is the experiment never gave a null effect. My present determinations are exactly in agreement with the 1887 results of Michelson and Morley. This fact has been widely announced especially in England, but the theory of relativity seems to be so acceptable to many persons that they overlook the apparent discrepancy."

It is remarkable, though I do not force the argument, that those who carried out these famous experiments on which the Relativist doctrine is based, have not accepted that theory.

Then there followed a series of experiments of Professor Piccard of Brussels which at first failed to show, even at the summit of the Rigi, at over six thousand feet of altitude, an ether wind of more than one and a half kilometres a second. Experiments by balloon gave a very different result, the ether wind at eight thousand feet being nine kilometres a second.

The conclusion on which most physicists are agreed is that it is doubtful whether any ether wind exists at all, and that if it does exist it is possible that it reaches the velocity of ten kilometres a second. Another series of experiments conducted by the physicist M. Sagnac, and controlled by others, would appear to show that the rotation of the earth is not accompanied

* Dayton Miller has written an interesting account of the experiments in *Science*, 30th April, 1926.

by any measurable ether wind. Finally it would seem, on comparison of all the results hitherto obtained, that the limits of error of the methods employed have been minimised, and that causes unnoted or not sufficiently well estimated may make the findings insecure. An important fact which is often left out of account is the movement of the sun through space, and, as Léon Lecornu points out in his *La Mécanique : Les Idées et les Faits*, the velocity of the sun is not well ascertained.

For one thing the conditions of the reflexion of light from the surfaces of the mirrors are exhibited diagrammatically and the assumption is made that the reflexion is instantaneous ; but the actual waves of ether do not come into contact with plane surfaces, but with surfaces where both the molecules and the distances that separate them are not uncomparable with wave lengths, and where the molecules are in continual motion. Moreover, assuming plane surfaces for the mirrors, the mirror M is struck at right angles by the beam of light at the point towards which this beam was directed ; in the case of the mirror M_2 the beam of light strikes the surface at a point, certainly very close but still distinct from the point at which it was aimed ; in fact at a distance from this point equal to the distance travelled by the earth during the time necessary for the beam to reach the mirror ; moreover the mirror M_2 on which the beam impinges is moving at the moment of contact, in the direction of its plane, at the velocity of the earth in its orbit. This source of error was taken into consideration in the experiments after 1887 ; but the fact that no allowance was made for it at that time shows how hazardous is the assumption of

the limit of error in these very delicate experiments. Even when allowance was made, the exact theory of the form of the waves of transmission and interference was not ascertained. That will be considered later when we come to more exact mathematical discussions. It is assumed also that all the effects of light in all conditions are known ; that may, or may not, be true, but nothing proves that our knowledge of the behaviour of undulations of the ether is complete. The Doppler effect, the Zeeman effect, the Stark effect, the Compton effect, the Raman effect, were all discovered long after the classic treatises, official and complete, had been written on the subject.

Equally important with the accurate measurement of phenomena as far as known is the interpretation of results. Even with the same data before them, distinguished physicists offer explanations which completely satisfy themselves and their pupils ; and then other equally distinguished physicians offer explanations that differ so widely that they introduce new agencies.*

There exists a classic experiment, due originally to Fizeau, and devised to put to the test the question

* The Zeeman effect, for example, first observed by Zeeman in 1896, consists in the decomposition of spectral lines by a magnetic field. Thus a spectral line derived from the radiation of an atom, when submitted to a magnetic field appears, when observed in the direction of the lines of force of the magnetic field, as divided into two components ; but when observed in a direction perpendicular to this, it appears as divided into three.

Professor Lorentz, who has greatly helped the cause of Relativity, when he heard of this at once offered an explanation, which was accepted with delight in our Universities where such matters are studied. It will be found given in books written under the aegis of Sir J. J. Thomson, and therefore offered without the slightest doubt. The most up-to-date physicists, who make a fetish of the ' quanta,' pooh-pooh this explanation, and give their own. They cannot both be right ; but it is possible that neither is right.

whether the ether could be carried along by water. Fizeau passed a current of water, at a velocity of 7 metres a second, through a tube bent in such a way that one part was parallel to the other ; and the flow of water in the second was consequently in a direction opposite to that of the flow of the same stream in the first part of the tube. A beam of light was sent, by an ingenious arrangement, along the course of the water in the direction of its flow, and another beam in the opposite direction. As the distance travelled, apart from the effect of the water, was the same in both, the concord of the phases would not be found altered when the beams were at length brought together. There was, however, a distinct alteration of phase observed, and Fizeau came to the conclusion that the ether was in part carried along by the stream of water. Fizeau's experiment was reproduced with even greater care by Michelson and Morley in 1889, and the conclusion of Fizeau was confirmed.

Then subsequently Lorentz, whose name we will meet again, put forward a theory in which the very existence of the ether was ignored ; he ascribed the effect observed to the influence of the outer electrons of the atoms in the molecules of water.*

It is curious to remark that Lorentz attempted here to hoist Fizeau with a petard, which served, with certain further modifications of theory, to attack his own explanation of the Zeeman effect.

* Here it should be observed that at this period the theory of electrons was new, and by an effect, which has a real psychological interest, electrons became associated with all sorts of phenomena. Compare with this what we have already seen in regard to curvature ; evidently fashion influences the most solemn pundits no less certainly than the most frivolous of pretty women.

With regard to the ether Fresnel, who, with Thomas Young in this country, gave a great impulse to the modern undulatory theory of light,* concluded as the result of his observations that the ether was partly carried along by the earth, and by flowing water, and so forth. Other physicists believe that the ether surrounding the earth is carried along with it. The Relativitists generally reject the existence of the ether. They thus come to the old conception of action at a distance ; but they are not troubled by that. Their tendency is to regard physical facts and phenomena as identical with the measurements that define them ; so that in place of a world where effects are produced by an impalpable but real ether, they are satisfied with the formulation of mathematical expressions.

Yet the overwhelming force of observation and experiments tells in favour of the existence of the ether. The hypothesis itself presents nothing more shocking to our reason than did the conception, which for centuries was ignored, of a substantial and ponderable atmosphere. The assumption of undulations in the ether not only serves to explain a vast number of phenomena, otherwise difficult to understand, but it has been found consistent with curious observations which could not well have been foreseen or discovered on any other theory. Thus on one occasion when, as was justified, Fresnel's theories were being subjected to

* The undulatory theory was advanced, and argued with great power, by the Dutch philosopher Huygens in opposition to Newton's theory. The very latest tendency of a certain school headed by M. Louis de Broglie is to adopt both the undulatory and the corpuscular hypothesis in order to explain the phenomena observed throughout the whole range of the science of light. With regard to Fresnel he has passages which indicate that he held the ether to be stationary. In any case he had no data that gave him certainty.

the keenest scrutiny and forced to meet all sorts of objections, Poisson, who was a distinguished mathematician and physicist, said to him: "But on the basis of your hypothesis I have found by calculation that at a place where two shadows meet, in a certain construction, we should get a spot of light!"

"Very well," replied Fresnel, "let us try whether it is so." The experiment was made and the spot of light appeared. The phenomena of interference is immediately explained on the basis of the wave theory, and certainly on no other that has yet been imagined, still less demonstrated. The supposition of Fresnel, that the ether is in part carried on by material substances in close contact with the portion of ether affected, is in accord with observation. What it means is finally that ether possesses viscosity in common with all material bodies. If that were not the case how could the undulations themselves be produced? How can we otherwise account for refraction when a beam of light passes from a certain medium into a denser medium?

Here we come to a crux of great importance in the theory of Relativity. The whole difficulty of interpreting the result of the Michelson-Morley experiment disappears if we suppose that ether is viscous and that the portion near to the earth is in part, at least, carried along by the earth. In a compartment of a railway carriage—to supply an analogy to make this clear—everything in the compartment acquires the velocity of the compartment itself independent of the additional impulses given to diverse objects; and the effect of forces within the compartment is, in as far as affected by this speed at all, just the same as if the compartment

were at rest. And if the portion of ether surrounding the apparatus of Michelson and Morley were carried along at the velocity of the apparatus itself—that is to say at the velocity of the earth to which the apparatus is attached—or at a velocity bearing a fair ratio to that velocity, then the paradoxical character of the results would disappear.

But it is on the supposition that no such explanation is available that Einstein forms his theory. M. Picard in his criticism of the theory of Relativity says that it vanishes as soon as the Michelson-Morley experiment comes within the scope of known physical effects. One of Einstein's followers, M. Bricout, of the École Polytechnique, who has written an excellent book, though steeped in the Relativist doctrine, says that as experiments seemed to indicate that the ether did not participate in the rotation of the earth, while it was carried along by the earth in its movement of translation, "this proposition seemed so paradoxical that Einstein was led, in order to remove the difficulties, to set forth his famous principle of relativity based on a profound analysis of the physical nature of space and time."

I would here remark that the tangential velocity of the earth's rotation is, as we have noted, less than one sixtieth of its velocity of translation; so that even if the effects were as stated—Sagnac's experiment being much more difficult and subject to error than that of Michelson and Morley—there is nothing 'paradoxical' in the result.

Secondly we have already seen in how far Einstein's analysis of Space and Time was 'profound'; it is both imitative and superficial, superficial to such a

degree that it completely ignores the nature of the analysis which is requisite; for it is only by sounding to the depth the meaning and character of the Fundamental Processes that we can acquire right conceptions of the manner in which notions of space and time enable us to understand the psychological aspects of the question.

We may now in succeeding chapters press this criticism home with insistence on rigour and cogency of argument. We will also further consider certain physical facts with regard to the ether, and the possible interpretations of the Michelson-Morley experiments, as viewed in regard to other considerations.

CHAPTER IV

EINSTEIN LAYS THE FOUNDATION

WE have seen that the assumption, not at all an unwarranted assumption, of a viscous ether removes all difficulty in explaining the Michelson-Morley experiment. The assumption of an ether which remains unmoved as material bodies, even of the magnitude of the earth, pass through it, would force us to seek for some hitherto unsuspected physical cause. The conception of a space devoid of ether leaves us in face of a paradox. In this case there can be no question that one part of the original beam of light traverses a path longer than that of the other, yet no difference of phase is produced. The explanation at which Einstein at length arrives has become famous in science. In the pithiest expression to which we shall ultimately reduce it, his explanation amounts to saying that the velocity of light is constant whether relatively to a body at rest, in regard to the source of light, or relatively to that body in motion towards the source of light.

Here we have something that smacks of genius, if indeed the one mark of genius be disdain of common sense. Even his disciples, half in terror, half in admiration, admit that, as one of the most learned and the most faithful * remarks, "the new theory wounds in

* M. Pierre Bricout.

more than one point our philosophic conception of the Universe"; but what a small thing is this, a mere running counter both to science and common sense, compared with the compensation he has offered us—"Einstein has opened up to science new horizons, and rendered possible the union, in a grandiose synthesis, of the laws of Dynamics, of Electromagnetism and of Gravitation."

These laws, according to the Relativitists, are themselves nothing but mathematical expressions, for—to continue our citation—"it must be remarked that Relativity only aims at the realisation of a synthesis of *our measures.*" The italics are the author's.

No doubt the reader has borne in mind that he had to swallow a curved universe and the meeting of parallel lines in a plane, in order to preserve the etiquette that a straight line must always intersect another in one point; always? Yes, even when they are parallel and therefore cannot meet. Now in the course of my studies I have read strange stories of the force of etiquette; how Marie Antoinette could not be assisted to her feet when she fell from a donkey because none of the courtiers knew the etiquette; and how a princess of Spain scorched her legs—because the simple device of pulling back her chair was forbidden to underlings by etiquette; but see how Science—remember the capital S is that of our author—transcends all material pomp, even of the old Spanish Court, for it compels common sense, reason, the innocence of natural conceptions, the prestige of axioms, all to bow their heads before the etiquette of neatly labelled formulae. Relativity, we learn, despises Nature when Nature conflicts with its laws, and the aim of the laws

is to get a series of imperfectly understood phenomena nicely arranged under the same etiquette. The accountant of a City company with imaginary assets shows us a smoothly arranged balance sheet, and the reproach we make is that the figures do not correspond to the reality ; and that, in fine, is the gist of my reproach to the Einsteinists.

Here I am forced again to introduce a somewhat laborious argument : Suppose we have laid before us a proposition that appears to us, if not quite axiomatic, at any rate no less certain ; then if the truth of this proposition be contested, and another proposition be substituted that appears on the face of it to be absurd, we should seek an explanation ; if now the explanation begins by offering us, as a postulate, a proposition far more complicated and obscure than the original proposition, and then continues in a series of argument, each step showing complexity and uncertainty far beyond that of the acceptance of the original proposition ; what should we do ? We might simply get up and walk, as Diogenes did when the Sophists tried to prove that there was no such thing as motion. I have always regarded this witty act of the crusty old philosopher as a convincing reply ; but in another place * I have endeavoured not merely to refute the Sophists, but to throw a beam of light on the spot where their chain of argument admits a fallacy. I will do something similar with the Relativist paradox.

The principle of the first form in which he conceived Relativity was formulated by Einstein in 1905 : In every non-accelerated system of reference the laws of electro-magnetism (equations of Maxwell) have the

* *Principles of Psychology.*

same expression. This enunciation will be found more or less explicitly in the works of Einstein's disciples : for instance, Sir Arthur Eddington in England, Professor R. Carmichael in America, and M. Pierre Bricout in France.

We are further told that this proposition may be considered as an extension into the domain of electromagnetic phenomena of the property of invariance, with regard to uniform translation, of the fundamental law of classical dynamics : the force is equal to mass multiplied by the acceleration. I quote from the excellent book of M. Bricout. On the other hand this transformation does not conserve the form of the equations of Maxwell. Einstein was the first to have the audacity to hold that this divergence was due to the fact that the classic formulae constituted only a first approximation of the real law of transformation, and to state the principle, suggested by the negative result of Michelson's experiments, of the absolute invariances of equations of Maxwell.

"The development of the principle of special Relativity carries with it therefore : (1) the research for the group of transformations leaving invariant the equations of Maxwell in regard to uniform translation ; (2) the physical interpretation of the formulae obtained.

Finally when we arrive at the generalised form of Relativity we find this as the cardinal proposition : "All systems of reference are equivalent in regard to the formulae of the laws of Nature on condition to admit for each of them the existence of a suitable field of gravitation."

And all this to prove that $2 + 2 = 3$; or, at least to disprove something obvious : that if two bodies are

moving towards each other their relative velocity is greater than if, with regard to each other, they are in a state of rest. I ask therefore if I have exaggerated the magnitude of Einstein's feat.

Let us look at the statements closely. In the first place it is not a fundamental law of classic dynamics that $F = mg$, where F stands for force, m for mass, and g for acceleration. The conception of force is itself not fundamental. That is so true that some of the great thinkers in the domain of dynamics, Lagrange for example, have tried to avoid the use of the term force. Be that as it may, where the conception of force is admitted, the equation $F = mg$ is the *definition* of force, and a definition is not a law of nature. The confusion reminds me of the story of the intelligent little girl who asked why a lion was called lion; "because," replied the tamer, "that's wot 'e is!"

The laws of Maxwell, again, are not fundamental conceptions; it required the genius of Maxwell to discover them, and their formulation is the crown of his work. Moreover, they are, like everything in science, subject to criticism, and part of my own criticism is that they are not the outcome of arguments based on mathematical demonstrations applied to physical conditions, but are due to a transference of results, obtained by mathematical reasoning entirely within the sphere of mathematics, to physical phenomena discovered accidentally, and defined more closely by experiment.

Maxwell employs in the course of his demonstrations a well-known mode of transformation known as Stokes' Theorem. The essential service of this theorem is that, when we have a force moving round a curve, it enables

us to express the integral obtained on such a circuit in terms of the integral of forces supposed to act in the surface enclosed, taken over the extent of that surface. The demonstration of Stokes's theorem is entirely formal, and that demonstration has no reference to actual physical forces. This fact is more clearly seen when the demonstration is carried through in the terms of vector analysis, as shown in any of the excellent books in various languages on that subject, which is again the vital surviving portion of Hamilton's Quaternions.*

On the other hand it had been found in the course of experimentation in electricity † that if a current of electricity were passed round a wire a magnetic field became created with the lines of force, issuing—to give a rough indication—at right angles to the surface enclosed by the wire. Now such phenomena could not possibly be deduced from Stokes's Theorem, though that assumption is often made, for without experiment there is nothing to show the nature, or even the existence, of the forces produced. If, for example, a current of water were passed round a bent tube there would be no hydraulic phenomena arising in the enclosed surface. Further, the nature of the forces produced is modified by placing various materials, such as an iron core, within the circuit. It will therefore be seen that already a great assumption was made by Maxwell when he applied Stokes's Theorem to afford him quantitative relations among the forces.

So far we are dealing with electro-magnetic phenomena, whereas the original problem dealt with light.

* Perhaps the best exposition is in L. Silberstein's *Vectorial Mechanics*.

† Principally by Ampère, Oersted, Faraday.

The reply may be that light is an electro-magnetic manifestation; but that statement is not warranted by what we know of the facts. It is due in the first place to Clerk-Maxwell, and the assumption on his part was based on the discovery that the ratio of the unit of electricity in the electro-magnetic system to that in the electrostatic system was represented by the same number as the velocity of light. It is now generally agreed that the velocity of transmission of the Hertzian waves—those employed in wireless—is equal to that of light; but from that to a conception of the equivalence of the two phenomena is a big jump, and in certain respects it is unjustified. Light and X-rays depend on undulations transmitted with the same velocity, but they are not equivalent phenomena. The rays at the red end of the colour spectrum do not produce equivalent results with those at the violet end; the waves of greater length convey radiant heat to a greater degree than the other, while at the violet end the photo-chemical effects are more in evidence. Enough therefore has been said to show how far away we are from the conditions under which it is possible to set forth a postulate.

But apart from this Einstein affirms that all the laws of electro-magnetism should have the same expression in all the systems of reference. This is a remarkable demand, for it is so little of a postulate that it requires detailed examination before its veritable meaning appears.

There are two forms of invariants in mathematics, one of substance and one of form. The length of a given line in a figure is an invariant, for though it may be expressed first in one system of measurement and then in another, yet the line itself is not altered:

There is no necessity, however, for the expressions indicating the measures to have the same form. But there is another kind of invariance, and it is that to which Einstein refers in his postulate. The best account of this invariance I find in the work of a Danish mathematician, Julius Petersen, professor at the University of Copenhagen, and I give here a translation of a passage dealing first with covariance, from which he obtains invariance as a special case. Now most people can understand that if one gives motion to a body the relative motion of that body in regard to another is altered; so that the engine can be admired by which Einstein refutes that apparently obvious proposition. The setting up of his engine involves the consideration of invariance. Petersen's definition runs :

“ When one transforms the form

$$a_x^n = a_0 x_1^n + \frac{n}{1} a_1 x_1^{n-1} x_0 + \frac{n(n-1)}{1.2} a_2 x_1^{n-2} x_0^2 + \dots + a_n x_0^n,$$

one obtains a form of the same order with new coefficients a_0' , a_1' . . . , and similarly for every other form. Take u as an integral and rational function of the variables and coefficients of one, or several, forms, homogeneous in x_1 and x_0 . If in u , we replace the coefficients by those of the transformed form, and x_1 , x_0 by ξ_1 , ξ_0 , respectively, we obtain a new expression u' . Now, in u' , replace the new coefficients and the variables ξ_1 and ξ_0 by their values in function of the former coefficients and former variables ; if then u' does not differ from u except by a factor dependent only on the coefficients of the substitution, we will call u a covariant of the form, or a simultaneous covariant of the forms, if more than one intervene in the expres-

sion of u ." Further Petersen says: "The covariants become the invariants when we regard the variables as coefficients of linear forms of which the first term is negative." In a later passage Petersen makes the meaning still clearer: "A linear substitution transforms $\Sigma a_{jj}x_jx_j$ into another $\Sigma b_{ij}y_iy_i$, with other coefficients. Every function of the coefficients, which, after substitution, becomes multiplied only by a power of the determinant of the substitution is what we call an *invariant*."

Now lucid as Petersen's explanation may appear to those who have looked into the theory of Forms, they may not be immediately obvious even to every mathematician. In fact, I once asked Henri Poincaré, the most famous of modern mathematicians, a question dealing with forms, and he told me that he had never studied that branch, and he referred me to Clebsch. I found that Clebsch and Aronhold had written in fascinating style on forms, and that Petersen had been greatly beholden to them. But already we begin to see what spirits from the vasty deep Einstein has to invoke to disabuse our minds of the obvious.

Clebsch and Aronhold and Petersen, and other mathematicians, Laurent in France and Cayley, one of the most brilliant of English mathematicians of all time, have written with wisdom on the theory of invariants, but the invariants they generally dealt with were quantities which in their nature were invariant. This is also the case of the invariants considered in that branch of mathematics which, from Christoffel, Lipschitz, amongst others, through Ricci and Levi-Civita, Veblen, Hergoltz, has developed into the tensor analysis often employed by the Relativitists.

The recognition that the invariants—the quantities whose values are independent of the mode of measurement—are the most interesting objects of mathematical research, that has been the guiding idea in the formation of the tensor calculus.

But in the instance that we are now dealing with Einstein dispenses with that guidance, and in order to account for the result of the Michelson-Morley experiment he applies a formula of invariance to conditions where no invariance exists. This will be examined closely, and with reference to the mathematics employed, later; but for the present, in order to clear the ground, something should be said of another theory adopted by some of the Relativitists, who do not seem to appreciate the fact that it cuts away the ground on which the extended structure of Relativity has been built. This is known generally by the name of the “Fitzgerald Contraction.” Professor Fitzgerald after studying the apparent paradox of the Michelson-Morley experiment had a flash of genius. He said that bodies moving through the ether were contracted in the sense of the direction of the movement. That explanation had one merit; it explained everything; that is to say, if it were true. It had one demerit; it was on a par with the explanations of the mediaeval philosophers, for example, that a bird flew because of an affective influence of air upon feathers. There is an ugly phrase, sometimes employed in law, “*ad hoc*”; it means that some explanation is given that is suggested not by any general natural law, but of application to a specific instance. This phrase, if it did not exist, would have certainly been invented for the theory of Relativity, for it comes into action again and again.

Sir Oliver Lodge when he heard of Fitzgerald's explanation saw at once that it was perfect, and Silberstein in his book on Relativity congratulates him ironically on this rapid intuitive perception of truth. The explanation, however, presupposes the existence of the ether, though it leaves the operation of the ether delightfully vague; but if the ether does not exist then the mathematical formulae that supplies its place with the Einsteinists cause the bodies to contract. In any case this explanation is so simple and satisfying that it renders any further explanation, such as that beginning with Einstein's portentous postulate, out of place. To use the familiar words of the man in the street, you can't have it both ways. Sir Arthur Eddington finds a conflict between his devotion and his common sense; and he reconciles them. The Fitzgerald contraction exists, but it is not real. Here is his explanation.*

"Thus S (observer's frame) finds that a moving rod contracts when turned from the transverse to the longitudinal position. It may seem somewhat strange that we should be able to deduce the contraction of a material rod and the retardation of a material clock from the general geometry of space and time, but it must be remembered that the contraction and retardation do not imply any absolute change in the rod and clock. The 'configuration of events' constituting the four dimensional world which we call the rod is unaltered; all that happens is that the observer's space and time partitions cross it in a different direction."

This may be perfectly satisfying to the reader, but I confess that when I read it, it reminded me vaguely of

* Cf. p. 25, in Sir Arthur Eddington's *The Mathematical Theory of Relativity*.

the "New Religion" started by a negro waiter in one of the Western States; one felt that there was something idealistic in it, in the sense that it flew beyond the bounds of reason, but even in its iridescent beauty it was too phantomlike and elusive. If it means nothing more than that the appearance of contraction is merely something corresponding to the effects of perspective, then I would ask, what is the import of this contraction in regard to the Michelson-Morley experiment? We are dealing here with material things, waves of ether—for ether must be present surely to explain the contraction produced by movement through the ether—mirrors, and interference fringes. The rays of the divided beam are brought together finally in one place, so that the observer's "space and time partitions" cut across the two rays at their points of contact.

Parting company with Sir Arthur Eddington for the moment, therefore, I find that this Fitzgerald contraction—the real contraction—is a treasured possession of the Relativitists; and I have meditated upon it so long that I have found a means of testing it. The theory is that the whole block which is part of the apparatus employed by Michelson and Morley contracts, as does everything else, in the direction of motion, but not transversely.

Now in another part of Sir Arthur's *The Mathematical Theory of Relativity*, where he is dealing with Maxwell's equations, he says that "charge, unlike mass, is not altered by motion relative to the observer."*

* The more modern physicists challenge this assumption regarding electric charge, and, be that as it may, I will show later that the assumption in respect of mass is unwarrantable; but I am not here dealing with Nature. I am tracing out the arguments of the Relativitists to inevitable conclusions.

Suppose therefore we arrange a system S_1 thus : we wrap a coil of insulated copper wire round a long iron core, and send a current of electricity through the wire and then attach the ends of the wire to the receiver of a telephone ; then give a rapid motion of translation to the apparatus, with windings attached, in the direction of its length, or since relative motion only is required let us take a journey ourselves in an aeroplane in which we have installed the system S ; that is to say, we have set up the ' frame ' of the coordinates of Descartes ; contraction occurs in the iron core, and this in turn will modify the current passing through the copper wire, and finally will produce sounds which we receive by the telephone. These sounds can be altered at will by changing the rate of relative speed of S and S_1 . Therefore finally by the simple expedient of putting up Cartesian coordinates in a moving vehicle we may obtain the syllables " Ba ! Ba ! " or " Ma ! Ma ! " ; and thus receive from the vasty deep of Nature's arcana the most contemptuous expressions of lack of faith, or the tenderest appeal for sympathy and help. . . . And yet there are people who ask, " What is the use of philosophy ? "

CHAPTER V

THE VARIATION OF CONSTANT TIME

So far we have traced out various errors of the Relativitists ; and in that alone one could say that the task was completed if the object were merely to show that the upholders of this doctrine had not established it scientifically. I say deliberately that for such a purpose in regard to the "Special Relativity" it is sufficient to prove that it leads to such absurdities as that the velocity of light towards an object is unaltered by the movement of that object. With regard to the "general Relativity" it is sufficient to say that one cannot—as we shall later find the Relativitists claiming—alter the universe, and introduce new laws, simply by manipulating Riemann's metric, especially since this metric was not conceived with regard to any such purpose, or that those who have so adapted it ignore the physical conditions producing the phenomena they profess to explain.

There is here an appeal to common sense, but certain highly trained intellects, or, what often means the same thing, some completely sophisticated intellects, become impervious to any appeal of common sense. By common sense I do not mean the current acceptance of false notions on subjects of science, nor do I suppose that any man of good judgment could on this

basis alone controvert a finding in science ; for the instance I have already given of Fresnel's insistence that out of two shadows light might come, shows how dangerous it is to predict in intricate matters where the principles and conditions have not been completely studied. In that case, however, we were dealing with complex physical phenomena ; but when a Relativitist tells us that by adding to a quantity something of its own kind we leave it unaltered, or that fiddling with symbols which in themselves mean nothing we can gain the last secrets of nature, then if anyone fails here to use his common sense, I say that he is deficient in faculties essential to the pursuit of science.

That is general, but I mean to get to close grips. I will point exactly to the very wheel in an assemblage of Einsteinian effects which is faulty. Further I will trace out the wrong bias which has caused the introduction of the fault. But before entering definitely into arguments which, because they must be meticulous and close, may appear tedious to many, I feel that I must seek consolation in some philosophical reflexions.

What is the cause of the resistance found so often to new valid ideas in science, and parallel with this the tolerance or worship of absurdities ? The people love to be deceived, the shrewd old Romans believed ; but here we are dealing with men of the highest intellect, scientists enjoying high popular fame. Yes, but a man may be a scientist of repute, and deserving of his fame, and yet be deficient in general culture. He may be an ' expert ' on the atom, or a wizard in the field of X-rays, or he may have dissected earthworms till no secret of their apparatus is hidden from him ; and yet from these special studies he may never have raised his eyes,

or stopped like Rodin's *Penseur*, to allow new and strange thought to pass through his brain ; he does not even place his branch of science in the general body of scientific acquisition, still less seek to the depths the explanation of the manner of operation of the faculty of reason which, in one field, he so deftly uses.

I have known scientific men in many lands, and while finding them discourse like angels on what they really know, I have also known them to speak like poor Poll on subjects of which they knew nothing. These men are the most difficult of all with whom to deal, for their ignorance and obstinacy are fortified by that particular affectation of the soul so assiduously cultivated, and so detrimental to its possessor, the academic style. Yet there have been physicists who have had in regard to the larger scope of science zest for knowledge and clearness of view. Ampère, perhaps the greatest of all, is one, and certainly his work did not lose in value by this added quality.

At times the searcher after truth seems to be a solitary pilgrim finding the path amidst dense clouds of obstruction of solid interests ; and at the centre of these obscurities and falsities he finds the metaphysical ideas propagated by Kant, who has been accountable for the devastation of more intellects than the Great War for the destruction of material bodies. It may be objected that not so many millions have read Kant, and that it would have been a hopeful sign if they had displayed so much intellectual curiosity. True ; I believe that few people outside University professors, and their unfortunate pupils who must drill their brains to produce conventional answers, read Kant. The Fellows of the Royal Society who quote Kant at

solemn dinners are for the most part innocent even of misunderstanding him. They believe that etiquette requires that they should round off a thesis by some quotation that poses them as 'thinkers.' I have known a distinguished statesman do the same when on a serious occasion, after delivering arguments of which the ensuing months showed the futility, he capped his oration, and gained thunders of applause, by a quotation from Bacon that, however discutible in itself, had nothing to do with the subject in hand.

Yet—and it is this that impels me on with resolution ever renewed—I see that the appeal to philosophy, where the philosophy is valid, is always justified; and that a clear view of the basic premises on which the arguments are founded is of prime importance. Finally the analysis of all modes of thought into the Fundamental Processes of the Mind, must, however long it may take to reap the full fruit, be fertile in the highest degree, not only in the development of positive science, but in affirming the basis on which all ethical systems must rest. There is something in this conception of the activity of mind that is extraordinarily entrancing, and it lifts the conception of science far beyond that of the usual appreciation of a serviceable handmaiden to our material satisfactions or wasteful luxuries.

The fascination of this contemplation must not, however, blind us to the necessity of rigour in each step of our reasoning. Here no criticism can be too severe; the hammer of Thor may sound on the chain, but if it be truly interlinked it resists, for the strength of truth is as deeply founded as the constitution of the Universe itself; if there be false links it is well that

they should be broken ; but even then it is the glory of science that out of the fall of wrong theories it rises in new triumphs of the right.

When we come to search for error we find that we must narrowly examine the general idea of all philosophic theory, and then we shall find that in this world too, as in the most familiar instances, association plays a great part. Pythagoras, who sought with veritable scientific passion the secrets of the cosmos, was also the first to establish a theory of numbers ; but the association of the two forms of his activity led him to errors, such as that of the number of the planets, and his followers fastened eagerly on the weakest points of his speculation. Newton, whose principal work was concerned with the law of attraction, uses too exclusively the mathematical conceptions built up on this basis in his researches into the course of tides. Thomas Young, whose great life work was mainly concerned with undulations, thought of the theory of tides on the basis of periodic flow of water.

Maupertuis, who was a mathematician and a devout deist, thought to render service to mankind by demonstrating the existence of God by the infinitesimal calculus. Fechner, who was a spiritualist and a mathematician, tried to assess the quantity of emotion by logarithms. Lorentz having established certain interesting formulæ in electro-magnetism applies his method with confidence to the solution of problems of other types. Then Einstein, always a copier and a borrower, adapts the formulæ of Lorentz to cases where it has no application.

We will now investigate these matters with searching scrutiny. In pressing home my criticism with the

keenest point I ought, it would seem, to quote exclusively the text of Einstein, and that was my first intention. But looking into it closely I saw that it would not give satisfactory results. The reason is that there is no coherence nor consecutiveness to be found in Einstein's expositions. In him we find the first of the faults deplored by Jacobi, that of giving reasonings so short, that is to say, with so quick a leap between the premises and the conclusion, that one cannot for good or bad, get a hold on him. He may "see as a God sees," but he talks as a woman talks.

From Sir Arthur Eddington and Professor A. N. Whitehead I expected something better, but they avoid the opportunity of distinction. Here in the doctrine of the invariance of velocity of light is the calvary, the crucial test and the glory, of Relativity; and one would think that it would be set out in holy garnitures or preserved for pious mathematical thinkers for the ages to come; and that they would have left some sign of their guardianship.* Why, even the spot where William Rufus was shot by the arrow is adorned by a decent tablet, and we are all pleased to know what befell him, but here!

"So the two brothers with their murdered man
Rode past fair Florence. . . ."

* Einstein has abandoned the doctrine of the absolute invariance of the velocity of light, but that is only an "ad hoc" makeshift. He retains the dogma of the invariance of the velocity of light relative to an object, irrespective of the movement of that object, for that arises of necessity from his explanation of the Michelson-Morley experiment; it is indeed the foundation stone of Relativity. Some of his followers explain it away, as being of the nature of an illusory appearance, but that too would dispose of the basis of Relativity. It is a matter of choice whether the vessel goes down flaunting its gallant bunting of absurdities, or merely peters out in puerility.

I am reminded, often reminded of Keats—ah ! what a soul was that ; to think that such a being lived, thought and spoke, makes a balm for grief.

At length after much searching I come to the chapter on this subject in R. D. Carmichael's *Theory of Relativity*. Mr. Carmichael is professor of mathematics at Indiana University, and in addition to this book has produced another, interesting within a small compass, on the *Theory of Numbers*, among the excellent " *Mathematical Monographs*," produced with the help of various American universities.*

* Published by John Wiley & Sons, New York. Professor Carmichael is also one of the very few mathematicians and physicists who have shown a clear comprehension of what is called Least Action.

CHAPTER VI

THE POSTULATES OF RELATIVITY

PROFESSOR CARMICHAEL is quite honest. He has left the entrenched positions of obscurity, he has deployed his arguments, he has come into the open; and the Lord hath delivered him into my hands! Professor Carmichael has analysed the reasonings, and he has set them forth in a clear manner by indicating the postulates laid down and the theorems deduced. I follow the denotation of his postulates: "Postulate *M*: the unaccelerated motion of a system of reference *S* cannot be detected by observations made on *S* alone, the units of measurement being those belonging to *S*."

It will be convenient perhaps if after each postulate or theorem I offer my own comments. I am not sure what this postulate means. If the condition be that the observations must be made within the system *S* without cognisance of anything outside *S*, the postulate must be accepted. If by 'on' is meant that the standpoint is taken on *S*, but that objects outside of *S* are observed, then the postulate is not true, for with units of measurement belonging to the earth we can measure its motion round the sun. The fact that the motion is not uniform has no effect here on the argument. However, it would seem that we are supposed not to observe anything outside the system *S*; but we may ask later whether this condition is respected.

“Postulate R' : the velocity of light in free space, measured on an unaccelerated system of reference S by means of units belonging to S , is independent of the unaccelerated velocity of the source of light.”

This is the postulate to which Professor Carmichael attaches in a subsequent reference the greatest importance ; it deserves therefore our closest attention. We shall see that the proposition, true or false, cannot be called a postulate. If light were projected like a bullet from a rifle then certainly its velocity would be influenced by the velocity of the source of discharge. For instance, if the rifle were in a rapidly moving train then rifle, bullet, and everything at rest relatively to the train, would have the velocity of the train. Now when the rifle is fired the bullet, on leaving the rifle, has attained a velocity with regard to the rifle. That results from physical causes—that is to say the form of the rifle and that of the bullet, and the quality and quantity of the powder—independent of the velocity the rifle has acquired by being transported in the moving train. Therefore finally the velocity of the bullet would be the velocity of the train, added to that due to the usual velocity of discharge. All this is so simple that it seems almost unnecessary to explain it in this manner ; but we must not leave any loopholes of evasion by which any fallacy may escape.*

Suppose now that a rifle at rest were placed ahead of the train, and that from the train a shot were fired that discharged this rifle. If that shot produced a movement forward in the rifle and the discharge of the

* M. Painlevé, who is the most concessive of all the critics of Einstein, assumes what is here argued in *La gravitation dans la Mécanique de Newton et dans la Mécanique d'Einstein*, Ac. d. Sciences, 14th Nov., 1921.

rifle followed there would again be an addition of velocity. But if the shot had no other effect than to discharge the rifle, the only velocity to be considered would be that appertaining to the rifle in its usual condition.

Now to which of these categories of effect do the phenomena of the propagation of light belong? This is a question in physics which cannot be answered except by actual observation or experiment; and hitherto, as a matter of fact, such observations and experiments have never been made. We see already, therefore, that this postulate is not something that Einstein could evolve from his researches into time and space. He has no more right to make the assumption implied than to present us, in the guise of a postulate, with the extraordinary complex proposition that the laws of electro-magnetism must have an invariant form in every system of measurement. Postulate R' , Carmichael says later, is the crux of the whole theory of Relativity; and even here, in what should be the basis of his arguments, he leaps to inadmissible assertions. He begs the question, as the old logicians phrased it, that is to say, he hides his conclusions already in his premises.

It may be remarked, however, that we have no right to argue by analogy, and to assume that in light the processes are similar to those of the discharge of a bullet. That is a perfectly just objection, but it reinforces what I say that we are face to face with physical phenomena, and that the Relativitists have no right, without an examination of the physical conditions, to ignore that aspect of the matter by giving us as a postulate a solution of the problem that suits their own purpose. That is all that for the moment I

wish to establish ; but on this basis I will show that the whole theory is worthless.

If the corpuscular theory of light be admitted, then, in another form, we come to the conditions, that of a bullet from a rifle, which we have considered. Postulate R' is incompatible with the corpuscular theory.

Let us take, if only by way of hypothesis, the undulatory theory of light. Here two possibilities arise, the ether is either subject to Fresnel's drag, that is to say, complete or partial transport by a moving material object immersed in it ; or the ether is entirely immobile, except in regard to the undulations of its own substance. If the ether be transported by a moving body in some degree then Postulate R' is false. The conditions for the propagation of the undulations are determined by the stimulus which produces light and by the conditions of the ether no less than in the case of the rifle, and therefore if the ether itself were in movement relatively to some system S , then the velocity of transmissions of the undulations would be that obtained by the addition of the velocity of light, as in a condition of rest of the ether, and the velocity of the ether. This conclusion is rigorous ; for no objection could lie as to the variation of the character of the ether by reason of its movement ; we are speaking of the velocity of a portion of the ether relative to a material object ; and if the ether were supposed at rest and the material body were in movement, the addition of the velocity of light in the ether, and the relative velocity of the material body in the ether, would give the velocity of light relative to that material body. In this case also Postulate R' would be false.

If then we suppose the ether to be immobile, except

for its undulations, we still have a physical problem, and not a metaphysical imagining. To understand what would happen we should require to know, what we do not know, the exact mode of operation of the causes, evidently partly chemical, that eventually produce light. For example, taking a general view of undulations we might suppose an aeroplane immobile for a moment over a lake. If a bullet were dropped in these circumstances, it would fall vertically and strike the water in a direction perpendicular to its surface. Waves would be produced, and they would succeed each other, with the point of contact as centre. But if the aeroplane were flying at great speed, the bullet would describe a parabola, and on contact with the water it would have a horizontal component in addition to the vertical component. Would the velocity of wave propagation be the same as in the first instance? There is nothing to enable us in the case of light to say yes or no; yet Postulate *R'* requires that the answer should be in the affirmative. We do not even know whether, following on the introduction of a light-producing cause, there is a delay of a certain, perhaps extremely small but appreciable, time, as happens, in the discharge of a rifle, before the stimulus has acquired its full action.

All these questions in regard to physical action are ignored by the Relativitists in dealing with phenomena which are purely physical. We shall see later that no matter which of these hypotheses is adopted their arguments land them in contradiction because they are not consistent with themselves.

But they say: there is no such thing as the ether; there is only electromagnetism. In what then, in the

absence of a transmitting medium—call it ether or anything else—does electromagnetism act? Evidently here we come to a problem essentially physical, and more obscure than those we have left.

Finally, the Relativitists may affirm: there is not even electromagnetism; there is required only a set of equations, and further questions are futile; that is actually affirmed by the Kaiser-treu disciples. Now I have profound respect for mathematics, so much so that like the Mohammedan chief who would not destroy a piece of paper lest the name of God were written there, I would be loath to destroy anything that held a mathematical record; and I hold in respect the precision of the German mathematician Kummer, who hated any talk of mere 'utility' for mathematics, demanding that his equations should be kept "unsullied from applications!" But even Kummer has a 'surface' to which his name is attached; and so with the best will in the world I do not see how these before-mentioned Relativitists can speak of measures unless there is finally something to measure. The phenomena of light are not mere mathematical expressions. They are represented at times as interference fringes visible to the eye; and both these fringes and the eye appertain to the material world.

So far then we see on what false foundations this Postulate *R'* rests. If it were allowable to take a postulate at choice, true or false, one could on this basis prove anything; but given that freedom, I do not think that I should be content to produce anything so pale and featureless as this theory of Relativity as my vision of the world; I would call forth in radiant colours something of warmth and life and glorious

wonder of interest that tingled in the nerves and stirred the heart, and lighted the brain, something of the touch of the Arabian Nights!

Professor Carmichael, as the next step, lays down :

“ Theorem I. The velocity of light in space, measured on an unaccelerated system of reference S , by means of units belonging to S , is independent of the direction of motion of $S(MR')$.”

Here we have a striking example of the mode of reasoning of the Relativitists. Theorem I is ostensibly founded on postulates M and R' , but M is only true under conditions which here are not respected, and R' is an assumption without warrant. Moreover Theorem I would not flow from these postulates, if they were true, because there is a sliding, as if surreptitiously, into Theorem I of something not contained in the postulates. We will examine this position carefully.

Theorem I does not flow from M and R' , because M is only valid, as we noted when examined within the system S . Thus, if a man were sitting in a room with the windows closed, he might be unaware of any movement of the room, because he and all the objects in the room would partake of that motion; but if a ball were thrown through the window, he would, with the units of measure available in the room, be able to ascertain, within the degree of accuracy of the instruments employed, the relative velocity of the projectile.

R' does not apply, because there we are dealing with a particular physical phenomenon, and the problem involves the question of the manner in which transmission of light-producing effects is caused. In Theorem I, however, instead of this physical question,

we are dealing with the motion of the system S in regard to definite light-rays—no matter how produced—emanating from a source outside S ; for it must be remembered that even if the ether surrounded S , the source of light and the transmitting medium would be outside S , if they did not partake of the movement of S . If they did partake of the movement of S , it is true that the velocity of the light produced within this system would be independent of the direction of S .

So far, therefore, the argument has been invalidated by at least three defects, a false assumption regarding M , an unwarranted assumption, involving a wrong conclusion, set forth as a postulate in R' , and in Theorem I, shifting of ground involving new conditions rendering R' inapplicable.

Let us, however, persevere in our examination. We come to, "Postulate R ": The velocity of light in space, measured in an unaccelerated system of reference S by means of units belonging to S , is independent of the numerical value of the velocity of S ."

Then we reach Postulate R ; but this is defined as the postulate obtained by combining R' and R'' , so that nothing happens, except the gradual sliding of error into righteousness. We find, "Postulate R : The velocity of light in space, measured in an unaccelerated system of reference S by means of units belonging to S , is independent of the velocity of S and of the unaccelerated velocity of the light source."

Here we have nothing new added to the observation or argument. Professor Carmichael has no doubt felt that this so-called postulate would be immediately scouted if presented too abruptly, therefore he has produced it in bits so that its complete aspect would not

be seen till after the reassemblage of the less alarming pieces into which it had been dismembered.

However, I am very pleased to see Postulate *R* in all its beauty, naked and unashamed, for some mathematicians, inclined to accept Relativity, deny that the theory implies—I must insist on it again—that the velocity of something moving towards a given body, the velocity being taken as relative to that body, remains unaltered, whether the body itself remains at rest or receives an impulse of motion towards this something whose relative velocity is in question. Professor Carmichael states it, and he is also honest enough to say that it is customary only to give Postulate *R*. Everyone makes assumptions, he indicates, according to the manner in which he presents the course of argument. He further states that certain other postulates are useful, and he gives us :

“ Postulate *V*. If the velocity of a system of reference S_2 relative to a system of reference S_1 is measured by means of units belonging to S_1 , and if the velocity of S_1 relative to S_2 is measured by means of units belonging to S_2 , the two results will agree in numerical value.”

My only comment on this is that we have herein the acknowledgment of what I have been at pains to point out—that it is possible to measure the velocity of one system relative to another by means of the units of either system ; for if the ether, or whatever the Relativitists put in place of the ether, does not participate in the motion of a system, call it S_1 , then it is itself, as Professor Carmichael subsequently agrees, another system, call it S_2 ; and the motion of anything, as for instance, wave-length in the ether when ether is

assumed, is susceptible of measurement by the units of the system S_1 . But on this ground we have seen the falsity of Postulate M , when the conditions here considered are really implied. Then Professor Carmichael gives us finally :

“ Postulate L . If two systems of reference, S_1 and S_2 move with unaccelerated relative velocity and if a line segment l is perpendicular to the line of relative motion of S_1 and S_2 and is fixed to one of these systems, then the length l measured by means of units belonging to S_1 will be the same as its length measured by means of units belonging to S_2 .”

This is acceptable. It is indeed but a manner of expression of what is usually stated in books of mechanics in language clearer and better suited to applications.

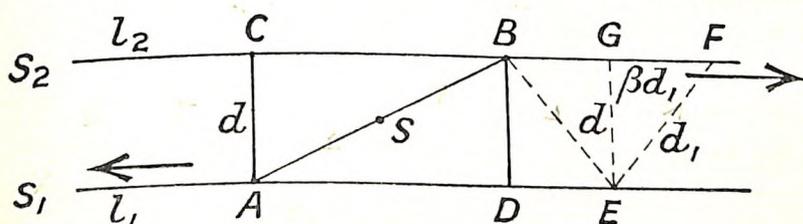
CHAPTER VII

THE POINT OF THE SOPHISTRY

THE preceding exposition of Professor Carmichael has been but preparatory to the establishment of a law which is regarded as one of the great achievements of Relativity, and so as to avoid any suspicion of unfair play I will quote him literally in his demonstration :

*“ The Measurement of Length and Time Relations
between the Time Units of Two Systems.*

Let us consider three systems of reference S , S_1 and S_2 related to each other in the following manner : the lines of relative motion of S and S_1 , of S and S_2 , S_1 and



S_2 are all parallel ; S_1 and S_2 have a relative velocity v , S and S_1 have a relative velocity $\frac{1}{2}v$ in one sense and S and S_2 have a relative velocity $\frac{1}{2}v$ in the opposite sense. The system S consists of a single light source and this source is symmetrically placed with respect to two points of which one is fixed to S , and the other is fixed to S_2 . This is possible as a permanent relation

on account of the relative motion of the three systems. For convenience, let us assume S to be at rest.

We shall now suppose that observers on the system S_1 and S_2 measure the velocity of light as it emanates from the source S . Let a point A on S_1 and a point B on S_2 , which are symmetrically placed with respect to the light source S , move along the lines l_1 and l_2 ; these lines are parallel. From Postulate L it follows that the observers on S_1 and S_2 will obtain the same measurement of the distance between l_1 and l_2 .* Denote this distance by d . From Postulate M it follows that neither observer is able to detect the motion. Therefore he will make his observation on the assumption that his system is at rest; that is to say, his measurements will be made by means of the units belonging to his system and no corrections will be made on account of the motion of the system. Let the observer on S_1 reflect a ray of light SA from a point A to a point C on l_2 and back to A ; and let the observed time of passage of the light from A to C and back to A be t . Since the observer assumes his system to be at rest he will suppose that the ray of light passes (in both directions) along the line AC which is perpendicular to l_1 and l_2 . His measurement of the distance traversed by the ray of light in time will therefore be $2d$. Hence he will obtain as a result

$$\frac{2d}{t} = c$$

when c is his observed velocity of light.

Similarly, an observer on S_2 , supposing his system to be at rest, finds the time t , which it requires for a ray of

* Note that Postulate V is required to make this hypothesis legitimate.

light to pass from B to D and return, the ray employed being gotten by reflecting a ray SB at B . Thus the second observer obtains

$$\frac{2d}{t_1} = c_1$$

when c_1 is his observed velocity of light.

Now for the assumed relations among the systems S , S_1 and S_2 , and from the homogeneity of space, it follows that the two observations which we have supposed to be made must lead to the same estimate for the velocity of light. This is readily seen from the fact that the observations were made in such a way that the effect due to either the numerical value or the direction of the motion of the systems S_1 and S_2 is the same in the two cases. In other words, if we denote by L_1 and L_2 the quantities measured on S_1 and S_2 respectively, then the relation of L_1 to S_1 is precisely the same as that of L_2 to S_2 ; and hence the numerical results are equal, as one sees from the definition of the systems of reference. Therefore we have $c_1 = c$.

Let us now suppose that the observer at A is watching the experiment at B . To him it appears that B is moving with a velocity v , since by hypothesis the two systems have the relative velocity v , and A and B measure this velocity alike. We shall assume that the apparent motion is in the direction indicated by the arrow in the figure. To the observer at B it appears that the ray of light traverses BD from B to D and returns along the same line to B . To the observer at A it appears that the ray traverses the line BEF , F being the point which B has reached by the time that the

ray has returned to the observer at this point. If EG is perpendicular to l_2 and d_1 is the length of EF as measured by means of units belonging to S_1 , then, evidently, GF (when measured in the same units) is βd , where $\beta = v/c$ and \bar{c} is the (apparent) velocity of light as estimated in this case by the observer at A . From the right triangle EFG it follows at once that we have

$$d_1 = \frac{d}{\sqrt{1 - \beta^2}}.$$

Now, if \bar{t} is the time which is required, according to the observer at A , for the light to traverse the path BEF , then we have

$$\frac{2d_1}{\bar{t}} = \frac{2d}{\bar{t}\sqrt{1 - \beta^2}} = \bar{c}$$

So far in our argument in this section we have employed only those of our postulates which are generally accepted by both the friends and the foes of Relativity. Now we come to the place where the men of the two camps must part company.

Let us introduce for the moment the following additional hypotheses :

Assumption A . *The two estimates c and \bar{c} of the velocity of light obtained as above by the observer at A are equal.*

Now we have shown that c is equal to c_1 , hence we may equate the values of c_1 and \bar{c} given above ; thus we have

$$\frac{2d}{t_1} = \frac{2d}{\bar{t}\sqrt{1 - \beta^2}} \quad \text{or} \quad t_1 = \bar{t}\sqrt{1 - \beta^2}.$$

But t_1 and t are measures of the same interval of time, t_1 being in units belonging to S_2 and \bar{t} being in units belonging to S_1 . Hence to the observer on S_1 ,

the ratio of his time unit to that of the system S_2 appears to be $\sqrt{1-\beta^2} : 1$. On the other hand, it may be shown in exactly the same way that to the observer on S_2 the ratio of his time unit to that of the system S_1 appears to be $\sqrt{1-\beta^2} : 1$. That is, the time units of the two systems are different, and each observer comes to the same conclusion as to the relation which the unit of the other system bears to his own.

This important and striking result may be stated in the following theorem :

Theorem III. If two systems of reference S_1 and S_2 move with a relative velocity v_1 and β is defined as the ratio of v to the velocity of light estimated in the manner indicated above, then to an observer on S_1 the time unit of S_1 appears to be in the ratio $\sqrt{1-\beta^2} : 1$ to that of S_2 , while to an observer on S_2 the time unit of S_2 appears to be in the ratio $\sqrt{1-\beta^2} : 1$ to that of S_1 (MVLA)

Let us now bring into play our Postulate R' . In Theorem I we have already seen that a logical consequence of M and R' is that the velocity of light, as observed on a system of reference, is independent of the direction of motion of that system. Now if c and \bar{c} as estimated above, differ at all, that difference can be due only to the direction of motion of S_1 , as one sees readily from Postulate R' and the method of determining these quantities. Hence the statement which we made above as Assumption A is a logical consequence of Postulates M and R' . Therefore we are led to the following corollary of the above theorem :

Corollary. Theorem III may be stated as dependent on (MVL R') instead of (MVLA).

Let us now go a step further and employ Postulate R'' . From Theorem I and Postulates R' and R'' it fol-

lows that the observed velocity of light is a pure constant for all admissible modes of observation. If we make use of this fact the preceding result may be stated in the following simpler form :

Theorem IV. *If two systems of reference S_1 and S_2 move with a relative velocity v , and β is the ratio of v to the velocity of light, then to an observer on S_1 the time unit of S_1 appears to be in the ratio $\sqrt{1 - \beta^2} : 1$ to that of S_2 while to an observer on S_2 the time unit of S_2 appears to be in the ratio $\sqrt{1 - \beta^2} : 1$ to that of S_1 (MVL R).*

Let us subject these remarkable results to a further analysis. Theorem III, its corollary and Theorem IV all agree in the extraordinary conclusion that the time units of the two systems of reference S_1 and S_2 are of different lengths. Just how much they differ is a secondary matter ; that they differ at all is the surprising and important thing. As Postulates M, V, L , are generally accepted and have not elsewhere led to such strange conclusions it is natural to suppose that the strangeness here is not due to them.

Referring to the argument carried out above, we see that no unusual conclusions were reached until we had introduced and made use of Assumption A . Moreover we have seen that this assumption is a logical consequence of M and R' . Further, R'' is not involved either in Theorem III or in its corollary. But these already contain the strange features of our results. Hence the conclusion is irresistible that the extraordinary element in these results is due to Postulate R' —or, to speak more accurately, to just that part of it which it is necessary to use in connection with M in order to prove A as a theorem.

This result is important, as the following considera-

tions show. Postulates V and L state laws which have been universally accepted in the classical mechanics. Postulate M is a direct generalisation from experiment, and the generalisation is legitimate according to the usual procedure of physics in like situations. Postulate R' is a statement of a principle which has long been familiar in the theory of light and has met with wide acceptance. Thus we see that no one of these postulates, in itself, runs counter to currently accepted physical notions. And yet, just these postulates alone are sufficient to enable us to conclude that time units in two systems of reference are of different magnitude. In the next section we shall show on the basis of the same postulates that the corresponding units of length in two systems are also different. Thus the most remarkable elements in the conclusions of the theory of Relativity are deducible from Postulates M , V , L , R' alone; and yet these are either generalisations from experiment or statements of laws which have usually been accepted. Hence we conclude: *The theory of Relativity in its most characteristic elements is a logical consequence of certain generalisations from experiment together with certain laws which have for a long time been accepted.*

One other remark of a totally different nature should be made with reference to the characteristic result of Theorem IV. It has to do with the relation between the time units of the two systems. This relation is intimately associated with the fact that each observer makes his measurements on the hypothesis that his own system is at rest, while the other system is moving past him with velocity v . If both observers should agree to call S fixed, and if further in this modified

' universe ' our Postulates V , L , R , are still valid, it would turn out that the two observers would find their time units in agreement. But, in view of M , the choice of S as fixed would undoubtedly seem perfectly arbitrary to both observers, and the content of the modified Postulate R would be essentially different from that of the postulate as we have employed it. Hence, if we accept R as it stands—or, indeed, even a certain part of it, as we have shown above, we must conclude that the time units in the two systems are not in agreement, in fact, that their ratio is that stated in the theorems above."

So far I have quoted Professor Carmichael literally and fully.

Let us examine this demonstration closely. The first point that attracts special attention is that where Postulate M is called into evidence. Remember that S is treated as a system of reference, and though S is a "single light source," a light source has no meaning in the discussion unless the light be transmitted, and that transmission implies a medium. Now whatever that medium may be the argument will remain unaltered so that for brevity we will speak of it as the ether. But in that case Postulate M does not hold as we have seen, unless the ether is carried along with the system S_1 , and since S_1 has a velocity $v/2$ relative to S then Postulate M loses all application; and the conclusion drawn that "neither observer is able to detect the motion" is false. This is, moreover, easily seen independently of these arguments. It is also false to say that the observer "will make his observation on the assumption that his system is at rest," and that no corrections will be made on account of the motion of the system.

If the observer on S_1 reflect the ray from A to a point C he will not be able to reflect it "back to A ." That line of reflexion looks quite well in a diagram, but frequently in examining the doctrine of Relativity I have noticed the kind of error produced in assuming that a diagram truly represents phenomena of nature. Thus if the ether moved with the system S_1 , just as the air in a closed compartment of a railway train moves with that compartment, then it would be possible to cast an object which also partakes of the same movement, from one side to another from a point A to C to have it returned to A ; but if the object be carried in the volume of air occupying the compartment for a moment but not transported along by the compartment, then by the time it arrived at the other side, C , as well as A , would have moved along a certain distance, and the object would meet the opposite side at a point, D , let us call it; so that $CD=f$; and during its return journey A would have passed along at a distance equal to $2f$ from its position when the object was first cast.

Now the Relativitist may set down his result, according to Professor Carmichael, as $\frac{2d}{t}=c$; but he would be making one of his customary errors.

Similarly for the observer on S_2 . He would, by a similar method of false reasoning, obtain the formulæ $\frac{2d}{t_1}=c_1$, where c_1 is his observed velocity of light.

An objection may be laid here that I have reasoned on the case of a material body moving in the atmosphere; but that was employed only by way of illustration. It is easy to transfer all the terms to the conditions set forth; and as we have here the veritable

crux of the whole theory of Relativity, according to the claim of the Relativitists themselves, I will now deal with the actual proposition.

The system S_1 is moving relatively to the source of light S at a velocity, $v/2$, and let us suppose that the movement of S_1 is to the left, as in the diagram. A beam of the light from S strikes S_1 at A , and is there reflected to a point C in the system S_2 which is moving parallel to S_1 , but in the opposite direction in regard to the source of light S . The reflected beam is represented as striking S_2 perpendicularly to a plane in S_2 which reflects it at once along the line AC but in the reversed direction. We may accept this perpendicular impact on the plane, for that is merely a matter of appropriate arrangement of the conditions. Now as S_1 is moving to the left, and as the beam of light takes some time to return on the same line for the same distance, then A , having moved on, the light reflected from C will not meet S_1 at A , but at a point in S_1 to the right of A . In order to arrange so that a beam from A sent to S_2 and reflected from S_2 should reach S_1 at the point A , it would be necessary to reflect the beam from A so as to fall not perpendicularly on the reflecting plane in S_2 , as shown, but obliquely, reaching S_2 further to the left, so that then by reflexion the beam will meet S_1 at the point to which A had been carried by the motion of S_1 .

We are here speaking, it must always be remembered, of the relative motion of S_1 and the luminous point S , so that whether S_1 moves to the left while S is considered stationary, or S moves to the right while S_1 is considered stationary, the effect in regard to the processes considered will be the same. In this

one particular I am in accord with the Relativitists, and I state the whole matter in this tedious style so as to allow no loophole for ambiguity. We may therefore keep in mind the picture of S_1 moving to the left.*

Here, however, we have the opportunity of showing the sophism that underlies the enunciation of Postulate M. If S_1 be considered alone without reference to any other system, M has no real meaning at all, for the words "unaccelerated motion" require a reference to another system. But we see here from the diagram given that if the beam of light from S be reflected at A so as to be directed perpendicularly at a plane in S_2 then this requires placing the reflecting surface at a certain angle, measurable, with regard to that plane. As S_1 moves, that angle varies, and the movement of S_1 can therefore be detected by units belonging to S_1 . But it may be said: "Yes, detected as a movement relative

*It is true that if we supposed S_1 at rest, and S moving to the right with the velocity of $v/2$, and if the emission of light at S were such that its direction was unaffected by the velocity of S , that is to say, that the medium of transmission did not participate in the movement of S , then a beam sent to meet S_1 perpendicularly would be reflected perpendicularly; and if it fell on another system S_2 perpendicularly, it would be reflected back again perpendicularly to the point of departure on S_1 . But in this case S_2 would require to have a movement in the same direction as S , and, with twice its velocity, in regard to S_1 . This conception brings in a want of symmetry of the disposition of S_1 and S_2 in regard to S , and it is not the case stated by Professor Carmichael. The want of symmetry would only be in the conception, not in the reality, if S , S_1 and S_2 were mere points; but here again we are dealing with physical facts, and we must not be deceived by the diagrammatic representation of these. There is the assumption previously noted that the transmitting medium does not participate in the motion of S_1 but is at rest in respect to S_1 ; but when, as Professor Carmichael does, we study corresponding conditions from the side of S_2 we should have this medium at rest in respect to S_2 . We must also assume that the direction of the beam is unaffected by the velocity of S_1 and that is a question, never solved, of physics. We must also assume, what is false, that Postulate M is, under all conditions, true.

to S ; but that is not what we mean." The only other meaning is that of movement relative to itself !

Now, all this is so clear that I pondered on the question from another standpoint. How is it that it was not almost obvious to Professor Carmichael and other excellent mathematicians among the Relativitists ? Then I had a flash of light, which gave the explanation. Why is it that some mathematicians have accorded their support to this Relativist theory, and why the Relativitists themselves, in perfect good faith, have been led to accept their own inconsistencies ? So this is it ! This is the " murdered man " I cried ; and yet, as showing that however minute and laborious may have been my researches, no matter how dry-as-dust my exposition, yet even pale students of abstruse things may have their moments of joy and expansion, I laughed ; I cried again, "*l'anguille sous roche, voilà*" (the eel under the rock, it's there), and remembering a talented and cherished American friend of mine, in another idiom, " So here's the nigger in the wood pile."

In order to make the meaning clear I must again retrace something of what has been said, though with a different intention, and since it does not matter whether we consider S_1 at rest or S at rest, let us consider S_1 at rest. Also, since Professor Carmichael allows it, let us assume an ether and, quite apart from the movement of S , let the ether be at rest with regard to S_1 . Under these conditions there is no difficulty in accepting the first part of his demonstration, of the beam sent from A to C , and back again from C to A . The equation he has given also stands.

Now we come to S_2 . Here at once we find difficulties, for if the ether be at rest with regard to S_1 , then

S_2 has a motion, with velocity v , with regard to the ether; and if a beam of light from S be reflected, by a mirror placed suitably, at a point B on S_2 to a point D on S_1 , then it will not be reflected back on the same line DB , so as to reach the point B whence it started, for B will have moved to the right. In order to reach B the beam would have to be directed to a point E on S , so that by reflexion at E it would be carried to the point F at which B would have arrived.

Yes, replies the Relativitist, but you have assumed an ether; you have no ground for that assumption. We assent for the sake of argument, and ask, then how is the beam of light transmitted? The Relativitist replies: "It is an electro-magnetic phenomenon." "Very well," we say, "but by what medium is it carried?" "By no medium. The electro-magnetic phenomena are produced in space." "Yes, but space then becomes the medium; let us trace the line of action in space." "Space is not a medium. Space has no substance. Space has neither lines nor points, for how can you conceive a point without reference to something? Space is simply space. And therefore not only S_1 has no motion in regard to space, but the same is true of S_2 . Therefore our second equation also stands. The whole theory is in fact worked out, without any necessity to consider a medium; everything is worked out by electro-magnetism and mathematical formula."

Here then is the crucial argument! Let us re-assemble our wits, for it behoves us to reply closely. Even if light be but electro-magnetism, whatever that may be, and though the ether may not exist, yet the

point A on S_1 may be held to exist, since the Relativitists refer to it, and also a point C on S_2 . Now light has a certain velocity. The Relativitists have made many statements about this velocity, some of which appear to me absurd ; but that proves at least that they accept the fact of a velocity, and moreover, a finite velocity.* And that is true of electro-magnetism ; in fact, the formula of Lorentz, so often employed by the Relativitists, implies a propagation of electro-magnetic effects at a finite velocity. That being so, then even if space has no points of reference, as space, that does not prevent us drawing a straight line between A and C , where C is merely the point where the straight line perpendicular to the direction of motion of S_2 , strikes S_2 . This straight line has points, and the rate of progress of the electro-magnetic effects along this line is susceptible of being traced and estimated at any point. In short, there is a velocity to be measured along this line, and, again, according to the Relativitists, this is the velocity of light. During the time this effect has occupied in arriving at S_2 , the point C that would have been met if the velocity had been infinite—that is to say, if the transmission had been instantaneous—will have moved to the right. The point of meeting will be another point, which, however, might be indicated by the same letter. The Relativitists assume that the reflexion will be instantaneous, and untroubled by the movement of S_2 . Then we get the return to the point A .

Now try to adopt this reasoning, beginning with S_2 . The electro-magnetic effect traverses the space B to

* If the velocity of light were infinite both the equations given would hold.

D , and becomes reflected; but on account of the motion of S_2 it does not become reflected to B .

The second formula is not true.

What I have done here is simply to translate into the terms used by the Relativitists—while accepting for the moment their unproved physical theories—the reasoning I have already used to show the falsity of the conclusions with respect to light. To those who say that this last exposition is almost intolerably fastidious and tiresome, I say I agree; but it is necessary to deal with the matter with greater regard for the scrutiny of each step, and with more elaborate and clearer statement, than the Relativitists have shown. I have indicated how, perhaps not wilfully, they have profited in their reasonings by their own obscurity and false assumptions and confusions; and I have, to use a phrase of Alexander Bain, rescued the truth from the very thickets of concealment.

It is not now so difficult to see why some mathematicians should have given their endorsement to the doctrine of Relativity, for the original error consists in substituting, for what is a matter of physics, an assumption leading to a mathematical formulation. The physicists themselves have been deceived by the substitution of space for ether, with the implication that as pure space has no velocity with regard to either system, and has no points of reference, then phenomena in space cannot be defined in respect to position or velocity. Finally, all the Relativitists have been deceived by the manner of expression, recondite, elusive, gradually shifting, fallacious all through, yet expressed as though in the style of rigorous exactitude.

It seems hardly necessary now to pursue to the end the argument by which the formula $\bar{c} = \frac{2d}{t\sqrt{1-\beta^2}}$ is obtained; but that formula has itself a special interest, therefore we consider it attentively.

Note: "To the observer at A , it appears that the ray traverses the line BEF ."

But E and F are actual points on physical systems, S_1 or S_2 , and, according to the conditions, the ray must have met the systems in these points; and of course the angle BFE which is equal to FBE is not a right angle. If then: "To the observer at B it appears that the ray of light traverses BD from B to D ," then that observer does not notice any difference between a right angle and an acute angle. That is possible; but the obliquity of vision of an observer, and the confusion of different things, are after all insecure foundations for a *Welterschütternd* (world-shaking) theory.

But let us proceed. Assumption A is now a false assumption; c and \bar{c} cannot be equal; therefore the equation $t_1 = \bar{t}\sqrt{1-\beta^2}$ is false. And, of course, Theorem III, which states that "the time unit of S_1 appears to be in the ratio of $\sqrt{1-\beta^2} : 1$ to that of S_2 " is false.

A little further examination will show it to be absurd. In the first place, what is meant by a time unit? One finds much talk, of which we may make a note later, in books on Relativity, of perfect clocks as measures of time. Therefore we may take these, on their basis, as allowing us to read the units of time. Amongst many extraordinary results of their theory is one referring to clocks, for they say that a clock changes in its measurements of time when it is transported on a moving system. This, if true, would be purely a physical fact,

not deducible even from a profound analysis of space and time, but they advance nothing to show that it is true ; and by their own postulates we can show that it is inconsistent with their theory. Thus, for instance, even if the clock be changed by being carried along a moving system, yet, if the two systems, S_1 and S_2 , were travelling in a parallel direction at the same velocity, and each carried a clock which was in all respects like the other, they would agree in their measures of time. Now, in the problem under consideration, we are measuring the velocity of light, and by Theorem I the velocity of light is independent of the direction of motion of the system on which it is measured. Therefore, if the systems S_1 and S_2 had equal velocities but in opposite directions the clocks would give coincident measures. It must be noted that even a change of rate of the clocks due to the velocity of the systems would not matter as we are dealing in the differences noted after the clocks have been working on the new regime. The movements of the clock are not in fact governed by our experiments on light, but by the physical conditions of their manufacture and the forces that play on them, such as gravitation and other physical forces.

Yet at the end of the long and involved reasonings we have these new elements introduced, and we are told that the "time units" of S_1 and S_2 are in a certain ratio.

This fact alone should have given the Relativitists pause, and made them retrace their steps and search for sources of error. They would then have seen that the apparent discrepancy has arisen from the error due to the assumption of difference of route, according to the suppositions set forth, of the beam of light.

The rest of the citation need give us but little trouble, for it consists of the assertion of the truth of the postulates and theorems which we have seen to be false. I am glad for Professor Carmichael's sake, however, that he recognises that he has been led to an "extraordinary conclusion"; the extraordinary conclusion should have led him to examine the implications of the sentence beginning "If both observers should agree to call S fixed . . ." He says they would find their time units in agreement. But it is not only in the theory of Relativity that this assumption is tantamount to any other that expresses the relative movements; it is in accord with the form of relativity known to Galileo, and much more easy to understand than that of Descartes and Poincaré in reference to the concordance of a geocentric and a helio-centric solar system; and yet, on this assumption, the time units agree. In other words the discrepancy of the time units, as asserted by the Relativitists, is due not to any physical conditions, but varies with our manner of describing a situation that remains unchanged! Professor Carmichael concludes, however, that the recognition of the agreement of the time units would show that Postulate R was false; and he also confirms what I have mentioned that on Postulate R , or on Postulate R' , from which R is derived, hangs the test of the whole theory. Well, I have shown, in rigorous demonstration, that Postulate R' is false. That is the gist of the matter, and this thorough-going Relativitist has been a precious witness in the case against Einstein.

CHAPTER VIII

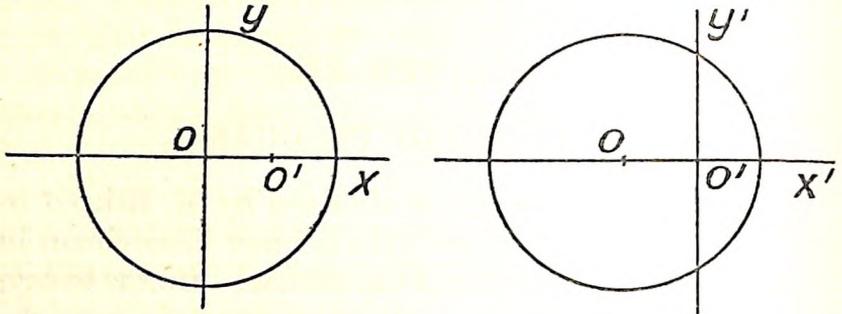
THE FETISH OF INVARIANCE

THIS formula of Lorentz is obtained by M. Bricout in quite a different manner. He is more Einsteinian in his methods than Professor Carmichael ; that is to say, whereas the American mathematician falls into the second danger indicated by Jacobi, that of long and tortuous reasoning where the truth is eluded on the way ; M. Bricout inclines to the first, that of a statement so abrupt and devoid of proof that one can always trust to its obscurity to defy refutation. Lest, however, I should be considered as lacking in fairness, I give M. Bricout's account here in full :

“ Group of transformations of Lorentz. We will consider two systems of reference : one, A , in repose, the other possessing in regard to the first a movement of translation of uniform velocity. It is possible, without changing the nature of the problem, to choose the trihedrons of coordinates in such a manner that these coincide at the initial instant $t=0$, the axes of the x (coordinates) being placed parallel to the direction of translation. Denote by ordinary letters the coordinates of the system A and by accented letters the coordinates of the system in motion B ; one passes from the first to the second by the classic formula (group of Galileo)

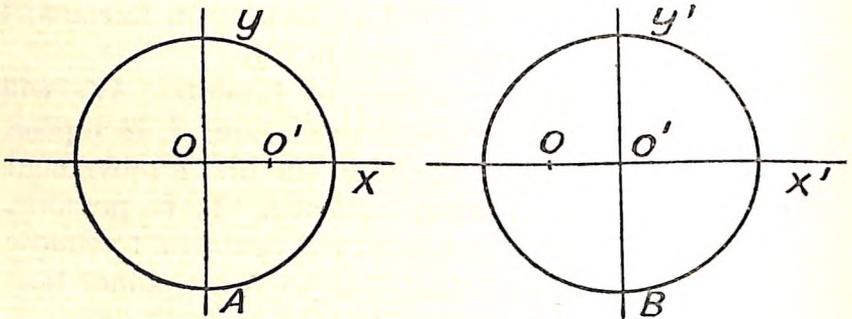
$$x' = x - vt, \quad y' = y, \quad z' = z, \quad t' = t.$$

The fourth equation expresses that the time is the same in the two systems (absolute time). We see immediately from the diagram that if we consider, at



the instant $t=t_1$ a luminous wave produced at O at the zero of the times, this will appear *in the two systems* as a sphere of radius ct , with centre at the point O .

The principle of special relativity states on the contrary that the surface of the wave is a sphere with



centre O in the system A and a sphere with centre O' in the system B (see diagram). Analytically, that condition is expressed in writing that the equations of propagation of the wave

$$\square h = 0, \quad \square H = 0$$

are identical in A and in B . The group of transformations of the special relativity should therefore have

the property of leaving the dalembertians invariant. Lorentz has shown that the transformation :

$$x' = \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}(x - vt), \quad y' = y, \quad t' = \frac{1}{\sqrt{1 - \frac{v^2}{c^2}}}\left(t - \frac{vx}{c^2}\right)$$

has that property.

The presence in the denominator of the term $\alpha = \sqrt{1 - \frac{v^2}{c^2}}$ shows that the *units of length and of time are different in the systems, A and B*, the notions of length and of absolute time usually admitted in the classical mechanics losing here all meaning.

It is well to remark, however, that in every case in which $\frac{v}{c}$ is sufficiently small, α is very near unity, and the formulae of Lorentz coincide with the classical formulae which appear thus as a *first approximation*."

It will be noted here that though M. Bricout arrives at the same conclusions as Professor Carmichael, he dispenses with the paraphernalia of supposed demonstration of his American confrère. Apparently, then, he believes that the conclusion does not depend on the postulates and theorems we have seen. His own proof, however, makes no less demand on mere improbity of reason; and on carefully comparing the two systems it will be discovered that they have a common ground—the assumption of a medium which is in repose in regard to two systems that themselves are relatively in movement. We have confirmation of what I have said about the gist of the whole Einsteinian theory.

It is remarkable also that while Professor Carmichael seems to accept the ether and M. Bricout does not discard it, and at least, in another place, recognises local

measurable effects of electro-magnetism ; yet to obtain accord with Einstein they accept this supposition of an indefinable space, and then attribute also to the phenomena of electro-magnetism or light this quality of being elusive to comprehension. It is worth while reading again and very attentively the reasoning of a man of acute intellect in order to be convinced that the hypnotism of a false creed—for it now amounts to that—may lead him to talk sheer nonsense ; for here we are face to face not with metaphysical imaginings but physical happenings. If a wave be created with centre O of a system A , and of another system moved from O so that its centre, which was at O , comes to O' , then by that fact of simply moving an immaterial system, that is to say, an imagined system, physical happenings follow that system. If there were another imagined system the physical happenings would also follow that, so that simply by moving imagined systems we could have innumerable centres of light propagation. It may be objected : No, but observers on these systems would believe that these various centres existed. That plea is not valid ; if we have merely a confusion of impressions, or an effect somewhat analogous to that of perspective, the whole theory loses its meaning. Moreover, the waves of propagation, whatever be the medium, are real ; so are units of measure implying instruments of measure, both of time and distance.

In order to indulge in the play of tracing out possible results of the theory that measures of time might vary by being transported on imaginary trihedrons, and that time could be hastened or retarded accordingly, I have taken a drama in which a black-avisaged wight

suddenly rises with a hammer in his hand and smites a red-haired buxom wench on the head. By arranging the transmission of signals through the ordinary world, and through the Einsteinian transcendental world, where we can lengthen or shorten time and space, I have found it possible that the red-haired buxom wench by a motion of her head dealt a blow at the black-avisaged wight and knocked him down.

In fact I have found quite a series of alluring combinations; and again I say that if a man is able to play tricks with reality and reason in order to create, or discover, fresh universes, the Relativity paradise seems to me a miserable outcome, this spectral stuff of a mind awry in a *Götterdämmerung* of false mathematics.

In order now to pierce down to the point where we find the suggestion that has led the Relativitists astray, a short reference to the meaning of invariance is again necessary. Let us take the case of a physical invariance which is independent of the system of reference employed. Thus, for instance, the length of an object, which remains undisturbed in regard to the physical conditions affecting it, is the same whether measured by means of one set of coordinates or by means of another, though the form of the expressions may be different. The Relativitists, also most of the physicists, who are not followers of Einstein, say that the "fundamental law of the classical dynamics" has a property of invariance when a change is made from a given system of reference to another which differs from it in having a velocity of translation with regard to it. This law given as

$$F = mg$$

which being interpreted means that, with suitable units,

the number expressing the force is equal to that of the product of the mass, on which the force is impressed, by the acceleration, or rate of change of velocity produced by the application of the force. The change of velocity may be from a zero velocity, the mass having been supposed to be at rest.

If the mass were not at rest but moving with a uniform velocity, and if then a similar force be impressed on it, it is assumed that the acceleration, that is to say the added velocity, would be the same as before. Now Einstein observed, in dealing with the electro-magnetic formulae of Maxwell, that under conditions of changing the system of reference this invariance of form was not obtained, and he thought it essential to secure this invariance. If, however, the change of the coordinates were carried out by means of a certain "transformation of Lorentz," then the results were concordant, the invariance was saved. The next step was to seek to make this transformation of Lorentz applicable to cases of quite another character, and where in any case its applicability should have been carefully ascertained and rigorously demonstrated. The attempt to introduce these formulae resulted in the contradictions with which we have already dealt.

But let us examine, even at the expense of repetition, why, in the first case, we get invariance in the formula

$$F = mg.$$

g , the acceleration, is the differential coefficient with respect to time of v the velocity. The term differential coefficient means a rate of change. If anything, expressible as a measurable quantity, changes in magnitude, the change is dependent on something, producing

the change, being itself changed. This something is called the variable, and the thing changed, as a result of the change of the variable, is called a function of the variable. The differential coefficient is the rate of change of the function compared to that of the variable, the rate being taken at the moment of change, that is to say, when the change is infinitesimally small.

Now if the velocity be constant there is no change in its value, and the differential coefficient is zero, as of course it is for all constants. If on account of an applied force there be a change of velocity, then the rate of change at the moment, as compared to the increase of the measure of time, is the differential coefficient of the velocity in regard to the time, and it is called the acceleration. If the body had been at rest the differential coefficient is the rate of the imparted velocity compared to the time, the increment of time considered being infinitesimal; and as the effect of the force is taken to be the same in imparting a new, or added velocity, whether the body be originally at rest, or in motion, the acceleration will be the same in both cases*; that is to say, the differential coefficient of the constant velocity is zero, and the acceleration, as the term implies, is obtained entirely from the added velocity. Hence we get the invariance of the formula $F = mg$.

If, however, we examine the matter more closely, we will find many questions to be asked.

What is force ?

If a man pushes a heavy obstacle he finds that the work is accompanied by various feelings and emotions which few have ever taken the trouble to analyse, and

* M. Painlevé terms this the "principle of Galileo."

of which indeed the analysis is complex and difficult.* If the obstacle resists pressure then amidst the complex impressions produced by the exertion of displacing it there is a definite recognition of a special feeling involved in the concentration on the particular muscular effort required in overcoming the resistance. To this a name has been given in all languages. We call it force. When then we see one object strike another, as for instance, in billiards, we say that a force has been in operation, without looking very deeply into the meaning which we attach to the word. But as forces evidently vary in effect, we seek some method of measuring them, and we find this in the estimation of the velocity they impart to certain masses, and finally to something we select as the unit mass. The velocity may be superimposed on a velocity which the mass has acquired; we call this the acceleration, and the term still applies when the mass has been at rest. Here again we have had a laborious, and even tedious explanation; but it has been necessary, for it shows us that the formula $F = mg$ is not a law of dynamics, but at most a definition of force.

That definition is itself vague, and since it is, moreover, only a measure of the effect of force, it is not a deduction from this law, but simply another assumption—however great may be the probability—that leads us to postulate that the same force would give the same acceleration to a mass in motion as to a mass at rest. We are far therefore from that invariance sought for by the Relativitists, not in experience of physical

* The Sense of Effort is the most debated of all the subjective impressions, and I have found it the most recondite of all the special subjects which I have studied in *Principles of Psychology*.

conditions but in the implications of mathematical formula. Here, as in every domain of this subject, their thought is not finer and keener than that of the Galileans ; it is obscure and confused.

Let us pursue the matter still farther, and for this reason ; the Einsteinians have made a sort of fetish of invariance, just as of old the followers of Pythagoras erected into a sort of mysterious religion the worship of numbers, and so prevented the proper development of the vital ideas of the great master. Invariance is the property of the dynamic world when the change of the system depends only on the change of velocity ; how fine a thing, they say, it would be to discover a mode of transformation that would preserve to us this invariance in all the laws of electro-magnetism as formulated by Maxwell, these being conceived also as giving us the true theory of light.

In the first place we see that invariance is not a quality of all phenomena when a change is made from one system to another moving in regard to it in uniform rectilinear translation. It is true of acceleration. But velocity itself is a physical phenomenon of the same order as acceleration, and by the very terms of the proposition the velocity is not an invariant in the two systems.

Further, the acceleration may in some cases not be an invariant. If the moving system of reference were carried through a field of attractive force, such as that of gravitation, where the attraction itself increased continuously in the direction of motion there would be no invariance of acceleration. There would not be any breach of natural law, of course, for Nature knows no paradoxes ; but there would not be a conformity to

the mathematical expression of invariance sought by the Relativitists. The invariance depends on the physical conditions of whatever phenomena we are considering, and, in regard to form, on the mathematical operations we perform on the terms of expression. Einstein leaves all this out of account.

The transformation of Lorentz, first introduced in 1867, which has led indirectly to the paradoxes of units of measure changing by being placed on mobile Cartesian coordinates, has in itself no mystery. If an electron in movement be considered for a moment at a point A , and if from this point a wave of potential be developed, this will arrive at a point M at a distance r_0 in a certain time. During this time the electron will have arrived at a point which we may call A_1 at a distance, say r_1 from M . Now evidently, since r_0 and r_1 represent two different lengths, if the potential at M be calculated, using r_1 as a measure, its expression will have a form differing from that when r_0 is used as the measure. The necessary change from one to the other constitutes a transformation of Lorentz, and when this was applied to functions representing potentials, the functions were called "retarded potentials."

It will be noted that this transformation has no special association with any particular system of coordinates, nor indeed with any special phenomena. If, instead of an electron at A , we had a sounding body, such as a whistling bullet, and at M an auditor, and if the bullet were in movement so as to arrive at A_1 when the air wave from A arrived at M , we would have a case for the application of the transformation of Lorentz. The transference of these terms to the case of the two moving systems and the source of light,

already examined, would be tedious, though any one who has read so far will find no special difficulty in making it. The result of the comparison may be stated; that the confusion is such that if the electron were a sentient being capable of ascertaining the velocity of the wave of potential he would be in error if, when he arrived at A_1 , he assumed that he was still looking at M in the direction AM . That would be wrong of the electron; and that in a more developed form constitutes one of the paradoxes of Relativity.

The connexion between the formulae of Lorentz and the principle of the special Relativity is emphasised by M. Bricout who gives a demonstration which he affirms shows the physical signification of the Lorentz transformation. It seems to me worth while to give it in his own terms, although it is in essence a restatement of the demonstration of Professor Carmichael.

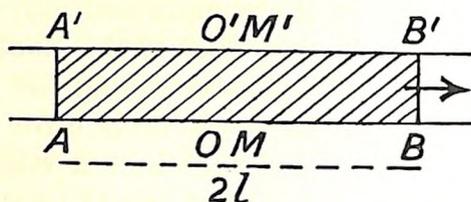
“ Applied to light, the principle of special Relativity may be thus enunciated: The propagation of light in space takes place with a constant velocity C , no matter what may be the direction chosen and the system of reference in use, provided that the system should not be accelerated.”

I interrupt here to remark that there is no proof adduced as to the constancy of the velocity of light, and that if such a proof were forthcoming it would be part of our physical data determined by experiment and observation; and in no case should it be set down as a fundamental principle, or postulate.*

The exposition continues: “ We will describe an experiment enabling us to realise, theoretically at least, such a measurement; the application of the

* Cf. note, p. 135.

principle will lead us to the formulae of Lorentz which will appear in their *physical significance*.



Consider (see diagram), a straight railroad on which a train is able to travel with a constant velocity v , little inferior to that of light. Let $2l$ be the length of the train when *at rest*: this length, indicated on the line by two marks, A and B , will serve as base to an observer at O , at rest in regard to the line, who desires to measure the velocity of light on the 'system represented by the line.' On the other hand, an observer, O' , furnished with rods and clocks identical with those at O , is ready to effect the same measures in the train which is in motion.

The train having acquired its velocity, O and O' synchronise their clocks distributed along the line and along the train, and place themselves respectively at the centre M of the base AB and at the centre M' of the train. At the precise moment when M and M' are in virtual coincidence, O gives out a signal which is received by O' ; the two observers agree to take that instant as the beginning of the time in their respective systems.

Propagation in line system. The waves produced at M at the instant $t=0$ arrive in A and in B at the instant $t_1 = \frac{l}{c}$.

Propagation in the train system. According to the principle of Relativity, the law of propagation of light

in the train is the same as in the line. The waves leaving M' at the instant $t' = 0$ arrive at A' and B' at the moment $t_1' = \frac{l}{c}$ (the time of the train).

Aspect of the train seen from the line. The preceding result announced by O' is completely incomprehensible for O . In fact, for him, the wave that is directed towards A' possesses a relative velocity of $c + v$ in regard to the train; it should arrive therefore at A' at the moment $t_1 = \frac{l}{c + v}$. Similarly, B' is not reached till the moment $t_2 = \frac{l}{c - v}$. Now, $t_2 - t_1 = \frac{2vl}{c^2 - v^2} \neq 0$. O therefore concludes that the clocks of the train that appeared synchronous to O' are not so in reality for him, and a simple reasoning enables him to determine that in his system of reference, the points of the abscissæ x which, at the moment t have the same time t' in the train-system, satisfy the equation

$$t - \frac{vx}{c^2} = \text{constant.}''$$

Here follow some mathematical calculations which are quite correct, and which lead to

$$'' x' = \frac{1}{a}(x - vt), \quad t' = \frac{1}{a}\left(t - \frac{vx}{c^2}\right).$$

“These are the formulae of Lorentz. We see that the accented letters represent the time and the lengths *really measured* by the observer at O' .

For the observer O :

- (1) *The unit of length employed by O' appears contracted parallel to the movement and reduced to a fraction of its value at rest.*

- (2) *The clocks of the train appear to go slow, the periods of their ticking being lengthened in the proportion of $\frac{1}{a}$ to unity.*
- (3) *The synchronism does not appear to be realised in these clocks, which show a different apparent timing, increasing with their distance."*

The demonstration here offered by M. Bricout need not detain us long after the attention which we have given to that of Professor Carmichael. If the ether—let us give that name for the present to any medium carrying the 'waves,' as M. Bricout himself calls them—be at rest in regard to the train, then the waves proceeding from M' at the moment $t' = 0$ would certainly not arrive at A' and B' at the same time, for A' approaches the wave with the velocity v , and B' moves away from the wave with the same velocity v .

The only case in which the formula given by M. Bricout for the arrival at A' and B' , $t_1' = \frac{l}{c}$, is correct, is where the ether is carried along in the train with the velocity of the train. That case is, however, inconsistent with the initial supposition of Einstein in his endeavour to explain the Michelson-Morley experiment, as we have already seen; had he accepted such an explanation the doctrine of Relativity would never have been born. Further, in that case there was the whole mass of the earth to be taken into account as affording attraction enough to carry along, if not the whole of the ether in close contact with the earth, at least a strip of it, though possibly with less velocity than that of the earth.

Moreover, as the train might move in any direction, and the argument does not preclude simultaneous experiments, another train, from which the same formula could be obtained, might be moving in another direction ; and the ether, to satisfy Einstein, would acquire an equal velocity in opposite directions. This sort of business would soon disrupt the universe, or alternately as the lawyers say, put Einstein's case out of court. It is for us to choose. I am on the side of the Universe.

We may now deal briefly with those paradoxes which at first may have seemed so entrancing in our metaphysical dreams, but now in the light of day are absurd. "The unity of length seems contracted." Yes, because it has been wrongly applied. The unity of length has nothing to do with our tricks of supposing coordinates in motion, and ethers to suit, either moving or resting, or non-existent at one and the same moment. "The clocks appear slowed down." Yes, any clock appears slow if we try to make it read a longer interval of time than exists. When the Relativitists speak of a clock being slowed while in motion, it must be remembered that they do not invoke any physical effect, as from a jolting train ; these trains are merely modes of expression, but they suffice to slacken a material clock. The motion of the train to the line is moreover relative ; so that if the train were at rest and the line, which is also fictitious, were slid under it, the clock would still be slowed down ; and if in another simultaneous experiment a line were slid at another speed, the clock would be slowed down at a different rate. But, reply the Einsteinians, the slowing down is not real ; it is only apparent. Well, once again, has this

enormous mystic construction of Relativity come to this—that it means simply a misreading of a clock, and a misinterpretation of the conditions of a physical experiment ?

Before leaving this aspect of the discussion, it may be well to remark that in indicating the points in the chain of argument of the Relativitists, where their characteristic errors found entry, I did not strain, for my own side, the reasoning they set forth. The weak points on which I put my finger are present in the writings of all the Relativitists, although Einstein himself is both so obscure and so perfunctory in his demonstrations that it is more difficult to join issue with him than with some of his disciples. Ah, then, the Einsteinians cry, You cannot controvert the master ! I reply, I can and I do, in regard to the germinal idea, the whole tenor of his argumentation, and his conclusions, for they are all unsubstantial ; and, if unwarranted assumptions, loose arguments, and the abrupt conclusions which he offers are assets, then it is easy to make absurdities acceptable ; and that is what Einstein has done.

It is not the astute medicine man who is beaten in fair combat, it is the valorous braves of the tribe who, trusting to their lack of discretion, rush to the fray, and offer themselves as sacrifices for their faith. After Sir Arthur Eddington, Professor Carmichael, to say nothing of a host of those, from Sir Oliver Lodge to Bernard Shaw, whose war demonstrations reveal rather the weakness of the camp, we come again to the excellent M. Bricout. He fears nothing, not even opening the gate to the enemy. I quote his exact words, and it will be seen that they reinforce mine :

“ Philosophical value of the theory of special Relativity :—Many people and several philosophers cannot bring themselves to accept the paradoxical results to which the theory of Einstein conducts them. That is perhaps due to the fact that they have not formed an idea sufficiently precise of the technique of physical measurements. If it is possible to *conceive* the absolute simultaneity of two distant phenomena, we are on the other hand incapable of verifying *directly* that property since *all* the known agents of signalisation possess a finite velocity. The theory of Relativity is above all a *physical theory* created by a physicist with a view to defining with precision happenings *directly measurable*. It is founded on two facts verified experimentally in the actual state of science : the absence of points of reference in space and the isotropic propagation of light, and from these it deduces a very precise definition of *simultaneity capable of being submitted to the control of experiment*.

“ What more could we demand of it since, according to the saying of Pascal, ‘ definitions are made only to indicate the things that we name and not to show their character ’ ? ”

Here I beg to direct attention especially to the two ‘ facts ’ on which Relativity is founded. Note the phrase, “ the absence of points of reference in space ” ; for in the writings of other Relativitists, including Einstein, it required long analysis, careful elimination, and yet withal the demand for the factors of demonstration, however faulty, before I came upon this discovery, before I was able to cry delightedly, “ Lo, I have touched it with a little rod.” On the other hand, had I read Bricout alone I would have puzzled to know

what he meant. Here then is the verification to the full of what I have said on pages 93 to 98.

The other phrase "the isotropic propagation of light" is a pure assumption. It may be true, but if so, it could only be stated as the result of observation and experiment, and the conditions of such as would be sufficiently ample and yet stringent are perhaps beyond our present powers. If the Relativitists' doctrine of a bounded space were acceptable, or even if it had any meaning, there would be implied a centre of ether and the relation of direction of our chief phenomena to this centre should be taken into consideration before such a statement could be made. As a matter of fact, the symmetry implied is doubted by many physicists, for M. Esclangon, the director of the Observatory at Paris, has made many observations and experiments which had led him to note an "asymmetry in space." * Other physicists believe that this asymmetry is very small; but the search for such asymmetry shows how hazardous is the statement, and how far removed it is from the simplicity of a postulate.

The affirmation of this 'fact' is, however, used, as we have already seen, to give it, by gradual extension, a much wider meaning; firstly, that the velocity of propagation does not depend on the velocity of the source—and this, if true, is something that belongs to the realm of physics, only to be determined by experiment—; and secondly, that the relative velocity of light received at a system has no dependence on the velocity of that system; and that, though a generation of Einsteins were to affirm it, is absurd.

The other statements I reserve for a special chapter.

* Cf. *Le Mois*, December 1931.

There is a fallacy in every sentence, and this I will prove not by taking refuge in any lack of precision, but by insisting on precision, and pressing this home to the last degree of scientific rigour.

The phrase "philosophical value of the theory of special Relativity" is in itself an unjustified plea. It affords, however, a vindication for the minute analysis, leading to the establishment of the Fundamental Processes of the Mind, on which I have insisted as necessary at the beginning of this book, but which is lacking not only in Einstein's work but in that of the whole pleiad of his followers. They talk like children on this subject, the science which should be the queen of the sciences. They have not shown the very commencement of such an analysis, they have never given a hint that they have posed the problem in the sole manner in which they could obtain a solution.

CHAPTER IX

THE CRUX OF SIMULTANEITY

It is not true that Relativity is a physical theory created by a physicist. Einstein himself is certainly a physicist of talent and erudition ; but as the creator of this doctrine he has not acted in the spirit of a physicist who obtains his data from nature and on this basis builds up a theory which gives meanings of relationships between these data and all that we know relevant to them in other phenomena of nature. In the ' Special Relativity ' his exposition is that of a metaphysician ; in his generalised Relativity, as we shall see, he employs a method of an extraordinary character, deducing explanations and interpretations, not from the study of concordances in nature, but by a manipulation of the coordinates of Riemann, employed in a manner that would leave that profound thinker aghast at the indifference to all warrant in the assumptions and at the lack of cogency in the deductions.

The special point I propose to deal with now, however, is one that is covered up in a peculiar verbiage. The Relativitists assume that the ordinary well-intentioned physicist is unable to follow their reasonings because he has been wrongly inducted into the science, and that as " all the known agents of signalisation possess a finite velocity," he is unable to find a

means of defining with precision happenings "directly measurable."

Now I hold no brief for the ordinary academic physicist, and this is true especially of those who have been influenced by Relativity. When I read their books I have a vague recollection of Jules Verne who, by a flood of scientific terms which seem perhaps at the time to have a meaning, and by arguments that have something of the character of scientific reasoning, brings us at length personally conducted to the moon, inside a large bullet. There is no heresy in making these remarks, for it is possible to multiply the cases where certain phenomena have received explanations, mutually exclusively, but each of which is held to be scientifically complete and definite by the particular school from which it emanates. In this science which in point of exactitude, precision, cogency of argument, should be at the highest, there has arisen a confusion, a fashion of abrupt assumptions, and a manner of speaking that more resembles the talk of the pre-Galilean schoolmen than that of serious, thoughtful, intelligent students of nature; and the big authorities are often the greatest offenders.

That being so, let us return to the cryptic phrase, cited above, of M. Bricout. The main point is that he denies that any means exist of ascertaining simultaneity in regard to two occurrences at distant places. Thus if an event happens in the sun it takes time for the evidence to be brought to our senses, for the velocity of light is not infinite. So far so good, and now in order to show the import of this statement I adjoin another in which M. Bricout interprets Einstein's ideas. "Einstein was the first to show that

the *only* case in which we could give a judgment absolutely objective and *absolute*, was the appreciation of the *simultaneity of two phenomena both in time and in space.*" (The italics, and the punctuation, as in all citations, are those of the original.) He adds later : " It results from that proposition that it is possible *with regard to a phenomenon to obtain a datum by a clock placed at the point where it is produced* ; that measurement, in effect, depends on the observation of the coincidence between the production of the phenomenon and the passage of the hand of the clock over its dial. Every measurement effected under conditions different to those which we have indicated raise, on the other hand, difficulties which we will study in detail."

The implication is, apparently, that Einstein, having introduced into physics better methods of determination of data, is entitled to ask our acceptance of Relativity.

We have already seen that it is one thing to secure precise measures, but quite another to ascertain what they really mean, and precise measures are not a compensation for inaccurate modes of reasoning. But with regard to the precision and objectivity of the measures, it is necessary to enter into the matter more deeply.

It was an attentive study of the paper of M. Painlevé, *La Gravitation dans La Mécanique de Newton et dans la Mécanique d'Einstein*, Ac. de Sciences, 14 Novembre, 1921, which first impressed me with the peculiar importance of this notion of simultaneity in the theory of Relativity. It is perhaps the 'germinal idea' of the whole doctrine. The line of thought might be thus expressed : We can only be certain of

simultaneity at the very place where two aspects of a phenomena occur. We must therefore contrive to arrange, or at least express, the conditions that are necessary for such simultaneity; and as, where these conditions are obtained, the descriptions of phenomena are independent of our structures of measurement, the expression of laws should be invariant for all changes of the variables.

We will consider simultaneity in itself; but in regard to invariance, it must be noted that the aspects of phenomena are generally by their very nature precluded from manifesting themselves at any one point under observation; and though there be invariant quantities to be noted the scientific determinations of these may necessarily be expressed in terms that do not preserve an invariant form when transformed from one system to another. That they do so is the most important, and the least warrantable, assumption of the Relativitists in this regard.

M. Painlevé himself expresses his criticism thus: "It is possible to deduce from the laws of nature consequences, invariant in every change of reference in space-time, and which define those laws, *à un tel changement près*"; that is to say, when the change is duly registered in the mode of expression. That last phrase, which makes a bridge from the region of unallowed assumption to lucid comprehension, suffices to cut away the ground from the Relativitists in regard to this aspect of the theory.*

In the first place, it cannot be said of the greater part of the phenomena of nature that they take place at a 'point'; but let that pass. Einstein has in mind

* Cf. p. 109.

either astronomical phenomena such as the passage of a star, or some other mechanical effect noted by similar means. But does he get 'objectivity' here that enables him to render an 'absolute' judgment? If he had reflected he would have remembered that one of the most famous cases of discovery, on the psychological side, in science, was that of the "personal equation" in observation. Maskelyne, the Astronomer Royal, had an assistant whose work was careful and in every way satisfactory except that, in the case where Einstein's statement is most applicable, that of marking the passage of a star across the meridian, the time given by the assistant always differed from that of Maskelyne. The Astronomer Royal never seems to have considered that the discrepancy may have been due to an error on his part, and he decided at length to dismiss his assistant. We know now that there was no special error due to either; it was seen that so far from being objective these measurements involved, in a very complicated manner, another instrument beside the clock, and that instrument was the organism of the individual who had to register the phenomena; it was a case of what has since been recognised as the "personal equation."

There is in books of Relativity a great deal of talk about clocks; clocks become almost sacrosanct as being the adjuncts of the theory, and still more so when they go fast or slow, through no change in their mechanism, but simply because they are placed on this or that system of imaginary coordinates. But the clock is itself a fabric of mechanics, and not less notoriously liable to error than humans. The Einsteinians say: We may suppose a perfect clock.

Granted. But between the clock and the percipient there is a passage of undulations of ether, for us at least, but for the Einsteinians something happening in an empty space that has no datum points, not even on the eye which these perturbations eventually reach. From the exterior eye to the retina there are whole series of delicate but quite real physical phenomena, involving refractions and changes of speed, differing in different individuals. Then when these waves strike the retina disturbances are set up in nerve terminations of a complex character, such that many theories, mutually exclusive, have been invented to account for them. Of these, for instance, the Young-Helmholtz theory held the field as conspicuously even as Relativity; but it was not even a theory of vision at all, but of 'objective' colour mixing; and Edridge-Green's researches have been of devastating effect among all these explanations which now decorate the limbo of dead doctrines.

From the retina onward the changes in organic tissues are so complicated and obscure that here even quite absurd theories still have right of city, while pious people reproach physiologists with 'materialism' for endeavouring to carry on the research. Then at length, and after a finite time—longer than a light signal requires to travel ten thousand miles—we have the result in consciousness, and with it a divergence of judgment. We are, in this simple example, far away from the 'objective' and the 'absolute' that Einstein claims. The Relativitists may say: We may suppose the perfect man. Yes, but what are here the standards? The models that I have so far seen offered have left me in perplexity.

On the other hand, with their clocks and their foot rules, or their graduated metres, the Relativitists assert the weakness of reasoning of those who do not parade these objects. Heaven forbid that I should speak ill of mechanical works, for, with all my admiration of the old Greek philosophers, I believe that it was their disrespect for mechanical work, and consequently for inventions, that proved to be the most serious limitation to the sphere of their intellectual activity. But we must distinguish. Mechanical measurements are necessary to give us data in the actual world, but a still more precious gift to man is that which the Greeks possessed—a faculty for clear reasoning on the data offered.

Let me come at once to the point. By reasoning in what might be regarded as an 'abstract' way, mathematicians have shown how to ascertain the dimensions of objects, for instance, the volumes of solids, or the ratio of the circumference to the diameter with a degree of precision unattainable by any mechanical measurements.* Certainly one may have a horror of the old metaphysical disquisitions, and yet keep a profound respect for the veritable analytical faculty in psychology, for that is the master-key to all the domains of science. Both the bent towards exact measurement, and the true philosophic activity in appreciating values and relations, have each its due place and its

* I remember that once in Paris a distinguished soldier and philosophic thinker, Colonel Monteil, showed me a mechanical device he had invented for measuring the length of the circumference in terms of the diameter. His results differed from those of the mathematicians, and he was angry with Henri Poincaré for not accepting his conclusions, but no intelligent man, studying the reasonings of both, would believe that Monteil had the better method.

proper role in scientific work, and they are not antagonistic but of material aid.

Consider for a moment the method of the 'abstract' mathematician. Euclid speaks of straight lines to which he gives a definition which is impossible to realise in nature. His geometrical figures being constructed of lines are also figments of the imagination. Yet this geometry has been the basis of a wonderful mathematical apparatus, by which eventually we are able to measure the distance of the earth to the sun, and to estimate also the velocity of light. These are feats which have not been accomplished by the application of metrical rods or the direct timing by a clock.

Euclid's straight line is an abstraction, but its use is reasonable. We may take a material representation of a straight line which is simply a long rod of a certain substance; the quality of the substance does not matter to our definition, nor does the thickness of the rod. In this way, by abstracting the inessentials, we arrive at a definition which even if not realisable is intelligible.*

Similarly, we can intelligibly speak of the simultaneity of phenomena separated by a distance, for, though the "agents of signalisation" have a finite velocity we can ascertain that velocity and allow accordingly for the lapse of time. Thus if an "agent of signalisation" apprises us of an event at ten seconds past noon, and if this agent has the velocity of, say, ten miles a second, we know that if the place of the event

* In this analysis it will be found that there is a point where it is evident that the mind proceeds by discontinuity. That is both essential and normal in the function of the mind. The Relativitists have never made such an analysis, or it would have saved them from employing a meaningless term, the 'continuum.'

be distant one hundred miles, then at that place the event took place at noon, and was simultaneous with an event that took place at noon at our standing place. This is conditioned by the accuracy of our measures, but so is every kind of measurement whether referring to distant places, or to the same place ; and in all cases the accuracy of record is conditioned also by the personal factor.

All this is so clear that one is astonished to find that scientific men have been stampeded by the momentum of Einstein's popular fame into abandoning their rights. That the Relativitists are desirous of destroying the value of this means of conceiving simultaneity is easily understood, for if reasonings be conducted on these lines and with avoidance of their special jargon a great part of the pretended proofs of their system falls to the ground.

The avoidance of what they call abstractions on the part of the Relativitists is not a sign of a more acute scientific sense ; it is the evidence that they have never conducted that analysis, which affords the only sure basis of reasoning of this sort, the research of the Fundamental Processes of the Mind. Abstraction, although in some cases it may appear as a difficult conception, is in a rudimentary form an essential faculty even in the simplest modes of reasoning. Generalisation, which is usually regarded as the sign of the highest exercise of the mind, and is so distinguished and rightly by Aristotle himself, is yet, in a rudimentary form, one of the Fundamental Processes, more fundamental than classification or symbolisation, to which it immediately leads, these themselves being essential and inevitable in the conduct of the simplest modes of reason. I

do not mean that symbolisation, for example, is necessarily carried on by the use of articulate language or in association with accurate definition; these are developments so late that they are not always present in the arguments of educated men; but in a rudimentary form they always exist, naturally, essentially, inevitably.*

But the Relativitists, though ostensibly avoiding these abstractions, are nevertheless tacitly or ostensibly continually employing them; otherwise, it is impossible to reason.

* Professor Julian Huxley says that the faculty of generalisation distinguishes man from the lower animals. On what grounds did he make that statement? I cannot find anywhere that he has ever undertaken the analysis necessary to form a sure opinion, and his dictum is false.

CHAPTER X

THE ETHER

RELATIVITISTS while denying the existence of ether, continually refer to it. Their only alternative is to substitute for ether a mathematical expression, but that is an abstraction which moreover has the defect of being inadequate even as an abstract representation. The story of the ether cannot be adequately told in a chapter. In fact, the Astronomer Royal of Ireland, E. T. Whittaker, has written a large volume on this subject, full of interesting facts and dealing with diverse theories of the constitution of this something which Einstein says does not exist. That it does exist is fairly well demonstrated, for it serves to make understandable a multitude of phenomena which otherwise would have no explanation that we can grasp in the present state of our knowledge. It is true that we cannot see it, nor have we succeeded in weighing it; it is known to us by its effects, just as are many other physical constituents of the universe.

The undulatory theory of light brought the ether into prominence in science, and even amongst those philosophers who have restored the corpuscular theory to a part of its former glory, like M. L. de Broglie, the undulatory theory is retained side by side with it, and so the ether is required. Fresnel, the genius whose

name is associated for ever with the undulatory theory of light, had much to say about the ether, but it puzzled him, and for certain reasons of accordance with observation and formulæ he gave it certain properties, and then revised them in view of other facts. F. Neumann in Germany worked on lines of his own, greatly influenced by electrical theory; C. Neumann, his son, developed his theories. Numbers of others followed. In England most of the physicists derive from Green, whose main paper is to be found in the *Cambridge Philosophical Transactions* of 1838.* Green conceived the ether as a continuous medium possessing, of course, elasticity and also density. He found that in this ether two kinds of waves, longitudinal and transversal, could exist; but various observations of phenomena had led physicists to conclude that light waves were transversal only.

The problem therefore with Green, as with his followers, was to endow the ether with properties that would eliminate the longitudinal waves. Green formed a theory which gave him some satisfaction by assuming that the longitudinal waves had a velocity of propagation extremely great compared to that of the transversal waves. Half a century later, William Thomson, afterwards Lord Kelvin, adopted the opposite hypothesis, that the velocity of the longitudinal waves is very small compared to that of the transversal waves.

Green represented the ether as a sort of universal jelly, almost incompressible, but I confess I cannot conceive of this substance permitting the phenomena of electrical and magnetic attractions and repulsions that

* On *The Laws of Reflexion and Refraction of Light at the Common Surface of the Non-Crystallised Media.*

are so familiar. Kelvin's ether is a kind of foam, but of very great rigidity.

Lord Rayleigh has made attempts that establish a theory on a sort of eclectic basis, but without convincing success. Then Sir Joseph Larmor, who has written in support of his own theory a volume which is interesting for its mass of information, arrived at conclusions such as that formulated in the words, which I now cite from Whittaker's volume: "Aether is represented by an incompressible elastic solid in which are two cavities corresponding to the conductors of the vibrator but filled with incompressible fluid of negligible inertia."

What I find about all these representations of the ether is that, in conceiving a substance or a structure to account for phenomena which, if not well understood, appear to us at least in a guise within the scope of our intelligence, the explanations are inscrutable mysteries. The reason may be in part that the mathematicians confine their consideration to the factors already found in simple phenomena, and apply the known models to a case, such as ether, where undiscovered forces and structural relations may exist. Moreover, beginning with Green in this country they have all constrained nature to the limits of their own mathematical powers. The mechanism invoked, whether by Lord Kelvin, or Clerk-Maxwell, or Lorentz, or Sir Joseph Larmor, or Einstein, to account for phenomena in the ether—for Einstein, though he discards it, yet, super-genius that he is, still uses it—these all seem to me, where comprehensible at all, inadequate in conception.

It is therefore a pleasure to find in a recent work by

Professor Zehnder, of the University of Bâle, on "The Ether and Meteorology," a style of writing on this subject where the conceptions, if possibly too simple, are not puerile. I quote it, not because I rely on Professor Zehnder's authority, but because he expresses, lucidly and concisely, conclusions with which I am mainly in accord.* "The reintroduction of ether into our classic physics is an absolute necessity. We need the ether to explain the propagation of light, of electricity and magnetism, across space, otherwise completely void, and already Maxwell required a vehicle for that propagation. It is inadmissible to suppress, as Einstein has done, the ether as a substance and to attribute, in return, the properties of ether to space without substance. That has almost the air of a dialectic evasion, imagined for the sole purpose of eliminating the ether without giving the slightest proof. In fact, the objections offered up to the present against the existence of the ether do not stand upright. That the aberration of light may be just as well explained with an envelope of ether surrounding our globe, as without that envelope, I have already demonstrated in the *Astronomische Nachrichten*. Reiger has been able to demonstrate experimentally that shearing forces become manifested in liquids and in gases. Moreover, every aeroplane screw shows the existence of shearing forces in gases. Does not the screw bore through the air as if it were solid? In an analogous manner we must attribute to the ether—even if it be a rarefied gas—shearing forces. Let us remember that the

* This theory is not unlike that of Carl Neumann, published in the *Mathematische Annalen*, 1869, in a paper: "Ueber die Aetherbewegung in Krystallen."

vibrations of light are carried out a million-million times more rapidly than the vibrations of the screw. It follows that henceforth the polarisation of light may be explained without difficulty by a gaseous ether. . . . The ether must then be a light, rarefied gas, existing in the free space of the world. It must be of atomic structure, and also it must possess weight, like all other substances, but it must, precisely, be much less heavy than other substances. It is only in case such an hypothesis, the most simple possible, proves to be not feasible, that it should be permitted, and even rendered necessary, that we should seek for more complicated hypotheses. In such an ether every body in movement should be subject to a resistance to that movement, a resistance that manifests itself more strongly in proportion as the body is smaller and lighter."

Professor Zehnder finds that the resistance to the large planets due to friction in the ether is insignificant but in the case of meteorites of small dimensions the effect is considerable, and one of the consequences is the production of "sun-spots," which in turn affect the temperature of the earth. We have nothing specially to do with that here, but the phenomena of light and electricity and magnetism are also necessarily varied by effects of the ether, and yet all our reasonings are carried on as if we knew everything about this ether, of which, on the other hand, we appear to know little or nothing.

Closely associated with the discussion of the ether is that of the velocity of light. This is a purely physical matter, first brought to our attention by experience and then determined more precisely by experiment and

measurement. It is not a question to be wrought out of the universe even by the profound analysis of space and time that his followers attribute to Einstein. He offers certain astonishing dicta about light, but he has no more right to impose his sayings on us than have the monks of Athos who, according to Carlyle, by gazing intently into their navels, sought to see the invisible world exposed. One of his doctrines is that there can be no greater velocity than that of light. How does he know? He does not, for in view of a little difficulty in his own theory he has, at a certain moment, discarded that idea.*

In the first place, there is no such constant as the velocity of light. There would be no refraction if the velocity were constant, and there would be no spectrum if the waves corresponding to different colours all had the same velocity. It may be said that such phenomena only occur when the light waves enter media of different density, but who can say that the whole of the ether has the same density, and especially how can Einstein, having discarded the ether altogether, use the plea?

But the constancy of the velocity of light is still claimed by the Relativitists, even when the recipient station is in motion towards the light. That point is denied by some who still play with Relativity. The common sense of Sir Arthur Eddington compels him to toss uneasily under this incubus. He does not see that if he rejects it altogether, then he has thrown out the baby with the bath water. I have returned to this

* Cf. Chapter XI., p. 140, and Chapter XV., p. 200 and p. 210, where Einstein, having been forced to 'modify' his doctrine of the invariance of the velocity of light, may find new grounds for reflexion.

point again and again ; I insist on pressing it home till the knife remains ; I “mak siccar.” Einstein does not deny it, always, for otherwise he would be deprived of the application of the transformation of Lorentz, and he would lose the harmony of the law of invariance in cases where no invariants exist. M. Bricout does not deny it, for he uses it in his demonstration of the transformation of Lorentz ; he only pities those whose methods are not refined enough to enable them to accept it ; and Professor Carmichael places it, at once boldly and a little surreptitiously, in his Postulate *R'*.

But there is more to be said on this velocity of light as representing the maximum of the speed-privileges Relativity allows us—and yet some hail Einstein as having opened out new horizons to our souls. Suppose a race of beings which had all the senses we possess but that of sight. That is even a possibility. Sound would be the principal of their “agents of signalisation.” Now retrace all the arguments of Relativity on this assumption and deal with the phenomena of sound as he does it with those of light ; then inevitably we shall be led to corresponding conclusions, and the sightless Einstein would declare with the same conviction, and the same degree of truth, there is no velocity in nature exceeding that of sound.

Further, in his latest efforts he seeks to make the law of invariance apply to gravitation. Gravitation is being considered more and more as a force subject to physical conditions, and requiring a medium for its transmission. In that case, it has a velocity of transmission, but all of those, notably E. Kogbetliantz, who have conducted researches on those lines have concluded that this velocity must be far greater than that of

light. He and the others may be talking on hazardous grounds, but when I read them I feel more in contact with reality than when I read Einstein. Be that as it may, it results that these problems are matters for determination on the lines of physical science, and there the offerings of Einstein are defective.

The actual measures of the velocity of light show a decided tendency to lessening values. In 1902 Perrotin gave $299,901 \pm 84$ km. per sec.; in 1924 Michelson found $299,802 \pm 30$ km. per sec. and in 1926, $299,796 \pm 4$ km. per sec.; in 1928 Karolus and Mittelstaedt found $299,778 \pm 20$ km. per sec. Further, M. Gheury de Bray points out that the velocity is affected by the magnetic field of the earth, and since that is variable there can be no constancy.

Another consequence of fixing the velocity of light as the maximum would be to stay the stars in their courses. As all velocities are relative they have a velocity of revolution round the earth many million times that of light, unless Einstein, like a new Joshua, keeps them in waiting.

*Shortest measure is equal
to 186,162 miles per second.*

CHAPTER XI

MASS AND VELOCITY

FOLLOWING closely on the application of the transformation of Lorentz to the cases we have seen, we find as a consequence of the theory another of those paradoxes which have astonished the world; the mass varies with the velocity.* This result of the theory is said to be verified experimentally, and is "the most remarkable discovery deduced from the theory of special relativity," as Bricout proudly states it. I certainly have no quarrel with the word 'remarkable,' but the 'discovery' intrigues me mightily.

Before looking into the alleged proof the actual nature of the mass and velocity may be considered. It is possible by a collocation of words to say that a pint of beer is changed into a perfect day; but in that case it is equally permissible to ascertain by analysis what is really meant by beer and what by day, and to show that the elements of which these two objects, or concepts, are built do not accord. Equally is it permissible to apply this method to the case of mass and velocity. Here is a test by which becomes rendered evident the lack of analytical faculty on the part of

* Lavoisier, by his experiments, arrived at the conclusion that mass was indestructible and by eliminating phlogiston he put an end to an era of false science.

the Relativitists. On the other hand, there has been available a minute and complete analysis of these concepts based on the establishment of the Fundamental Processes of the Mind, and this, if the analysis be retraced, shows that elements required in the subjective conception, or alternatively the objective presentation of mass, are not to be found in the nature of velocity, and *vice versa*. This appears to be so clear that it puts a tax on one's patience to express it in this heavy manner; but in what other way can one refute such theories obstinately retained?

When Galileo dropped his bullets of lead from the tower of Pisa—an act that might be called the virtual inauguration of the modern world—it did not occur to him that the lead and ^{the} speed with which it descended could be interchangeable terms; would his intellect have reached a higher level of genius had he made that confusion?

Certainly there are cases conceivable when in the course of phenomena material objects might lose a portion of their mass while at the same time acquiring additional velocity. This would be the case if Galileo had been able to drop his bullets through a hole in the earth reaching to the centre where the temperature became regularly increased with the depth; the lead would become melted and the central core would have less mass and the velocity would have increased. But here in the modern instances something different is meant; the actual mass in the total body becomes changed. I do not know whether any one of these philosophers has ever stopped to think what is actually implied. Possibly not, for the manner of reasoning employed does not require thought on these lines; it

requires simply the application of the transformation of Lorentz and the acceptance of whatever is required by the theory of Relativity.

To thinkers who have confused time and space and regarded them as of the same category, if not interchangeable, anything is feasible ; but the consequences of this transcendental thinking are more remarkable than they have supposed. For velocity is composed of relations between time and space, and since, as they claim, one may be expressed in terms of the other it may be taken as composed of time or, alternatively, of space. But velocity and mass are interchangeable, therefore mass may be composed of time, or, alternatively, of space. If mass be expressible by time alone, it acquires a fleeting character which seems to allow the material world to dissolve under our feet ; but if it be expressible by space alone our situation is worse, for space, according to the Relativitists, has no "*point de repère*" (registering point or datum point) ; it is so empty that we cannot seize upon any *point de repère* to measure the velocity of light or to fix its position ; it is void, absolutely, what we call void ; and so therefore is mass ! When Tom Moore sang in mournful regret :

"This world is but a fleeting show
For man's illusion given,"

he did not know what a great seer he was ; though even he with his kindly temperament was clement, for he does not dispose of the show by one fell swoop of the Lorentz transformation. Here, it may be objected, I am again talking nonsense. That too is possible, but I am talking within the data of Relativity and with arguments which, starting from their premises, are more rigorous

than those with which they supply us. The nonsense is theirs.

The mechanical proof of this transcendental theory is obtained from an experiment of Bucherer in 1908. The details may be read in the *Annalen der Physik*, Band 28, 513, of the year 1909. They are a little too technical and they would demand too much space and time to be reproduced here. The principle is this : We want to get a free electron, or electrons, in such a way that we can submit it to the action of a magnetic field, so as to modify its velocity. To do this we arrange to have the electrons free only to move in a certain direction at first. This is arranged by obtaining our electrons from a salt of radium placed between two flat-faced armatures of a condenser, kept at a constant difference of potential, associated with a long solenoid giving a magnetic field parallel to the armatures. The electrons which escape are such that the resultant action of the two fields does not affect their velocity, which is parallel to the armatures. On escaping they come under the influence of the magnetic field only, and, their course being deviated, a curve is produced which yields us data for calculating the relation between the fraction e/m and the velocity of the electron, e being the charge and m the mass.

From the results of this experiment we find that there is a 'transversal' mass which increases with the velocity, and a 'longitudinal' mass which decreases with the velocity.

In criticising this extraordinary theory and the experiments supposed to verify it, there are a great many considerations to be taken into account that are tacitly ignored, especially when in place of the actual

mechanism we think of diagrammatic representations. The conditions of the experiment are always simplified in their expression in mathematical forms. Just as men carve God in their own image, so mathematicians constrain problems to the limits of their own knowledge and skill. Each in turn defines the conditions to fit in with the scope of his powers and give a conditioned solution accordingly. And so it is in this case of the experiment of Bucherer.

In the first place, it is not possible to get a uniform electrical field in the manner employed, and it is not possible to provide by a long solenoid a uniform magnetic field, and it is still more difficult to cause these fields to furnish a resultant of these two fields so that the resultant action on the electron shall be null. It is not true, as claimed, that the only electrons that can leave the condenser are those whose initial velocity is parallel to the armatures. Next, it is assumed, without warrant, that the charge in the electron is constant although electrons may differ in mass and in velocity. And, although electric and magnetic forces are not considered to possess mass, they are taken to vary the mass while leaving the charge, which is electrical and subject to magnetic influences, unchanged.*

Finally, in the interpretation it should be noticed that concordant results might be obtained while using the metric of Lorentz, without carrying the implication that other applications of this metric were justified. For example, a metre and a yard are not equivalent, but the results of an experiment estimated in metres

* G. I. Pokrowski (*Z. Physik.*, 58, 1929) as the result of experimental tests says that electric charges vary with velocity. Most physicists, including Sir Arthur Eddington in his *Mathematical Theory of Relativity*, assert the contrary, but that is mere assumption.

would be concordant with those obtained when the yard was employed as the unit. It would be improper to deduce from this that the yard and the metre were equivalent, or that a calculation which seemed to show it was justified because of the result of an experiment. When all these considerations are weighed it will be seen how precarious is the ground of claiming this confirmation of the theories which we have examined so carefully, and of which we unearthed the origin of the error.

M. J. Le Roux, whose investigations of certain doctrines of Relativity have served to disintegrate the whole structure, attacks one part of this problem of changing mass in a note published by the Académie des Sciences (*Comptes rendus*, tome 180, 18 Mai, 1925). He shows there that "in particular every movement representable by aid of a constant relativist force and of a relativist mass, function of the velocity, is capable of being represented also by the consideration of a constant Newtonian mass and of a force, function of the velocity."

By 'Newtonian' mass he means mass understood according to the usual definition. The tenor of his criticism is to state in another way what I have already indicated—that the interpretation of the results of such experiments, themselves ill-defined and uncertain, may be made to depend on the conditions, such as variation of force rather than variation of mass. Force is defined by reference to mass, and conversely mass might be defined by means of force; but the conception of mass is more clear and more fundamental than that of force.

I do not quote M. Le Roux for his authority, but for

his arguments, which I adopt as being lucid, intelligible and correct, cogently leading from agreed premises. In default of space I refer to the original, but his method of reasoning may be indicated. He defines force according to the Galilean or Newtonian conception, such as we have already seen. Then he supposes that by other measures, whether true or false, this expression is turned into another, using the metric of Lorentz, which serves them to define force according to this system. It is possible to work out consistently a mathematical argument on this basis ; the gauges of measurement of force are different, that is all ; and we may have consistently a transformation again with the first form.

That mode of envisaging this metric accounts in part for the acceptance by some mathematicians of the Relativist doctrine, for they are concerned only with the rightness of mathematical procedure ; but they are wrong in taking for granted that every argument involving this metric is right in itself.

This may be illustrated also by the continual use of a form which is subject to caution ; that which represents energy as $\frac{1}{2}mv^2$.

In the old days of Euler and other great mathematicians, the form for representing energy was the subject of passionate discussions, for some claimed as the proper expression mv . Then d'Alembert offered an opinion which virtually concluded the dispute. He said that either form could be used, since results obtained consistently on the one basis could be transformed into results shown on the other. Anything measurable and capable of being applied to measure may be used as a unit ; the choice of the best unit is a

matter of convenience. Now the form $\frac{1}{2}mv^2$ corresponds to the expression, mgh , where h is the height to which a body possessing an initial virtual velocity v would be carried in our ordinary gravitational field ; and a convenient and intelligible standard of work was given by the height to which any body in question was raised.

In all that discussion it was understood of course that the conditions were such as prevailed on the earth's surface, of a gravitational force which remained constant in any one place, for all practical purposes, and varied little when the place was varied.

But in dealing with the movements of electrons in fields of force which are not constant, and are in some experiments rapidly changing, the use of this form for energy is unjustified ; and in the calculations actually set forth to verify experimentally the application of the Lorentz transformation, error is produced by this misuse of the form $\frac{1}{2}mv^2$ for energy, by reason of its being held to be equivalent to other forms for energy depending for their justification on the constancy of the field.

All this, however, even though it be in itself of interest, does not touch the gist of the problem of mass. Mass may be defined by means of force, and M. Le Roux himself admits for argument the definition so derived ; but that only throws back the problem by demanding a definition of force. We have found the conception of force more elusive, and have accepted that of mass as being much more immediately intelligible and fundamental, even in the way of reasoning of ordinary common sense ; and it is on the basis of the meaning of mass, as resolved into its ultimate

constituents, that I say that Einstein's conception is false.

The rest of the discussion has been undertaken in order to trace out the entrance of that falsity in his own reasoning.

CHAPTER XII

RIEMANN'S METRIC

AMONGST the works of Bernhard Riemann—who is one of those who might be called “mathematicians’ mathematicians”—is a treatise of great note in the history of Relativity. It is called *On the Hypotheses which serve as the foundation of Geometry*. It was read by Riemann on 10th June, 1854, as a paper on the occasion of his candidature for admission to the Philosophical Faculty of Göttingen. The paper itself is philosophic in vein and at the same time important, but it is written in a style of high generality, without sufficient illustration to indicate what precisely was in the author’s mind, perfectly clear to him for the most part, but not always clearly conveyed to his audience. This is not merely my own impression, for his commentators have found it necessary to throw light on his meaning by citing other papers of Riemann written on allied subjects on later occasions. The treatise excited the admiration of Gauss, but that was natural for he quotes Gauss more than once, and his manner of thought and his view of the import of his speculations were derived from Gauss.

The paper begins with some psychological views vaguely expressed, which, had I found it needful, I might well have cited in support of my insistence on the necessity for searching to the very depths to reach

the origins of our mathematical conceptions and the sanctions of our modes of combining them. More than once Riemann refers to the necessity of such analysis in order to find the fertile source of mathematical development along the true lines. Acute and original, however, as is this treatise, and stimulating and illuminating as are all Riemann's writings, I say definitely that he himself had no grasp of that very mode of analysis—really the research, though he does not quite perceive it, of the Fundamental Processes—which he sees to be necessary. He admits indeed that he was deficient on this side of his equipment.

He seeks at once to emancipate himself from the restrictions of the Cartesian coordinates by searching for some more general expressions. The line of thought here is comparable to that of Sophus Lie : the conception of 'multiplicities' of n dimensions such that our ordinary space is a 'multiplicity' of three dimensions, and the coordinates of Descartes a special case of a more general theory. It must be noted here distinctly that Riemann treats the space we know, and which to us is the sole knowable, as being of three dimensions ; he does not argue that point, he takes it for granted.

That being so his development of the theory of a 'space'—this term being here only used by analogy—of n dimensions is purely algebraical, and only theoretically conceptual ; that is to say not applicable to our familiar space. Except in the language employed I find all this in accord with Sophus Lie's ideas.

An analysis of the whole of Riemann's treatise would be too voluminous, and I am afraid to most, very tedious. I will, however, cite a passage on which a certain part of the theory of Relativity is based.

The French translation by L. Laugel is probably the most accessible of the books containing this treatise, and the passage to which reference is made will be found on page 295 of that volume.

Riemann has spoken at length of the distinction to be made between relations of 'extension,' or definite portions of space, and the metrical relations concerning them. Then he says: "When one extends the constructions of space to the immeasurably great, it is necessary to make the distinction between the limitless and the infinite; the first applies to relations of extension (space), the second to the metrical relations. That space is a limitless variety* of three dimensions, that is an hypothesis which applies to all our conceptions of the external world, which enables us at every moment to complete the domain of our effective perceptions and to construct the possible positions of any object sought for, and which is always found verified in all its applications. The property of space to be limitless possesses therefore a greater empirical certitude than any other assumed fact of our experience of external objects.

"But the infinity of space is not at all a consequence of this; on the contrary, if we suppose the bodies in space to be independent of their position, and in accordance with this we attribute to space a constant curvature, the space will be necessarily finite, if the measure of the curvature has a positive value, no matter how small it may be. By producing, along the lines of shortest distance the initial direction situated on an element of surface, one would obtain a limitless surface of constant

* Riemann uses this term in a special sense; it is closely enough synonymous with category.

curvature, that is to say a surface which, in a plane variety of three dimensions, would take the form of a spherical surface, and would consequently be finite.”

If the reader fails to make out a clear meaning in Riemann's words, here quoted, I shall not be disappointed; on the contrary I shall find it the easier to claim some indulgence for any of my own passages of obscurity. As to the veritable import of this famous paragraph I confess that when I read it first I did not understand it, and when I read it a second time I did not understand it. Again and again I returned to the study of these words, but not even under the force of concentrated attention, nor again in the more enticing form of solicitation of ideas in which the imagination has its play, could I induce the arguments to march before me, distinct, consecutive, cogent.

I have come to the conclusion, for which I will give reasons, that Riemann himself did not here have a perfectly illuminated conception of what he attempted to demonstrate. It is the most dubious part of his whole writings, but—just as the Schoolmen with Aristotle, so the Einsteinists with Riemann—they have seized with peculiar avidity on this feeble structure to make it one of their foundations. There are mathematicians and mathematicians. Those who gain fame in their lifetime are the ‘solutionists,’ that is to say, those who, without adding anything to our actual knowledge of mathematics, and without sending the plummet of their researches deep into the foundations, yet display admirable skill and deftness in the employment of what is already known. Such have been Daniel Bernoulli, Schläfli, and, in another sphere, Le Verrier, Adams, Clerk-Maxwell and Lord Kelvin. Hesse shines by

elegance, Painlevé by the deeply penetrating insight, while others, Hermite, Picard, Klein and Darboux, add to vast knowledge a stimulating manner of exposition. Then there are mathematicians who look into the finest inner working of the machine and suggest new modes, new developments; such was Descartes himself, Euler, Lagrange, Monge, Gauss, Cauchy, Steiner, von Staudt, Poncelet, Plücker, Niels Abel, Jacobi, Evariste Galois, Hamilton, Weierstrass, Sophus Lie; and to this category belongs Riemann. Therefore everything that he says demands devout attention. But in the development of science, surer guides than the workings even of a mind of genius are the workings of nature; and as I find that Riemann has never entered deeply into that domain of analysis in which the fruit of the Fundamental Processes may be gathered, I say here that the conclusions he has arrived at are not faulty, but rather, that they have nothing but a conventional meaning. The strain of thought that led him to such expression may be broadly traced. The curvature of surfaces had not been dealt with as a special study till Gauss took it in hand, and produced a formula for the measurement of the curvature of a defined surface at any point. Then subsequently different forms of curvature were studied by various mathematicians. The plane is, in this regard, a limiting case between the figures where the curvature at a point is convex, or in the other concave. Imagine, passing through a point, a sphere or a saddle-back surface.*

* The change of sign of a parameter, or index, may transform from one form of curvature to another. Riemann's curvature is denoted by a positive form of a mathematical expression. Lobatchewski's or Beltrami's by a negative form.

Following Gauss the works of mathematicians were haunted by curvatures, and the tendency has always existed to bring in forms of learning which have obsessed the mind into the consideration of problems of a much simpler character. But note that when a clear conception of curvature was held, it was curvature of a surface. Up to that point it would have appeared as absurd to speak of curvature of space, as to ask the question: Is this blackboard a blackboard of straight lines and triangles, or of circles and ellipses? The answer would be that the blackboard itself knew nothing of these categories, but that the figures of triangles, or the figures of ellipses could be inscribed on it. So with space. Space is not in itself plane, or Riemannian, or Beltramian, but within the compass of space any of these forms of surface may be constructed or imagined.

Even when finally Riemann speaks of curvature of space he has some form of surface in mind: Now where is that surface? He seems to conceive it as a surface bounding our space; but if that surface be unique, by what law does he determine that it shall be say at a certain distance, however great, from us? Where is the empirical certitude in this? I for one have no certitude of any kind, and Riemann gives no sort of assistance in discovering where this surface is. If the surface be not unique then there is no reason, certainly none in our empirical certitudes, why certain portions of space should be favoured, and so once more we are brought back to the onion-like layer on layer of surfaces; * but this conception is not in accord with his argument.

* Cf. p. 39.

Then once again, what is the meaning of curvature of something immaterial? We see that an actual palpable sphere, or a saddle back, has curvature; and even if the material substances were absent we could delimit out of empty space the positions of these surfaces; and of course we could also delimit any kind of surface of which we could think.

If by curvature he means that at the places where we come in contact with his surface there is a certain force, or constraint of any kind, impelling material things in certain directions, then we are dealing with physical phenomena, such as in local conditions electricity or magnetism might be; but Riemann has made no attempt to show that such influences exist. Even he finds it necessary to account for the curvature in any case, for he says, "if the measure of curvature has a positive value no matter how small it may be." That proviso reminds me of Voltaire's comment on St. Denis walking a mile with his head under his arm—it is only the first step that costs. But suppose there was no positive value, but simply zero, what then?

We have come to a point where we are not talking science, we are not talking mathematics, we are not talking any sort of sense, we are simply talking as the Schoolmen did in the Dark Ages.

Having got so far I felt that somewhere in the history of this matter I should find the conquering mind of Kant, conquering because he has left so many dead brains on his march. Following upon Riemann's papers appeared a series of discussions carried on even with a certain heat; some defending Riemann, others criticising Riemann adversely; but all appealing to the authority of Kant. Herr W. Tobias wrote at great

length on *The Boundaries of Philosophy indicated in opposition to Riemann and Helmholtz*.

The actual conclusions of this book are those that I accept ; but I only cite it as an instance of what we shall meet with frequently, that an argument may terminate at a just conclusion, and yet be valueless in itself. Herr B. Erdmann in his *The Axioms of Geometry* attacks Tobias in turn ; and appeals to Kant.

But once more, neither in Tobias, nor in Erdmann, nor in Riemann, nor in Kant, do I find anything but mere adumbrations of the only kind of analysis by which the question could be effectively treated ; the analysis that traces the psychological operations finally to the Fundamental Processes ; and then the observation, from these as a basis, of the synthetic forms by which we acquire the notion of Externality.*

I feel that here I have accomplished a good social work, for, as I am reminded that Froude used to make a practice of inviting sleep by cogitating on the Kantian world outside our present powers of consciousness ; so I can imagine the insomnia of some honest bourgeois, earnest if not specially intellectual, trying to imagine a world that is limitless but finite. By way of an amelioration of the situation one can say that by 'limitless' Riemann seems only to mean, not blocked by some inevitable and insurmountable barrier. On

* This investigation of Externality is very elaborate, for, to give us at length the sense of objectivity that is familiar to us, a great number of factors, of which we should form a just comprehension, act in a coordinated manner. The exposition will show that in the nature of things there is a complete reconciliation between the Idealism pursued determinedly, and with avoidance of Berkeley's erroneous confusions of ideas, and Common Sense, if that term be understood, on the other hand, without Reid's obscure perceptions and limitations. I have dealt with this minutely in *Principles of Psychology*.

this sphere of his to which he has assigned his hypothetical curvature an imaginary sentient creature might crawl for ever without coming to the end of things.

This contribution of Riemann's to the theory of Relativity is, however, small compared to what he has offered by his famous 'metric'; the "metric of Riemann" is heard throughout the higher circles of Relativity like the *leit-motif* of a Wagnerian opera, but only in the higher circles, and there no others than the élite who are versed in mathematics are allowed to enter.*

The coordinates of Descartes form a metric system; by their use we have measures, finally expressible in numbers of units, that indicate the position of points, the length of lines, and so forth. Now Riemann's system is a formal extension of that of Descartes; but as Riemann himself states the actual space we know requires no more than three dimensions, what is the meaning of Riemann's series of dimensions?

They have a meaning if regarded as algebraic terms, but if they be measures of space relations then they may be reduced ultimately to three; that is to say, they are not independent. If they be independent, as they are usually considered, then they cannot be defined in terms of space relations. This must be made quite clear. If there be only one variable to be defined in an algebraic expression, one equation is sufficient; $x = a$, for example, is the simplest case. If there be two or

* Even Bernard Shaw is definitely excluded from the ranks of the inner saints round the throne. Perhaps it is as well. If he were admitted, his cheerful Irish audacity might lead him to say that he understood the "metric of Riemann," to the detriment of his reputation; or he might be tempted to examine it, seriously, and then what about the Applecart?

more equations than these are either mutually equivalent, and therefore not independent, or there is a redundancy in the expression that does not admit of a determination. If there be two variables, then two independent equations are required, and so on.

If, however, there be two variables, and only one equation, then in an ordinary way a mathematician would say that a determination of both variables is not possible; and if another comes forward and declares that he has found a solution that gives a clear determination of the two variables, he is not in this case regarded as a super-intellect but merely as one who talks nonsense on algebra. Simple as this may be, I beg that it be continually kept in mind when in place of algebra we say Relativity.

As the metric of Riemann does not deal with our space it is, of course, only by a convention that one uses, in referring to its forms, the usual terms applicable to our space—lines, planes, volumes, and so forth—and this convention is quite allowable, as we have noted in the language employed by Sophus Lie. But the Relativitists insist that the Riemannian system does refer to a space. What space? A Kantian transcendental space. But that is but the beginning of the wonders they ascribe to the “metric of Riemann.”

When the man of science of early days was confronted with the problem of falling bodies, he first of all sought to ascertain whether there was such a force as we now call gravity, and then he made attempts to ascertain the law of its operation. Gravity was known to the Greeks of the days of Pythagoras, and afterwards was so familiar to Galileo that he made experiments to discover the law. Subsequently Kepler, who had

prepared the way for Newton, also endeavoured to formulate the law, and his vision was so clear that he failed only because the data on which he worked was incorrect. That same defect also delayed the success of Newton, who having ascertained the law in the few cases tested, jumped to a conclusion which, however entrancing, was at that time a daring assumption, that gravity was universal and its law invariable.

Newton was not quite content with the assumption of action at a distance in respect of gravity, but he offered no sort of suggestion in place of it. Various philosophers have devised explanations to account for the working of gravity, and amongst these Clerk-Maxwell, whose constructions, however, have the air of being tentative and not at all convincing, especially as they elude demonstration. At present the problem is being zealously studied by ardent thinkers, and E. Kogbetliantz, amongst others, believes that the undulatory theory as applied to the ether affords the true explanation, the velocity of the waves being far greater than that of light.

So far we have been dealing with men of science who have had a conception of an objective reality, and who, by the appeal to Nature, have endeavoured to ascertain the veritable sense of that reality. With the Einsteinists we find nothing of the sort. It is true that Einstein himself has imagined a mechanism which he only vaguely indicates, and which does not seem to have even greatly interested himself. He has other resources—the fantasias that can be played on the “metric of Riemann.” Remember always that this metric was never conceived as having any relation to physical forces; it is a purely formal, algebraic

conception. But side by side with this metric there has become developed a branch of mathematics, already mentioned as due principally to Ricci and Levi-Civita, the tensor calculus. The tensor calculus combined with the metric of Riemann provides us with many of those complicated expressions which are the delight of mathematicians.* Einstein 'chooses' one of these expressions, and after various exercises of trimming down the generality of the expression to suit his needs, he 'fiddles'—I can find no better term—with the demonstration till at last he produces what he calls the Relativist law of gravitation. During the whole of the discussion there is not the slightest reference to gravitation, to the manner of its operation, to anything of the external world at all. How then does he finally arrive at a result which he can plausibly put forward as a law of gravitation?

For one thing he knows the result at which he is aiming. That has been ascertained by Newton in the only space, according to Riemann, of which we can have cognizance. The certitude of that law has been tested tens of thousands of times by experiment, and by astronomical observations, so that, as Huxley remarks, not a star crosses the line of observation of our telescope but certifies to the exactitude of the law and the wonderful precision of our instruments. What then has Einstein done new in this domain? Nothing in this domain, but something in an unknown, transcendental domain. That is to say his law agrees with that of Newton in the universe, as we can know

* This part of the discussion will be elaborated later with indication, as full as necessary, of the mathematical terms and the course of the argument.

it, but differs from Newton's at points inaccessible to our observations. He can always claim that no test will prove him wrong, for his domain is that which lies beyond the limits of testing.

When Newton offers the demonstrations of his system, or when Laplace exhibits the working of Newton's law, with admirable completeness, in the solar system, or Poincaré by newer methods of calculation of greater precision, refines upon Laplace's results, they are all using measures, and applications of measures, which we can appreciate in their own character, and of which we can examine the mode of employment. But if the Cartesian coordinates be ample for mathematical determinations in our universe, what meaning can be given to the parameters, or coordinates, of Riemann, unless we have the means of transforming them into Cartesian coordinates? Nothing of the sort is attempted.

When I first met with this mode of juggling with mathematics on the part of one of Einstein's followers, Schwarzschild, I was not a little disconcerted; and on reflexion I put the inquiry: Since it is not necessary even to look into the physical conditions in order to solve physical problems in this way, why not dispense with study of physics in all other matters? Why not, for instance, solve in this way the problem of the movement to the red of the Fraunhofer lines obtained from solar spectra?

Zu meinem Erstaunen! (To my astonishment!)—the words of John Bernoulli on a certain occasion jumped to my mind. That is what the Einsteinists have done!

Ask Einstein to account for the 'irregularity' in the movement in the planet Mercury; does he consider

what Mercury is, what is its 'official' orbit, and what are the forces that act on Mercury to produce either the expected orbit or the divergences noted? Nothing of the sort. He simply 'chooses' an expression of the metric of Riemann; and modifies this expression by means of a series of processes that have no sanction either in the physical conditions appertaining to Mercury, or in the true meaning of mathematics; then he says: There you are! . . . We will consider this later in regard to the actual mathematics of his gravitation doctrine.

Yet even if all this be brought to evidence, I do not engage myself to convince the congregation of the Einstein cult, for there are opinions on which the shafts of reason fall blunted or broken. Sir Arthur Eddington, one of the keenest and ablest of those who have written on this subject, seems to look up from his mathematics at times, as if to ask: Is not this sheer humbug? * He questions the meaning of Einstein 'choosing' certain forms for his ultra-mathematical manipulations, and he appears even to have questioned the master personally in regard to this procedure. Having got so far he must needs have been convinced of the untenable style of the reasoning. But there it is!—the influence stronger than reason; the acceptance of the creed of a cult; and so Sir Arthur reconciles him-

* The celebrated astronomer, Le Verrier, once showed to Wilfred de Fonvielle, who told me the story, the great mass of his *cahiers* (memoranda books) which contained the calculations that led to the discovery of the planet Neptune. He gazed for some time on these repositories of his genius and his patience, then suddenly remarked: "*Si tout cela n'était que de blague!*" (What if all that were not mere humbug.) I have often meditated on the saying. I think it was a philosophic reflexion, in Jacobi's vein, of the perilous nature of a long series of deductions where any step may have lacked the necessary rigour. In any case Le Verrier—so M. de Fonvielle assured me—burnt his books, though—as M. Escanglon has assured me—the calculations are preserved.

self to talking orthodox Relativity in the same vein as Einstein, though in regard to the demonstration of the laws of gravitation he is content to give, without comment, the clever and outrageous demonstration of Schwarzschild. Add to this the influence also that affects the less intelligent, the desire to be in the majority lobby, for even if wrong they will have mutual support; and the small still voice of reason is drowned amid the noise of popular slogans.* We will return to the mathematical demonstrations when the ground has been still further cleared.

* It is quite advisedly that I use these political terms, and I will point my ideas here by a parable. Once returning from the House of Commons by Tube, I had as companion a distinguished statesman. He noticed that I had two volumes under my arm, and when he saw that they were *Psychology: A New System*, he became greatly interested. He asked to look at them, and he did, attentively. That is to say he studied the binding, the title page, and the general appearance. Then he weighed them, with gravity. More than once he balanced them in his hand, and every time he muttered, turning to me, as he beamed forth his appreciation: "A monumental work! A monumental work!" Then he gave a look of doubt. "I wonder is that linotyped! I'm not sure! Pity! If it is, it detracts much from the value of your book. H'm'm. . . . By the way do you follow Kant?"

"I follow no one. This is an original work."

"Ah!" And with this he handed me back the two volumes, remarking, "I was brought up at the University on Kant. I am a Kantian. Must stick to your Party, you know!" And again he beamed, if not forgiveness, at least smiling indulgence.

I laughed inwardly; but on subsequent reflexion I saw in this little incident the whole mass of the obstruction that any true saying may meet with.

Was this man a born fool? On the contrary he was an exceptionally intelligent statesman; he is the only one I have ever known to be interested at all in psychology, except Arthur Balfour and Haldane, whose philosophy I distrusted from the ground up. Moreover though he omitted to take any notice of the only part of a book on psychology that should have interest—the "germinal idea" and the trend of the reasoning—he had with remarkable judgment covered every point which would enable him to stand up in a philosophic House of Commons, to deliver his opinion, and to be sure of going into the biggest lobby.

CHAPTER XIII

RELATIVITIST SPACE AND TIME

THE third section of Einstein *Grundlage* * deals with the *Raumzeit-Kontinuum* (the space-time-continuum), and the necessity for a general covariance for the equations that express general laws of nature.

Einstein first undertakes to show that in the measurement of natural phenomena different results are obtained when different coordinate systems are employed. The methods hitherto employed of adopting definite coordinate systems for the space-time continuum, he tells us, are faulty ; and no other way can be found by which coordinate systems can be applied to the four-dimensioned world so as to produce concordant formulations of natural laws. Therefore there is nothing left but to take no one of all possible coordinate systems as having any right to preference. This, he concludes, leads us to the necessary conclusion (and here he uses italics) :

The general laws of nature should be expressed by equations, which are true for all systems of coordinates ; that is to say, are covariant (covariant in the general expression) in regard to all substitutions.

He says that it is clear that a theory of physics which satisfies this postulate would be in accord with the postulate of general Relativity.

* *Annalen der Physik*, Band 49, Leipzig 1916.

Here confusion is piled on confusion. It is not disconcerting that the expression of a natural law may be different in two different systems : but note that the statement refers entirely to the expression. The natural law should not be understood as involving any particular terms we take to describe it.

Note also that there is a covariance, in fact an invariance, when the natural phenomena are not changed ; but the meaning either of covariance, or of invariance, that recognises such a fact, is not what is meant by covariance in mathematical expression ; and it is this last that Einstein searches for. He is not thereby a greater philosopher, for he has to bend both common sense and science out of their proper meaning to attain his result.*

But, even though it involves repetitions, let us get to close quarters in time and space. I find Sir Arthur Eddington more informative than Einstein. In his interesting *Mathematical Theory of Relativity* Sir Arthur opens his Introduction by these remarkable words : “ Consider, for example, length or distance between two points. It is a numerical quantity associated with the two points ; we all know the procedure followed in practice in assigning this numerical quantity to two points in nature. A definition of distance may be obtained by stating the exact procedure ; that clearly must be the primary condition if we are to make sure of using the word in the sense familiar to everybody. The pure mathematician proceeds differently ; he defines distance as the attribute of the two points which obeys certain laws—the axioms of the geometry which

* The reader is here invited to turn to the remark of M. Painlevé on this matter which is crucial in regard to “ generalised Relativity.” Cf. p. 123.

he happens to have chosen—and he is not concerned with the question how this ‘distance’ would exhibit itself in practical observation. So far as his own investigations are concerned, he takes care to use the word self-consistently; but it does not necessarily denote the thing which the rest of mankind are accustomed to recognise as the distance of the two points.”

I find here a clear issue as between Sir Arthur’s psychology and my own as applied to this limited question. In the first place, however, I would point out that from his angle he expresses the character of the ‘pure’ mathematician in substantial accord with what I have already said; and M. Bouasse has in his piquant style remarked that a mathematician may reason with great acumen in a restricted domain, while possessing no opinion of value on the main theory within which his exposition is contained. That being noted, now to the immediate question.

In view of what I submit to be the true psychology, the appreciation of distance is in the first place due to the direct, immediate, impression of space, particularised in regard to the two points. This stands by itself as a fundamental condition of our perception of the external world. There are diverse forms of measurement of space, varying in degrees of exactitude, and none absolutely exact; but when we arrive at that stage we have already advanced several grades in the combination of those Fundamental Processes by which we gain knowledge of the world, and of which the immediate recognition of space is a special process, conditioned by the constitution of the world and the constitution of man’s intelligence. This may be called

the qualitative recognition as apart from quantitative.* When the qualitative characters are well recognised we proceed to quantitative determinations.

Let us disengage our minds, for a moment, by considering other examples. Thus, having discovered that water is composed of oxygen and hydrogen, we find by a different series of tests that the proportion by volume is two of hydrogen to one of oxygen. But even then though we have acquired a certain new knowledge of facts that may further serve to build up our scientific theory, we have not altered our recognition of the qualitative characteristics. Dalton, the pioneer of the atomic theory, for example, made out a list of atomic weights, and these were afterwards checked and altered. That was a useful work of science, but again it did not alter the qualitative appreciation. It is necessary to insist on this, for the same problem, or rather a problem having its roots deep in the foundations of our psychology, presents itself in regard to time, and a similar confusion is evident all through the work, not only of Eddington, but of Einstein, of Minkowski, and all the Relativitists.

Sir Arthur further says : " To find out any physical quantity we perform certain practical operations followed by calculations; the operations are called experiments or observations according as the conditions are more or less closely under our control. The physical quantity so discovered is primarily the result of the operations and calculations ; it is, so to speak, a *manufactured article*—manufactured by our operations."

Here again there is confusion. When water was split into oxygen and hydrogen, these were not

* Cf. the note on Lord Kelvin's assertion, p. 9.

manufactured by the experiment ; they were not in themselves the result of our experiment. That which resulted from the experiment was a change of relations in their association by virtue of which we were able to recognise them separately. When Davy dissociated potassium and sulphur he knew vaguely what he was searching for, but he had no idea of manufacturing either of their elements. Let us recall the words of Bain, one of the clearest-minded of all the British thinkers. He speaks of Davy hunting for the secrets of nature in the very thickets of concealment. In Bain's view there was no manufacture, but the discovery of something concealed. There is an analogy between such a discovery and that of a photographer who shifts his apparatus to get a better view of a horse partly concealed by another. Does the photographer manufacture the horse ?

Now let us apply the lights so obtained to the investigation of this question of 'distance.' Here the matter is clearer, for the Immediate Presentation * of that relation which we call distance, is in simple cases inevitable ; and in cases where we make appeal to imagination, or to some constructive faculty of the mind, it is finally reducible to components of which the Immediate Presentation is inevitable. Following upon this, we may render our conception more precise by means of measurements. All this is so natural in an ordinary way that any intelligent person has a clearer comprehension of distance and length than the Relativitists have of what they call 'interval.' But they

* I have used this term on account both of its exactitude and of its wider scope of application, in preference to sensation, or complex of sensation.

say : That is all very well for ordinary purposes ; but to lay the basis of a new science, or a new branch of science, we must analyse our processes and, where possible, find a general expression in which are included the particular forms that we discover.

With this desire I am in agreement, and the more so that, where it is itself expressed in the most general way, and where the processes of analysis are carried out to their limit, we get a suggestion which underlies the whole system of my *Principles of Psychology* ; but we should insist on the proviso that the analysis, in order to form a system, must be complete, definite, and of all-embracing scope ; in other words, that we must arrive at the Fundamental Processes of the Mind, these being such that by the syntheses of these Processes we may obtain all forms of thought.

The Relativitists, Einstein, Minkowski, Eddington, or others have never made such an analysis ; they have never even contemplated such an analysis ; and it is the gravamen of my reproach to them that they have failed to see that such a system of analysis is essential to the understanding of the questions with which they deal, and that by ignoring this fact and giving tentative and faulty modes of analysis they are led into errors at every turn, and finally into maintaining absurd conclusions.

Sir Arthur continues : “ But the physicist is not generally content to believe that the quantity he arrives at is something whose nature is inseparable from the kind of operations which led to it ; he has an idea that if he could become a god contemplating the external world, he would see his manufactured physical quantity forming a distinct feature of the picture.”

I would say here that the physicist is right, but in order to avoid what in logic is called the fallacy of many questions, I desire to make the matter clearer. A god would have the Immediate Presentation that we designate by the term 'distance,' but he might not be concerned with any determinate measurement, though he would, when that determination was called for, see that there were many methods of measurement; and where, for example, Sir Arthur's method was employed, the god would have a clear understanding of what it meant, and also the apprehension that it was something superadded and not to be confused with the Immediate Presentation of distance.

Sir Arthur continues: "By finding that he can lay x unit-measuring rods in a line between two points, he has manufactured the quantity x which he calls the distance between the points; but he believes that that distance x is something already existing in the picture of the world—a gulf which would be apprehended by a superior intelligence as existing in itself without reference to the notion of operations with measuring-rods."

This last passage, except for the unfortunate use of the word 'manufactured,' is fairly in accord with what I have stated. But suppose that we could show, in successive steps and by appropriate syntheses of sensations and ideas, what the god sees at once, we would have in this limited field an analysis of Externality. That analysis I have laboriously and elaborately carried out elsewhere.* It involves a consideration of a vast complex of objective and subjective factors, physical, physiological, and characteristically mental. That exercise of analysis is fascinating in itself, and

* Cf. *Principles of Psychology*.

essential to the understanding of certain essential problems ; but the Relativitists have never made even a beginning of such an analysis.

Sir Arthur has hitherto offered to us, as necessary, in regard to our conception of distance, a set of operations which require an extensive series of processes for their realisation ; but on page 9, after remarking that “ in our common outlook the position or *location* seems to be fundamental,” he proceeds to combat this assumption, and states : “ The view which we are going to adopt reverses this. Extension (distance, interval) is now fundamental and the location of an object is a computational result summarising the physical fact that it is at certain intervals from the other objects in the world.”

Here again, I submit, there is a confusion, even though he has reached the point in his reasoning when he finds it necessary to assume extension (distance, interval) as fundamental, for he introduces his measurements—though these are not fundamental but secondary, and relatively of great complication—to define position. He says : “ Our ultimate analysis of space leads us not to a ‘ here ’ and a ‘ there,’ but to an extension such as that which relates ‘ here and there.’ ”

To this I reply again, by the simple application of the principles of the Fundamental Processes, that the recognition of a ‘ here ’ and a ‘ there,’ whenever we have a conception of that sort at all, is an Immediate Presentation ; that is to say, one of the Fundamental Processes.*

* It must be observed that in dealing with the Fundamental Processes, and in proceeding to any application of the system, we are concerned with ‘ momentary ’ conditions, though, as the state of affairs may change from moment to moment, the operations of the Fundamental Processes follow in accordance.

That is also evident from the statements at which, in his own tentative efforts, he arrives, for how is it possible to obtain, or conceive of, the distance that relates a ' here ' to a ' there ' unless there is a conception of a ' here ' and a ' there ' to be related ? Even the obscurest metaphysician must know what he is searching for, if he is to produce a rabbit, no matter how tenuous, from a hat, no matter how transcendental.

All this becomes crystal clear, however, in the light of the Fundamental Processes. At one moment in the mind there is a conception of an Immediate Presentation, together with a relation to another Immediate Presentation, the first sinking in consciousness as the second rises.

There is thus a clear, and inevitable conception of a space related, momentarily, to any positions to which attention is directed in that space. After that immediate intuitive, qualitative conception, the questions of possible or actual measurements may or may not arise in order to give a closer determination ; but these two series of events are distinct, and must be held clearly in mind to be distinct.

Sir Arthur sums up : " To put the conclusion rather crudely—space is not a lot of points close together ; it is a lot of distances interlocked. Accordingly our fundamental hypothesis is that—*Everything connected with location which enters into observational knowledge—everything we can know about the configuration of events is contained in a relation of extension between pairs of events.* This relation is called the *interval*, and its measure is denoted by *ds*."

All this is in the style beloved of the Schoolmen, and

one can see why, for it would be possible for two opposing schools to argue for centuries as to whether a point is the extremity of a line, or a line something whose extremity is a point.

In the same section (p. 9 of *The Mathematical Theory of Relativity*) Sir Arthur compares a system S consisting of events A, B, C, D, \dots and a system S' consisting of events A', B', C', D' , and in this manner endeavours to make his meaning clearer.

I confess I should hardly have been able to make head or tail of the whole section, had I not in mind the conception that one obtains from the Cartesian system of coordinates ; but with the perfectly lucid exposition of Descartes it is possible to see the gist of it, and to observe how the Relativitists have obtained their suggestions by coasting along the ideas of Descartes and giving them the air of paradox. But Sir Arthur says at the end of this section : " Again let the systems S and S' be abstract coordinate frames of references, the events being the corners of the meshes." Now, if this have any meaning, the ' events ' are known by way of Immediate Presentation, and the meshes are only of use to give more accurate determinations. So that an analysis of space, which is ultimate in this particular, does, contrary to what he has previously said, lead to a ' here ' and a ' there.' *

* There is no objection to a mathematician taking a familiar conception and then finding means of approaching it from a different standpoint ; on the contrary, this exercise of the analytic faculty is always commendable. Plücker has used it to great advantage in taking, not the point, as Descartes did, but the line, ostensibly, but in reality the plane, as his basic element. Two planes intersect in a straight line, and the intersection of this straight line with another plane gives a point. Plücker has also coasted along the ideas of Descartes, while representing the basic elements by combinations of other forms ; but he had in

Sir Arthur says (p. 10) : “ We must keep side by side the two methods of describing the configuration of events by coordinates and by the mutual intervals, respectively—the first for its conciseness, and the second for its immediate absolute significance.” He then defines the ‘ interval ’ by means of coordinates ; that is to say he defines a certain relation of space which is “ a lot of distances interlocked ” by means of a coordinate system based on points, although he makes the distinction, “ space is not a lot of points close together ” :

$$ds^2 = g_{11}dx_1^2 + g_{22}dx_2^2 + g_{33}dx_3^2 + g_{44}dx_4^2 + 2g_{12}dx_1dx_2 + \dots + 2g_{34}dx_3dx_4. \dots\dots\dots(2.1)$$

Where does he get these coordinates ? He does not say, but, as a matter of fact, they are derived from a generalisation of a quadratic form which, with suitable modifications, represents the Cartesian coordinates.*

This form (2.1) claims our attention again, for as we are dealing with new coordinates, and as we have lost the old idea of space—for otherwise the three Cartesian coordinates and the parameter, t , for time would suffice—we may ask what ground have we for limiting to a quadratic form this new space ? Sir Arthur certainly gives a reason. “ It follows that when time is not involved the interval reduces to the distance. It

view the utilisation of his system for cases where the mathematical exposition was aided by the assumptions he adopted ; and where, moreover, it was possible to find a geometrical representation for algebraic expressions in which the number of parameters, or coordinates, was greater than that of the Cartesian coordinates ; but all this is conceived in a scientific spirit different to that of the Relativitists, who affect to find a certain superiority where they have merely given a confused aspect to clear issues.

* Cf. the chapter on Riemann’s metric, p. 148 ; and also p. 178.

is for this reason that the quadratic form (2. 1) is needed in order to agree with observation ; for it is well known that in three dimensions the square of the distance between two neighbouring points is a quadratic function of their infinitesimal coordinate differences—a result depending ultimately on the experimental law expressed by Euclid I. 47.”

These words bear out what I have remarked again and again, that there is nothing self-sufficing in their particular conception of Relativity ; it is simply a gloss on the Cartesian system, with an opening of escape that, whenever a test may be possible, it pleads that it is reduced to that system which it claims to replace. But if that be so when time is not involved, what is the meaning of the interval where time *is* involved ?

Lagrange, we have noticed, has an expression for the position of a moving point—where, therefore, the distance involves time—but in order to determine the position of the point at any moment we require, of course, to know the law according to which its coordinates depend on time. The Relativitists dispense with that necessity, and in return supply us with an expression for the interval, while disdaining to tell us how they allow for the dependence of the interval on time, beyond once and for all giving us an undefined form which appears to apply to all varieties of conditions.

This interval affair is so potent in Relativity and so mysterious that I have sought to probe its secrets to the bottom. What is it that measures time, apparently in the same way that a foot-rule, or some other rule, may measure the Cartesian coordinates ? Sir Arthur says that “ intervals which can be measured by a clock cannot be measured by a scale, and vice versa.”

In another place (p. 12) he says: "If we have a machine capable of cyclic motion, its cycles will measure equal intervals. The clock primarily measures equal intervals; it is only under more restricted conditions that it also measures the time-coordinate t ."

Now as his two measuring instruments are rods and clocks, and as his interval, in the general expression—that is to say, under general conditions—involves time, and yet the clock does not, except under certain conditions, measure time, we might seem forced to the conclusion to measure time by rods.

This may be a matter of verbal expression, and so I will not force it, though if there be a confusion in expression it is not mine but Sir Arthur's. But let us now, even at the risk of tedium in repetition, reach bed-rock in this question. Here is my own statement. Time is a condition of the world in which we live, and our conception of time is inevitable and immediate, independently of all measures.* The measures which we may subsequently employ are not necessarily related to those of space, though, as space and time are both fundamental, and continually interlinked, they may be such as also have an application to space. Thus, in the first instance, the very faculty by which we have consciousness of time enables us also to estimate that any occurrence has a certain continuance, small or great, in time, and that between two occurrences there is an interval—not the interval of the Relativitists—which we feel to be small or great as compared to others. These subjective impressions are, however, not definite, and we seek for better means

* I do not wish here to set down an assumption. Cf. *Principles of Psychology*.

of assessment. If we watched successive crests of waves pass a point we might find a local measure of time, not very accurate but sufficient for some purposes. There is nothing spatial in this measurement, in the sense of belonging to a definite stretch of graduated space. If we watched a light that shone, disappeared and reappeared, we might take that as a measure of time. Now in the production of this appearance there is required an elaborate mechanism involving spatial relations ; but we may know nothing of this intuitively, and yet effectively we use the results for our purpose. In measuring time by the pulse there is no spatial measurement. As we elaborate, and render more accurate our measures of time, we take into account, as in the case of clocks, spatial conditions ; but we must not confuse these conditions with time. If instead of having hands of a clock we used another mechanical device, such as the production of the words, one, two, three, at intervals which we would, by that faculty already mentioned, recognise to be equal, and which then we might for our purpose take to be equal ; then we could have a measure which was not spatial.

All this explanation is, I know, laborious, for a child of three knows perfectly well what is meant by time, though he might be puzzled about the relativitist interval, and moreover the child knows inevitably and clearly what is meant by 'before' and 'after.' But I am answering the thesis of the Einsteinians who are continually bringing forward, in new guises, the same old misconceptions.*

* Sir Arthur breaks through the cold beauty of his academic style to ridicule the naive simplicity of such ideas as I put forth here. I am not displeased ; these little razzias of wit alleviate the pain of trying to follow him, but what he gains in genial levity he loses in weight of

The process of measuring may be inaccurate, but, step by step, as we discover causes of the errors, we may rectify them, and so, within the scope of precision of our instruments, obtain serviceable data ; and that is all we are able to do, no matter how theoretically perfect our methods of determination may appear.

In all this, however, we are on sure ground ; we know what we are doing, and we are able to reach any degree of accuracy within the limit of human skill. The Relativitists either do not know what they are doing, or else they are incapable of explaining it to us. I quote Sir Arthur :

“ It is obvious from common experience that intervals which can be measured by a clock cannot be measured with a scale, and vice versa. We have thus two varieties of intervals, which are provided in the formula (2.1), since ds^2 may be positive or negative, and the measure of the interval will accordingly be expressed by a real or an imaginary number. The abbreviated phrase ‘ imaginary interval ’ must not be allowed to mislead ; there is nothing imaginary in the corresponding relation ; it is merely that in our arbitrary code an imaginary number is assigned as its measure-number. We might have adopted a different code, and have taken, for example, the anti-logarithm

judgment. We are far from done with the ‘ interval ’ yet ; it has some choice surprises in store. While on the subject I would cite the hankering of the Relativitists for seeking to make time run backwards. Dean Inge, when President of the Aristotelian Society, illustrated their idea. He imagined a diver plunging into the water, and then described the cinematograph film moving in the reverse order. This was not a case of time moving backward, but simply of one set of images being noted, and then, still in sequence of time, another series. A small boy counting the buttons of his waistcoat first down and then up is not a great philosopher, but he does not commit the error of the Dean.

of ds^2 as the measure of the interval; in that case space intervals would have received code numbers from 1 to ∞ , and time-interval numbers from 0 to 1. When we encounter $\sqrt{-1}$ in our investigations, we must remember that it has been introduced by our choice of measure-code, and must not think of it as occurring with some mystical significance in the external world."

In this last phrase we have the good style of Sir Arthur, for the misinterpretation of the symbol $\sqrt{-1}$ has become the scandal of mathematical science. I say here in parenthesis that, as he indicates, there is never any paradox in nature; and secondly that there is never a veritable explanation in mathematics that leaves the subject in any obscure or mythical region. It is, as Gauss observed, in the obscure corners of any theory or explanation that we must search with especial assiduity, for therein is the chance of discovery. Finally, a good explanation leaves the subject as plain as that of a cabinet into which a carpenter has fitted the parts.

But that being said about the last sentence, I return to Eddington's interpretation of (2.1) in respect to time.

Here we have an extension by Riemann, seeking algebraic generalisations, of the presentation of space by means of Cartesian coordinates. The measurement by Cartesian coordinates of time as time—and not merely of the mechanical constructions recording time—would be a greater feat of misplaced imagination than the mystical interpretation of $\sqrt{-1}$. But this is the crux of that part of the Relativity theory where it seeks to rise superior to the Cartesian, or Newtonian

method. When we require a real explanation, if only either of determinations or assumptions, we get a vague and meaningless passage.

The continuation of the study of the form (2.1) is of great interest, for from this mathematical expression the Relativitists have derived results that would have astounded Riemann himself. It must always be remembered that Riemann states that space, as we know it, is of three dimensions ; * and in a later passage he remarks that the form to which Sir Arthur reduces (2.1) is that of space, and that it is the simplest of all those with which he deals, † thereby indicating that his further developments do not refer to our space but are merely mathematical relations, for which terms derived from ordinary space may be employed simply for purposes of designation. I have great respect for Riemann as a mathematician, but I do not quote him for his authority here, for, as he himself says truly, he was deficient in the philosophic studies necessary to probe these matters to the ground ; but the Relativitists have erected him into a sort of pontiff whose pronouncements are infallible, and they have misconstrued these pronouncements. These remarks will have an application in the course of the present investigation. Sir Arthur reduces the form (2.1) by a well-known method, to a sum of squares, and thus (2.1) becomes

$$ds^2 = dy_1^2 + dy_2^2 + dy_3^2 + dy_4^2$$

He recalls that this transformation is only applicable where the g 's in (2.1) are constants.

* On the hypotheses that serve as the Foundation of Geometry (in the Introduction). Cf. *Œuvres de Riemann* (translated by J. Houël), 2nd ed., p. 272.

† In the same work, Section I. of Division B.

He continues :

“ Consider all events for which y_4 has some specified value. dy_4 is then 0.

We get $ds^2 = dy_1^2 + dy_2^2 + dy_3^2$ which is like familiar space in which the interval (distance) without time is

$$ds^2 = dx^2 + dy^2 + dz^2.”$$

The formula is so like indeed that of familiar space that it is identical, and we may wonder why it is necessary to lead us through the mysteries of the Relativist ‘ interval ’ when we might have had from the first Lagrange’s clear exposition ; but we will soon be led to a characteristic Relativist error necessary to cover up a characteristic Relativist fallacy. y_1, y_2, y_3 for real events, Sir Arthur notes, must be real, therefore we have reduced the general expression to

$$ds^2 = dx^2 + dy^2 + dz^2 + dy_4^2 \dots\dots\dots(4.4)$$

where y_4 involves time.

Here already we see a move, slipped in, which is not, except in special circumstances, admissible. It does not follow that if we get a reduction of

$$ds^2 = dx^2 + dy^2 + dz^2,$$

where y_4 is constant, that, when y_4 is variable,

$$ds^2 = dx^2 + dy^2 + dz^2 + dy_4^2.$$

That would not be permissible if x, y, z varied with t ; therefore if we accept it we must assume x, y, z as not dependent on t .

The argument continues : “ We have for the two events marking the beginning and the end of the cycle

$$dx, dy, dz = 0$$

and (4.4) gives for this case

$$ds^2 = dy_4^2.$$

“ We have seen that the cycles correspond to equal intervals ds ; hence they correspond to equal values of dy_4 , but by the above definition of time they also correspond to equal lapses of time dt ; hence we must have dy_4 proportional to dt , and we write this proportionality by

$$dy_4 = ic dt,$$

where $i = \sqrt{-1}$, and c is a constant.”

Thus (4.4) becomes

$$ds^2 = dx^2 + dy^2 + dz^2 - c^2 dt^2.$$

This is the goal to which Sir Arthur was heading, for he knew it was there, and that it was necessary to reach it for ulterior, inadmissible purposes ; and it is here precisely that we may hold him up for presentation of his papers.

We have had once more the “ thickets of concealment,” and the best way out of tangle is to invoke the clear light of Lagrange again.

If dx , dy , dz be independent of time, then the development of the interval at any moment, following upon $ds^2 = dx^2 + dy^2 + dz^2$, is due solely to the position of the moving point at the moment chosen, t , where the motion of this point has been in accordance with the manner of movement of the law we may choose to consider. That motion may, or may not, be expressible as simply proportional to dt or dt^2 ; it may be very involved ; but in any case, as it is a movement in space, it is expressible in terms dx , dy , dz , however complicated, as functions of dt , and therefore—if it be necessary to introduce this further complication—of $-cdt^2$.

And now that we have discovered the meaning of y_4 , which was first given to us without any explanation whatever, we should have, instead of dy_4^2 , $f(dy_4)$.

If the interval were simply an expression of a

distance, with a further expression of some function of time simply appended to it, it would be impossible to attach any serviceable meaning to this interval. If the function involving time involves the position of the second point whose motion decides, at any moment, the interval, then $f(dy_4)$ becomes a function of new dx, dy, dz , involving time. There would, however, in this case, be no sense in introducing generally i or c , and there would be gross error in representing the interval now in the form given; and if we sought to use such a form for the general expression of distance (where the interval at any moment was represented as a distance) then the form would not be a quadratic form, and the whole edifice built on that quadratic form of Riemann falls to the ground.*

It should be noticed here also that as dx, dy, dz do not contain t , and as c is constant, then the interval would remain unchanged for all modes of motion of the point which at a given moment would mark the further extremity of the interval.

Sir Arthur observes then that another form

$$ds^2 = dx^2 + dy^2 + dz^2 - c^2 dt^2 - 2ca dx dt - 2c\beta dy dt - 2c\gamma dz dt \dots\dots(4.7)$$

may appear more general, but he gives reasons for rejecting it. His argument runs thus :

“The clock records equal *intervals*, hence the difference of clock readings at the beginning and end of the journey will be proportional to the integrated interval.

$$\int_1^2 ds \dots\dots\dots(4.81)''$$

* A similar conclusion is reached by M. Le Roux in discussing the use of this expression of Riemann in Einstein's theory of Gravitation. Cf. p. 208, *et seq.*

It may be remarked parenthetically that this is true only when the 'interval' is entirely composed of time units, for if it involved space units, then the clock might have any relation, even the most fantastic, between its cycles and the space interval; it might be carried all round the world, to be finally brought to rest at any arbitrary space interval from the starting point.

Now considering only the x axes, "we shall have from (4.7)

$$\begin{aligned} -ds^2 &= c^2 dt^2 + 2ca \, dx \, dt - dx^2 \\ &= c^2 dt^2 \left(1 + \frac{2a}{c} \frac{dx}{dt} - \frac{1}{c^2} \left(\frac{dx}{dt} \right)^2 \right). \end{aligned}$$

Hence the difference of the clock readings (4.81) is proportional to

$$\int_{t_1}^{t_2} dt \left(1 + \frac{2av}{c} - \frac{v^2}{c^2} \right)^{\frac{1}{2}} \dots \dots \dots (4.82)$$

where $v = \frac{dx}{dt}$ is the velocity of the clock.

This will not in general reduce to $t_2 - t_1$; so that the difference of time at the two places is not given correctly by the reading of the clock. Even when $a = 0$, the moving clock does not record correct time."

If u be very small, he remarks, and hence $t_2 - t_1$ large, we may neglect $\frac{u^2}{c^2}$, and then (4.82) becomes

$$\int_{t_1}^{t_2} \left(1 + \frac{a}{c} \cdot \frac{dx}{dt} \right)$$

approximately $= t_2 - t_1 + \frac{a}{c} (x_2 - x_1)$.

"The clock, if moved sufficiently slowly, will record the correct time difference if, and only if, $a = 0$."

Here again there is a wrong use of mathematical processes. We are dealing with an interval (undefined) which depends, in this simplified case, on the value of x and on a function of t , involving a determined x and an extension of x , which in order to avoid ambiguity we may call x_1 ; and to make the equation intelligible there should be expressed the manner in which this x_1 is a function of t . Then we could work out the differential equation in order to obtain this variation of the interval produced by the variation of t , and we should then add this to the part of the integral not dependent on t . The only other alternative is to add a time interval to an independent spatial interval, but even in Relativity that does not appear to have any signification. But what has been done here is first to take the variation of the interval as dependent on t —otherwise the transformation of (4. 81) to (4. 82) would not be justified—and then in the integration to take the old variable x as a function of t ; but in that case the whole interval would be a function of time, and we should have

$$\int_1^2 ds = \int_{t_1}^{t_2} f'(t) dt = f(t_2) - f(t_1),$$

or, since in the reasoning cited, the case of $f(t) = t$ is tacitly assumed, we should have

$$\int_1^2 ds = t_2 - t_1$$

and the mystery would have vanished.

In the general expression we have the case of a movement of a point from a fixed point, which may be the origin, according to a certain law, expressed by $f(t)$; and this case Lagrange would treat with perfect lucidity, without invoking a metaphysical 'interval';

but also he might point out that the law of motion would not in general be expressible in a quadratic form, and that therefore finally this paraphernalia of the 'interval' is either useless, or a source of deception.

Once more, I confess, I have been tedious, but I plead that I was not born to it, I did not willingly achieve it, it was forced upon me. For, look at the difficulties. I am now dealing with, not such absurdities as the curvature of space, or limitless space remaining finite, or the velocity of light being a maximum of the Universe; these are, after all, only fritters of this Baratarian dish of illusions; I am here tackling the pith of the doctrine of Relativity, without which indeed it could not exist. . . . I have read Lagrange, Jacobi, Darboux, all men of higher calibre than Einstein or Minkowski, and I have had no difficulty in understanding their expositions of movements involving time. They do not confound time and space; nor does a child of five. It requires a specialised, sophisticated intellect, warped by bad metaphysics, to reach such a pinnacle; and it needs further mis-intelligence to identify space and time with the measure employed. That being so, first Minkowski, and then Einstein, and at length Sir Arthur try to inculcate into us the doctrine of the 'interval.' There has been no need for it, and when we ask them what it is, they throw it at us without explanation. Sir Arthur is, where he is lucid, quite lucid, but I have read him on the interval again and again till my brain reeled; almost I had become a Relativist! Recovering, I sought for guidance, but no Relativist seemed to know what he himself meant by the 'interval.' I have quoted Sir Arthur largely and literally; if then

any reader can tell me, from these citations, distinctly and definitely what the interval means, I will award him the palm I refuse to Einstein.

M. Le Roux, whose mathematical skill and clear comprehension of the meaning of physical relations are in a sphere beyond the Relativitists, seems baffled by the 'interval'; at any rate he reaches a conclusion about it which not all Relativitists would accept, the more so, that they differ amongst themselves. However, with patience and care, I have from Sir Arthur's expositions fixed upon certain implications. These I have followed to their consequences. They are false. I was led to the point where I had to accept the conclusion that a clock would go fast or slow according as it moved relatively to imagined systems of coordinates. Now I believe that the motion of a clock depends on the operation of physical forces and that these, as the constructors of clocks know, have nothing to do with the influences imputed to Nature by the Einsteinists. Knowing, therefore, that the absurdity existed, just as our old friend Diogenes, when the Sophists told him there was no such thing as motion, I went a step further, and determined to place my finger on the very spot. To do this I had to follow portentous mathematical demonstrations, for Sir Arthur, in a style reminiscent of Maupertuis, tries to vanquish our common sense by a wrong use of the calculus; and it was in this journey from an intellectual Dan to Beersheba that I lost all cheerfulness and became the dull plodder you have seen; *Peccavi*.

Sir Arthur remarks: "The clock, if moved sufficiently slowly, will record the correct time-difference if, and only if, $a=0$."

A little further on he says that he is one of those who “regard all coordinate frames as equally fictitious structures,” and as in reference to the coordinate frame we have the same relation whether the clock, or the frame, be moved, then we arrive at this remarkable result that the clock goes fast or slow according as we move a fictitious frame one way or another ; and since simultaneously one Relativitist may select one frame, and another another, the clock will simultaneously move fast and slow. At this point I turned back to the title page of his book to see whether by mistake I had picked up the work of another noted mathematician—*Alice in Wonderland*.

CHAPTER XIV

THE SPACE-TIME CONTINUUM

THE elimination of the ether by edict of Einstein—for he has offered no reason for this action—might seem to have left the world somewhat bare ; but that is not the case. The space which takes the place of the ether has no *points de repère* (points of fixation or registration) so that the interior constitution of that space gives us no resting place, but this formless space is bounded, according to Einstein, following Riemann, by a continuous surface on which a sentient being might wander while believing that his freedom was limitless. How they can certify this I do not know, but let that trifle pass ; they leave us still our old infinity even if they give it another name. Where I feel, however, that I must join issue with Einstein is in that conception of space without *points de repère* which in the end confronts us with an infinity of *points de repère*, in fact, in the language of mathematics, an infinity to the second power, since it provides us with a surface. All that has been accomplished too with but little assistance from Nature, for since the material objects in the Universe are, compared to space, like “ five flies in Europe,” the building of the boundary was a great work of imaginative engineering ; remember, for instance, that Wren in framing the

dome of St. Paul's had need at least of *points de repère*.

But let us take all that in our stride, for these wonders pale their ineffectual fires before the solid foundation of Relativist faith laid down by Minkowski—the space-time continuum. Professor Whitehead and others speak of this as a wonderful conception of genius. They are modest. If I could conceive what it means I should not stop at these tame expressions; I should not be content with promenading in its glory the three graces of journalism: Dramatic, Sensational, Amazing; I should try to invent a new language. But then I cannot conceive what it means, and I am shrewdly inclined to add, neither do they.

We must remember that this continuum, thus sensationally introduced, is not a substitute for the space without *points de repère*, it is the space without *points de repère*; for, having taken away our ether, each of these has been offered to us as the sole replacer, and if two things are equal to the same thing, they are equal to one another.

That last phrase is from our old friend Euclid, and as I write it I feel, in the presence of Relativists, how weak it seems. Euclid and I can only reason, we are united by the fetters of sense; at times, if we must come to humiliating confessions, we are tied to common sense; and as we look, one to the other, we feel how forlorn is our condition in the company of men who know what a space-time continuum is—Whitehead, Minkowski, Einstein, to give the music of the names, the music of the spheres, its full mysterious sweep up to that high diapason.

But even so, as worms will turn, and we have no

other resource than reason, we reason. We will pass over the confusion of time and space, as we have dealt with that already.

If the continuum have no *points de repère*, how can we envisage it? Lord Kelvin, following the tendency of English genius, so befitting in an Irishman, declared that he could not understand anything unless he could make a model of it, and incidentally, he gave us the "foam ether," whose filaments—too delicate for touch, for they had a negative resistance to pressure—had to be nailed down once and for all to some still more mysterious rigid container. What model can we form of the continuum? After earnest searching throughout the imaginations of scientific men, from Roger Bacon's 'Hyli' to Bernard Shaw's 'Methusaleh,' I find that the only structure that vaguely approaches the continuum is Sir Joseph Larmor's ether. This substance is so delicate that it allows material bodies to pass through it without let or hindrance, and yet is so solid and stupendously rigid that the buttresses of Milton's hell are egg shells in comparison. What a wonderful thing is science!

But no, that would not do for the continuum, for surely the continuum is too exclusive to allow stray bodies to slip in. I would be content to sweep away their qualities altogether, and find rest in a sort of scientific Nirvana without *points de repère*; but then! There are material bodies in the Universe whether we desire them or not; these are the flies in the—Continent. The continuum would be possible if those flies could be multiplied into so many that they filled all Europe, and were themselves homogeneous; or if there were no flies at all; but a continuum without

points de repère broken at irregular intervals by disparate things which, having themselves diverse structures, are not continuous ; that is not possible.

Let us then abandon for a moment the peaks of ultra-science, and descend to the earth. The material things of which the earth, and the objects that, remote from the earth, come within our ken, are comprised ultimately of atoms ; and these atoms, so far from responding in any degree to the demand of the continuum, are notoriously discontinuous, because they are composed of discrete bodies, electrons and protons, certainly not identical, and separated from each other by such distances that they form in miniature a sort of solar system. Further, when we open our eyes we behold a myriad of discrete, separated objects, of all the colours of the rainbow, and of all manner of differences of shape and texture. Yet, such is the influence of authority, such the malign effect of that brain-burning, so assiduously carried on in education, from the elementary schools upward, that a mathematical friend was surprised and almost scandalised when I said that I found discontinuity in these objects of nature. He had been taught the doctrine of the 'continuum.' It is true that as he was a serious thinker, resolved not to be led like a fool all his life, Mr. M. Kendall aforementioned, for it was he, applied his reason to the matter and then found himself face to face with a spate of new ideas that had not appeared in his curriculum.

I refer to him of set purpose as a wrangler because it is usual in mathematics to speak of continuity, and to persuade ourselves that all the apparatus of the differential calculus would collapse if we did not speak of continuity. That also is sheer nonsense, for the

differential calculus is founded on the assumption of discontinuity, although the scope of the discontinuity may be reduced to limits as small as we please.

Those of the German mathematicians, Weierstrass, for instance, who sought for rigour in demonstration, made a great point of defining clearly what is meant by continuity ; and their definitions I not only accept, but believe to be of great assistance in giving a clear conception of the subject. In the *Cours d'Analyse* of Jordan, this is set forth in a lucid manner, and Jordan's work is very valuable. But even here we are dealing with mathematical conceptions ; we are delimiting the domain within which we can apply certain symbols, and carrying on reasonings according to logical laws. Whether all this applies to the forms of the material world is another question ; but even if a direct application were not possible it would not follow that this use of the mode of argument were inadmissible. For instance, we may reason quite correctly by way of resolving forces into forces at right angles which are purely suppositions ; but we should know what we are doing, and not declare that forces do actually exist because we have supposed them in the course of transforming mathematical expressions.*

* The distinction between a condition of nature and a mathematical convention is well expressed, in regard to continuity, by an eminent French mathematician, Jacques Hadamard, in a work to which we shall refer later, "*Leçons sur la propagation des Ondes*" (Librairie Hermann), 1903, p. 59 : " We will suppose that the functions x, y, z , are, in general, continuous." Here is a mathematical convention. Then, " There is no reason to suppose that the different parts of a certain fluid do not diffuse one into the other as do the molecules of two different fluids ; if it is so x, y, z , while continuous with regard to t , will be functions entirely discontinuous of a, b, c ." He uses, however, for his purpose the " hypothesis of continuity " as defined, for instance, by Jordan ; but all this is remote from the " space-time continuum."

We can, then, hold to our common sense in observing that no continuum exists in the world, and yet, if we retain our perspicacity, use the notion, or rather convention, of continuity in the differential calculus. This notion of a continuum is often found in branches of mathematics where it is least applicable, as in questions involving infinity. The notion of infinity can never be reached by any repeated use of concepts of continuity. There in fact lies the elusiveness of the Sophists' paradox when they asserted that Achilles could never overtake the tortoise.

Even if we were to postulate an objective 'continuum,' it would not be possible for the mind to conceive of it other than as a meaningless symbol; for the action of the mind is carried on by a series of discontinuities.* This depends on the actual physiological constitution of the nervous system, with whose conditions our mental states are correlated. Thus in regard to the motion say, of a projectile in the air, we represent it as being in one place at one moment, and then—since the conception of time in its advancement is a condition of nature and of our perception—we represent it at a subsequent moment in another place, the conception of space being also a condition of the nature of our perception of such events. It is impossible to trace the movement as an absolutely continuous change, for even in the mathematical sense we form the conception of successive intervals, no matter how small.

There is, however, no difficulty in this mode of perception, for our minds have been adjusted, in their

* I am not making mere assertions. The proof of what is here set down is given with all necessary completeness and rigour in *Principles of Psychology*.

original constitution, and in their development, to perform their functions precisely under these conditions. When, in considering a long series of which the sum of the terms gradually approaches to a definite amount, we do not determine the equality of the sum with that amount, by tracing out the whole course of the series, term by term, but by making a 'leap,' and so arriving at the limit. There is nothing hazardous, or artificial, in this leap; it is not directed by ourselves any more than any new sensation, or Immediate Presentation is directed by ourselves; it is part of the normal functioning of the mind. The old adage "Nature does not make a leap," must not be taken in too narrow a sense. The argument was used, in a slightly different form, to prove that a vacuum could not be attained, until Toricelli first amongst mortal men beheld the vacuum in his barometer and recognised its significance. There was a break of continuity there evidently. And so in the more recondite mental phenomena the processes of progression are always marked by discontinuity; and if the correlated physiological processes be investigated, the processes will be found there also to be marked by discontinuity.

It is strange that the Relativitists who are so immersed, not in valid psychology, but in fantastic metaphysics, should never have considered the researches of Fechner. This German philosopher, who was a physicist and a spiritualist, endeavoured to form a bridge between his psychic and physical researches by showing that the connexion between a stimulus and the effect in consciousness was represented by a logarithmic curve. Fechner, however, did establish the doctrine of the *Schwelle*, or threshold, that must

be passed before consciousness becomes evoked, and E. H. Weber, in a series of masterly experiments, gave us valuable data on this subject.* Since that time numerous physiologists and experimental psychologists have extended the subject and rendered the data more accurate; and in an analogous branch of science, that dealing with what the French, who have studied the matter assiduously, called *Chronaxie* (chronaxy) one is brought face to face with the discontinuity of mental perceptions, and at the same time with their dependence on the nervous substratum.

In the doctrine of the 'quanta' also which nearly all the Relativitists including Einstein accept, there is a direct contradiction to any conception of the 'continuum.' I do not here enter into any discussion of the validity of the doctrine; I set it down simply as an 'argumentum ad hominem' for those who accept it. In that branch of mathematics also, the *Mengenlehre* associated with the name of Cantor in Germany, Borel in France—*Théorie des ensembles*—and with that of W. H. Young in England—*Sets of points*—there are arguments used in regard to these sets of points which, in my opinion, are vitiated by the lack of appreciation of the true meaning of infinity, so applied; but in any case nothing is more evident than that the theory presupposes discontinuity; otherwise there would be no points at all, not even *points de repère*.

All that being said, do I hope that my arguments will dispose of the continuum? At one time I would have said, Yes; for the arguments are beyond refutation; but—I have become acquainted with the minds of highly trained philosophers. It was sufficient in the

* *Tastsinn und Gemeingefühl*, by E. H. Weber (Ostwald's Klassiker).

old days for the Schoolman to quote Aristotle, always in his work of least value, to defend their own ignorance ; it was laudable to admire Maupertuis with that stroke of genius of his in enhancing God's greatness by faulty interpretations of formulae, and to cite Euler and Frederick the Great by way of resisting plain proofs ; it was the sign of eminence in science to defend the theory of Phlogiston against the demonstration of Lavoisier ; it was the sign of a reconciliation of piety and advanced biology to believe in spontaneous generation and to support Bastian against Huxley ; it was an article of faith to uphold Cuvier and Owen and to denounce Darwin in regard to Evolution ; it is now the pride of certain high authorities in the medical world to exalt that doctrine, the most scientifically contemptible of all, Freud's theory, as against scientific psychology ; and now buttressed by the *morgue académique* some of the University lecturers exclude arguments so low as those of common sense, reason, rigorous analysis, while in reply to cogent proofs they think it sufficient to reply—Einstein.*

If this language appears somewhat gross for a criticism of science, I reply that when the Relativitists put before us a scientific argument I criticise it in scientific terms, seeking only more precise definitions,

* Curiously enough this very book, before publication, has been criticised on these lines by a certain University professor, Kerr Grant, of Adelaide, Australia. Charles Lamb remarked humorously that reading a book before criticising it prejudiced one ; this professor evidently does not need such aid. His argument is that Einstein must be right, because many University professors endorse him. He gives an illuminating insight into the value of such endorsement. The argument is the most contemptible of all, but perhaps the most influential. In this case, however, we will see that it does not hold. The great majority of scientific men reject Einstein's theory.

a more profound analysis of the conditions, and a more rigorous standard in regard to cogency of the reasoning. M. Picard shows the most delicate handling of Einstein's argument, but even he cannot refrain from the enjoyment of his own irony. Henri Poincaré exclaimed in impatience against the style of Lorentz continually trying to hold untenable positions by the invention of new hypotheses, *ad hoc*. M. Le Roux, addressing his communications to the Académie des Sciences, responds to all the forms of politeness usual in that illustrious assembly, but at times he is forced to use the word 'absurdities.' M. A. Duport, Emeritus Professor at the Faculty of Science of the University of Dijon, author of *La Loi de l'Attraction Universelle*, who has put his finger on mathematical errors of Mayer and Riemann, and therefore does not feel intimidated by Einstein's popular vogue, lays on even more stoutly. I do not quote him for his authority, but for the terms of his appreciation: "Einstein and his disciples," he says, "have merely made a Jezebel of all the worst theories of the mathematicians in the interpretation of the discoveries of Electricity and Optics. . . . The affair has been cleverly prepared. Memoirs have crowded the German scientific periodicals, full of contradictions and rectifications; then they have announced some sensational results: the contraction of Lorentz, the space of four dimensions of Minkowski; and the popular books have followed before one could criticise these affirmations."

He opines that this new science is called Relativity, "doubtless because it can be understood only by people who have a particular conformation of brain, a relativist conformation."

“ It is of small importance to the relativist to solve the problems if only he has the air of doing so. . . . His procedure is simple ; he takes the results demonstrated by other methods, and he accepts them *à priori*, afterwards he alters them for his purpose. One searches in vain for an exposition of what he accepts and what he rejects.”

M. Duport insists that the prestige of Einstein is not derived from his science, but from the intensive propaganda set up with deliberate purpose by the “ Einstein group.”

But M. Bouasse, Professor of Physics at the University of Toulouse, author of a great series of works of real value, uses language still more disrespectful. He speaks of the ‘ insanities ’ of the Relativitists.

Now in spite of the ‘ courage ’ attributed to me—possibly in a dyslogistic sense—for daring to read Einstein in a state devoid of hypnotism, I have not yet used the word ‘ insanity ’ ; but I here make a confession. After having wearied and irritated myself in my attempt to find some real value of science in Relativity, I turn to Bouasse from time to time, watch him tossing Einstein in a scientific blanket, and when in sheer exasperation he explodes his verbal bomb—‘ insanities ’—I feel in that blessed word some consolation.

CHAPTER XV

GRAVITATION: PRELIMINARY CONSIDERATIONS

GRAVITATION only came gradually into the orbit of Relativity. It was, however, bound to follow, for if the formula of Lorentz could be applied to Optics and to Electro-magnetism without investigation of the necessary physical conditions, why not to Gravitation ?

Gravitation was known to the wonderful old Greeks, who had the gist at least of all that we call science now ; but it required centuries to traverse the distance between the recognition of a force and ascertainment of its law of action. Galileo, Kepler, Newton, all tried to discover this law, and Newton was the first to achieve success, for he was the first to be supplied, by means of observation and experiments, with the necessary data. I have mentioned this expressly once more because it is necessary to insist that whatever be the cause of gravitation it is a purely physical phenomenon, and the means of approach to its understanding lie always in the domain of observation, experiment and reasoning on the data given. Nothing of this sort will be found in Relativity, as we shall see when we examine the demonstration by means of mathematics, offered to us by one of the most talented of Einstein's disciples, Schwarzschild.

Herr Schwarzschild pleases me not less than Professor

Carmichael and M. Bricout, for he gives us something definite to lay hold of, and his mathematics are as hazardous as Einstein's psychology. In the meantime, however, I want to discuss some aspects of the less technical side of the theory of gravitation according to the Relativitists.

The old hypothesis of action at a distance is less and less in favour at present amongst physicists, who are searching for explanations, implying a medium, which may be no other than the ether; and following upon this assumption the problem becomes suggested of ascertaining the velocity of propagation of gravitation. M. J. Chazy, Professor at the Sorbonne, who is of the school of Einstein, does not admit a finite velocity for gravitation, or did not, at least, at one time. M. E. Kogbetliantz not only believes that gravitation has a finite velocity, but he has devised an ingenious apparatus destined to ascertain this velocity, and to his apparatus he has, from time to time, added certain improvements. Then M. Chazy points out that the necessary conditions for the structure of this apparatus may be found in Nature. Using the solar system as his instrument he finds that from a study of the secular accelerations of the earth, the moon, and Mercury, the velocity of transmissional gravity must be enormously greater than that of light, perhaps 380 times as great, perhaps 70,000 times as great. M. Kogbetliantz has not got that far, he has not given any definite measure, but he is not in this particular in disaccord with M. Chazy's results, for even 70,000 times as great a velocity as light is not infinite.

There is this important point to be borne in mind at this stage—that if the lowest estimation of M. Chazy

be accepted, it destroys the value of Einstein's assertion that the velocity of light is a constant, and that no greater velocity is possible in the universe. M. Chazy says that, though he believes the velocity of gravitation to be too great to be detected by a laboratory experiment, yet if M. Kogbetliantz could demonstrate a velocity possibly greater than that of light but of the same order, then both the Newtonian theory and that of Relativity would go by the board. That is doubtless true as regards Newton's tacit assumption of action at a distance and instantaneous action. As to Einstein's Article of Faith, he never had the slightest grounds for putting it forward, nor is it even of any value to his theory. It is merely one of his dicta. As a matter of fact, recent careful measures have left in doubt as to whether the velocity of light may not change with time. There is at least nothing absurd in the supposition, for other physical phenomena, magnetism, for example, change periodically, and also secularly, perhaps in longer periods. Reference may here be made to the figures on page 137.*

These figures are interesting for more reasons than one, for they show that with the most refined

* As showing how hazardous it is to lay down dicta, derived from theoretical considerations, on the velocity of light, Helmholtz may be read, "Ueber die Bewegungsgleichungen der Electricität für ruhende leitende Körper" (Crelle, 72, 1870). He suggests a modification of a formula of W. Weber by the introduction of a term involving a constant K . Clerk-Maxwell at one time adopted 0 as the value of K , but this gave an infinite velocity for electricity. F. E. Neumann made $K=1$, which gave electricity the velocity of light, and the application of this result gave Maxwell one of his greatest triumphs. Helmholtz believes it possible that electricity has a greater velocity than light. Now all these men are great and serious thinkers, yet I do not find their arguments convincing, and what renders it less difficult for me to say so is that their theories are all different.

instruments and most careful measurements, errors of observation may occur in excess of those that would suffice to render nugatory the Michelson-Morley results.

Another scientist who has devoted much time to the study of gravitation is M. G. Maneff. He has a theory of his own by which he deduces the displacement of the rays of the spectrum towards the red in a manner which excludes Einstein's explanations. He has some points of contact with Einstein, for he obtains various expressions of the mass of a body in movement, and in consequence submits a law that modifies Newton's and explains the displacement of the movement of the perihelion of Mercury better than by Relativity.

Then M. Duport, M. A. Kastler, and M. Le Roux enter the arena in much more decisive opposition to Einstein. M. Duport's point of view will be taken into account later in considering the mathematics of the problem. M. Kastler* is interesting if only because he is an original thinker, who treats gravitation on a new basis; the mutual acceleration of two bodies creates in them "forces of acceleration," tending to oppose the action. These forces are too small to be measured in two small bodies, but when one of them is the universe they appear as "forces of inertia." Now as the fields of inertia and gravitation are equivalent, gravitation is another case of the general forces of acceleration. The old static conception of gravitation must give way to a dynamical conception, and we are led to the problem of seeking for the concealed dynamic principles which produce the effects of gravitation. This effect emanates from the ultimate corpuscles of

* *J. Phys.*, 2 (1931).

electrons and protons of which matter is composed, and therefore the effect depends on the internal dynamism of these corpuscles. He finds this dynamic character of the corpuscles becoming more and more evident, as in the spin of the electrons and of the protons, and in the internal vibrations which characterise the theory of M. L. de Broglie.

Now all this has not been elaborated to the degree where it presents clear points of issue, and moreover it is hazardous to ascribe effects to electrons and protons which are so far little understood. Sir Ambrose Fleming as a result of recent experiments has offered reasons for questioning the existence of protons, while others who recognise their existence deny their 'spin.' I cite this study rather because, in place of Einstein's moonshine, the author is endeavouring to get to grips with physical realities.

M. Le Roux, as usual, in his mathematical investigations on the problem of gravitation, makes the subject clearer. In his study of the Michelson-Morley experiment, following the method of M. Hadamard, one of the most accomplished of French mathematicians, for the study of the propagation of waves, he has given reasons for supposing that the ether is influenced by gravitation.

M. Le Roux has worked out an invariant expression of the law of gravitation, which differs from that of Einstein in that it has a real meaning, and the mathematics are intelligible and valid.* He also runs counter to Einstein and Schwarzschild in this, that he finds that it is impossible to formulate a law of gravitation when only two material points are considered; the

* *Comptes Rendus*, Académie des Sciences, 8th June, 1931.

only law which can be obtained in such a case is that which represents the variation of the distance.

M. Le Roux had some years previously * in a brief paper stated arguments that to me seem conclusive against the whole theory of gravitation of the Relativitists. It is somewhat technical, but while some readers may skip it, I give, for the sake of the mathematician, an indication of the reasoning.

In the classical mechanics the problem of $n+1$ points is the scheme of the general theory of gravitation. Among the properties of the differential equations of the movement, attention should be called to the following : (1) the equations of the movement of each point contain all the coordinates of the point, and at the same time the coordinates of all the other points of the system ; (2) by the integration of these equations the coordinates of all the points become expressed in functions of the same variable.

The first property corresponds to the mutual actions and the perturbations. In consequence there arises a solidarity such that it is not possible to integrate separately the equations of a point or a fraction of the system. The complete system must be considered as an irreducible whole.

In virtue of the second property there exists between the points of the system a correspondence and a co-ordination such that to any position of one of the elements there correspond determinate positions of all the others. That solidarity of the movements is the essential character of gravitation in the classical mechanics.

In the theory of Einstein nothing of the sort exists.

* *Comptes Rendus*, Académie des Sciences, 6th November, 1922.

The fundamental hypothesis, that which serves as the base of all the calculations, consists in admitting the existence of a quadratic form of differentials of *four variables*.

$$ds^2 = \Sigma g_{jk} dx_j dx_k$$

of which the geodetics define the movements of the points submitted to the law of gravitation.

The equations of the movement of a point established in virtue of that hypothesis, contain only the four coordinates of the point. They can be integrated without the necessity of considering the concomitant movements of the other points. The problem of the $n + 1$ points assumes therefore in Einstein's mechanics a singular and unexpected simplification. There is no solidarity, no mutual actions, no perturbations. Neither is there coordination.

Let us consider in particular two points of the system, defined respectively by two coordinates of space-time :

$$Px(x_1x_2x_3x_4) \text{ and } Py(y_1y_2y_3y_4).$$

The differential equations of the movement of each point, united to the initial conditions, enable us to express three of the coordinates of the point in functions of the fourth. Consequently we may regard x_1, x_2, x_3 , as functions of x_4 ; and y_1, y_2, y_3 , as functions of y_4 . On the other hand, these equations establish no relation between x_4 and y_4 .

The movement of each point depends therefore on a single variable, but there exists no necessary relation between the variables which refer to the movements of two different points. That result signifies the absence of all necessary correlation between the points of two different geodesics of the same surface.

As one conclusion M. Le Roux submits that by the theory of Einstein, thus mathematically expressed, he could never explain, or predict, with even the widest approximation, the secular movement of the perihelion of Mercury.

Now, here again, I do not quote M. Le Roux for his authority, but simply because he has expressed concisely conclusions to which I had independently arrived. Reference will be made to this paper subsequently when we consider the ' verifications ' of the theory of Relativity which Einstein and followers have put forward. It is only right to say that the arguments of M. Le Roux were criticised by M. Brillouin, who is an eminent physicist well worthy of commanding attention. He points out that the coefficients of the ds^2 of the Universe of four dimensions depend on the coordinates of all the points of the system, and that all the coordinates vary together.

To this M. Le Roux has replied * saying: " We have here certainly a quadratic form of four differentials, but, in virtue of the variation together of all the coordinates, that form is no longer a linear element of four dimensions; the movements are not defined by the geodetics of the Universe; the considerations relative to the curvature of the Universe have no longer any sense; and, finally,—a more serious fact,—the quasi-geometrical explanation of gravitation disappears. . . ."

The difference in the points of view of M. Le Roux and M. Brillouin may be reduced to this, he says: M. Brillouin accepts for the coefficients $g_{\mu\nu}$ the most general solutions of the equations of partial derivatives

* *Comptes Rendus*, Académie des Sciences, 4th December, 1922.

of Einstein ; he, on the contrary, demands, in conformity with the original suppositions, those solutions which yield a veritable ds^2 of four dimensions.

I leave the matter there, for in dealing with this part of the theory, I take objection even at a deeper level than that of M. Le Roux. With respect to Einstein's own expositions, these are never so explicit and precise as those of his followers. In the *Annalen der Physik*, Leipzig, 1916, he gives an authoritative account of his theory. We find (p. 770) the statement : " To two given material points of a (rigid) body at rest corresponds a stretch (*strecke*) of determined length, independently of the site and position (*Orientierung*) of the body and independently of time."

Now by virtue of gravitation alone, to say nothing of other physical factors, such as heat, it may be said that two material particles in a so-called rigid body—there are no absolutely rigid bodies—change their relative distance from each other. The change may be extremely small, but the whole system of movements of any kind is formed by the combination of elementary movements. The answer to this is that in actual practice we can neglect these movements and changes, which at the limit may be infinitesimal, and that we can suppose a theoretically rigid body. This, precisely, we invariably do in dealing with such matters, but that is an example of abstraction, and the Relativitists endeavour to eliminate abstraction by the use of their devices of measurement. Here, however, Einstein tacitly invokes the principle of abstraction, and so he does right throughout the whole series of his speculations, and so do all the Relativitists.

It is true that the General Relativity theory does

not involve acceptance of the positions here laid down; but that is due to other divergences from pure reason, and it does not affect the point which here I wished to make clear, that Einstein assumes something which is only true when dealing with abstract constructions.

Einstein reinforces (p. 772) the argument which he calls a "ponderous argument" in the theory of knowledge (*Erkenntnistheoretisches*) by citing "a well-known physical fact."

If there be a Galilean system of reference K , that is one in which a mass, sufficiently removed from other masses, moves uniformly in a straight line; then let us suppose another system K' which moves relatively to K in a uniformly accelerated manner. Finally, relative to K' let there be a mass sufficiently removed from others, which moves with an accelerated motion. He asks whether an observer at rest with respect to K' could draw the conclusion that he was on an accelerated system, and he answers in the negative.

The reason he gives is this: Let the system K be unaccelerated and suppose a gravitation field, which produces the accelerated motion of the body relative to K' . [Here it might be submitted that the body would not be susceptible to such a gravitation field unless there were some other body present to produce the field. But let us pass over that objection.] He says that the behaviour of the body relative to K' is the same as it would be relative to a system at rest; therefore, he concludes it is easy to assume from the physical standpoint that it is equally right to take either K' or K as the system at rest.

Here we are with Einstein; that is to say, if we are

permitted to express clearly what he involves in a certain ambiguity.

If we are describing a succession of events, we may take any system, and any centre of coordinates that we find convenient; and as the phenomena in question have nothing to do with mental difficulties they will proceed according to the 'laws' that govern them. The considerations that guide us in the selection of one or another system of reference are mainly the desire to obtain simple, comprehensible expressions of the course of events.

That being so, the next sentence of Einstein's requires close attention. He says: From these considerations one sees that the development of the general theory of Relativity must lead at once to a theory of gravitation; since one can 'generate' a gravitation field simply by changing the system of coordinates.

That sentence stamps the whole mentality of the Relativitists, and one finds that imprint, again and again, in Sir Arthur Eddington's writings, not that Sir Arthur is a muddled thinker. He is indeed remarkably lucid, till he comes into contact with Relativity.

Instead of replying directly to Einstein, I will put the matter in a form that will make such a reply unnecessary. Gravitation, as we know it and as Einstein accepts it, is a force of nature, universal in the scope of the Universe that we consider. It does not depend on our system of coordinates, for in fact it existed centuries before anyone had begun to inquire as to its existence or character. If a man is falling from Brooklyn Bridge it does not suffice for him to say, "Let us forget all that"; he continues to fall.

If he sets up a system of coordinates to measure the rate of falling, or to enable him to describe the movements of a falling body which has had an initial velocity in any direction, that system does not determine the phenomena; its only use is to enable him to obtain numbers that register measurements, or otherwise to express in some sort of a formula what he observes. And so, if he changes his system for another, he does not change the nature of the phenomena, and certainly he does not 'generate' the force of gravitation. At the best, and worst, according to his selection, he changes the magnitude of his figures, or the forms of the expressions of the movements.

I dwell on this a moment, for the reason that right through the writings of the Relativitists will be found the influence of the doctrine that the meaning of the physical phenomena is found in the expression of the quantitative determinations. Now phenomena exist apart from our appreciations of any sort; and at first blush it would seem that our minds are not endowed with the subtle quality required in psychological analysis if they cannot discriminate between a sequence of events, and the particular forms of description, for instance, measurements applied to the events. But it is precisely that lack of analytical faculty that has made the 'strength' of the Relativitists; that is to say, the hardihood to push forward, no matter what contradictions or sheer absurdities they encounter.

Einstein continues: In a similar manner we see '*unmittelbar*' (immediately) that the principle of the constancy of the velocity of light in a vacuum must be

modified. For one easily recognises that the path of a beam of light, relative to K' , must generally be crooked, when the light, with respect to K , moves in a straight line with definite constant velocity.*

The word '*unmittelbar*' amused me so much that I have taken care to give it in the original German. By that sign alone I would recognise a Relativist. Jean Bernoulli, you remember, receiving an unsigned solution by Newton of a mathematical problem, cried, "I know the lion by his claws." I recognise the Einsteinian by his flaws.

The whole paragraph is interesting because it goes on to deal with one of the profound discoveries of Relativity, that the velocity of light in reference to a body is the same whether that body be at rest, or in motion towards the source of light! I have repeated this again and again, and I have already dealt with it in considering Professor Carmichael's exposition; but I have referred to it once more because it is continually returning to view in the writings of the Relativists.

I notice for the moment that Einstein, having postulated the constancy of light, is content to 'modify' it when his own reasoning leads him to contradiction; but he does not touch the previous mode of thought that led him to decree this constancy.

* What Einstein sees here '*unmittelbar*,' he failed to see during the many years when he was insisting on his dogma of the constancy of the velocity of light. Cf. note, p. 135.

CHAPTER XVI

THE DEVELOPMENT OF THE MATHEMATICAL INSTRUMENT

WE have already seen the general course of the development of modern mathematics since the discovery * by Descartes of the system of coordinates that bears his name. Soon afterwards, the discovery of the infinitesimal calculus, then the calculus of variations, the exploration of the vast field thus opened up, and the applications of the results, occupied the minds of mathematicians for many generations. After Leibnitz and Newton, Euler and Lagrange did most to develop the subject, but, wonderful as was their work, it was not specially marked by the introduction of entirely new mathematical instruments.

The method of projection had, it is true, been known long previously, and it had been employed by Newton in his investigation of the properties of curves of the third degree, but Poncelet was the first to make of this method a new branch of study. He was a prisoner in Russia after Napoleon's expedition to Moscow, and being deprived of all books, mathematical or other, he

* Hermite in his letters to Stieltjes always speaks of the establishment of mathematical relations as being not inventions but 'discoveries,' and therein, I think, he shows the spirit of a great mathematician, or again, as he preferred to say, 'algebraist.'

employed his mind in the study of the effects of projection of geometrical figures. This subject was destined to receive great developments.

The most notable *Ausgangspunkt* (source) of new ideas in mathematics was, however, the work of Gauss. He examined with sharp criticism all manner of assumptions, including the axioms, which, either directly or by tacit acceptance, had gained admission during the foundations of mathematics. It is to him that the researches into non-Euclidean geometry are due. There is nothing fantastic in the conceptions upon which he worked; the problem was simply that of tracing out the consequences of accepting one or other, but not all, of the conditions of parallelism as understood by Euclid. From Gauss therefore proceeded many of the ideas that appear in the doctrine of Relativity, but with Gauss these had a well-defined meaning, and his intellect had no leanings to mere fantastic aberrations. Gauss inspired the work of the Hungarian Bolyai and also of the Russian Lobatchewski; and he thought the work of that remarkable thinker so important that in his old age he learnt Russian in order to follow his demonstrations.

From Gauss also proceeded the works of men whose product was very dissimilar, that of Jacobi, who handled mathematical expressions with extraordinary virtuosity, and that of von Staudt, who had an aversion to the symbols of calculations, and who built up, with marvellous skill, the science of representation by geometrical constructions alone.* Contemporary with

* Darboux, towards the end of his own great career, became more and more impressed by the importance and the fascination of von Staudt's work.

Jacobi, the Swiss Steiner had also developed a system which had affinities with—though also considerable differences from—the conceptions of Poncelet, Brianchon, and von Staudt.

We will see the effect of these later in regard to Relativity. The mathematician who has had the greatest influence on Einstein and his followers is, however, Riemann, whose famous memoir on the foundations of mathematics owes its impulse to Gauss. Like the work of the master, it is remarkably clear-sighted and sane, except perhaps in one unfortunate extension of a philosophic character which has been the glory, and the undoing, of the Relativist school.

Meanwhile there was still another break-away from the previous modes of development of the Cartesian conceptions; and that was the effort to recognise, and at times discard, whatever was arbitrary in the assumptions, so as to give a greater generality to the entire system. Plücker led the way here by taking, not the point, but the line, or rather virtually though not explicitly, the plane as the fundamental figure;* and the geometry which he developed on these lines inspired the work of Felix Klein who covered an extraordinarily wide range of mathematics, and also of Sophus Lie, the Norwegian, whose researches in the theory of differential equations are truly profound. Plücker, Riemann and Sophus Lie made mathematicians familiar with 'multiplicities' of several dimensions, and incidentally deranged the intellects of

* It should be remarked, though I have not seen it noted by any mathematician, that Plücker was not entirely consistent, or he would have taken, in place of the origin of coordinates, a plane. The three planes of Descartes would be represented by three points symmetrically disposed on the plane.

ingenious and clever men in their attempts to realise a space of four dimensions.

Previously two mathematicians of genius whose lives were cut short prematurely, the Norwegian, Niels Abel, and the young Frenchman, Evariste Galois, who was killed in a duel at the age of 21, had done great work, part of which, the theory of groups, is important in the mathematics employed in discussions of Relativity.

Galois was in some respects the continuator of Abel, and whereas Abel had given some examples of the group system in the roots of equations, Galois made it evident that the conception of the group was the key to the understanding of the meaning of equations in general. Sophus Lie subsequently developed the ideas of Galois, and the theory of groups now forms an extensive branch of mathematics.

Cauchy had in the meantime, in the course of original researches into the character of integral equations, shown the value of the symbol $\sqrt{-1}$ in mathematical work. Here again a device of great value has been the cause of much confusion, for many memoirs, principally in this country and in France, have been written to explain the 'inner meaning' of the symbol, and as they differ widely, they cannot all be right; my own opinion is that they are all wrong, for not one of them gives evidence of that basic analysis which is necessary in questions of this sort. Cauchy regarded the symbol as a mere index of classification, and within the scope in which he employs it, he is consistent and correct. In most English books it is taken as a sort of operator producing perpendicularity from a given datum line. Sir William Rowan Hamilton, the inventor of

quaternions, adopts this view, and extends it; his three symbols i, j, k , are based on the model of $\sqrt{-1}$, as he makes the square of either of these equal to -1 , and he also makes the square of a unit vector equal to -1 .

Hamilton has taken great pains to show that the method he adopts does not rest on mere assumptions, but has a profound psychological basis. Here, however, in spite of my admiration for his work, I am unable to follow him. His arguments in this respect, though ingenious and meticulous, are not based on a clear conception of ultimate psychological analysis; they amount to nothing more than a forced application of the conventions of mathematics, and they are not consistent with themselves. They are an example of a fact, often to be noticed, that an acceptable conclusion does not necessarily imply the correctness of the arguments on which it is supposed to rest; it would be possible to reach all the valid conclusions of the quaternion system without the use of his assumptions. The interpretation of $\sqrt{-1}$ as implying perpendicularity is fantastical, although, following upon new assumptions and conventions, such a use may in certain circumstances be useful by way of application. The multiplication of one vector by another has no real meaning, and the assumption, again, reveals the lack of psychological analysis; but that fact does not prevent him from obtaining, in certain circumstances, correct results.

To show, however, that the results do not depend on any inevitability of Hamilton's postulates, it may be remarked that Grassmann, a mathematician of no less talent, has in his *Ausdehnungslehre* used different interpretations—the square of a unit vector he takes to be

positive unity—and he reaches equally correct conclusions. The multiplication of one vector by another is, however, in itself meaningless. One may bring them into juxtaposition and employ a conventional sign ; but we could do that for two cows as well as two vectors, and by using a conventional sign, and again subsequently interpreting the sign by a convention, we might reach valid results. There is no multiplication except by scalars,* and the conventional so-called multiplication of vectors may be taken as a symbolic form for the veritable scalar multiplication.

It is not right, however, to say that the two are equivalent, for nothing is so important in the treatment of questions of subtlety as the clear comprehension of what we are doing step by step ; and in no subject is this more apparent than in the doctrine of Relativity where, as M. Le Roux points out in regard particularly to Minkowski's 'interval,' by using *si* in place of *u*, the change of expression of the interval is such that the transformations of Lorentz become reduced to the more familiar orthogonal transformations for four variables. " But we must not fail to recognise that such simplification is illusory in the interpretation of real phenomena. . . . That artifice of calculation may be advantageous in certain cases ; it may produce inconvenience in others. It is nothing but an artifice, and the conclusions which some claim to draw from it from the point of view of physical realities are absolutely unacceptable." Here he puts his finger on a cardinal fault in the reasoning of all the Relativitists.

* The analysis by which I have been led to this conclusion is too minute and delicate to find a place here ; but those interested may consult *Principles of Psychology*. It is noteworthy, too, that the remote origins of multiplication do imply associations of sets of material objects.

The germinal idea of Hamilton's work was that of extending the conceptions of Descartes, by introducing algebraic forms into the changes of geometrical figures, as, for instance, in the transformation of one line into another, with more directness than by the use of coordinates. The addition of vectors was known, and for many years he sought the secret of multiplication, and at last obtained a solution which, though apparently based on a searching analysis, is really empirical and conventional. Instead of a point P being reached from the origin O by means of three steps—along the axis of x , then along the axis of y , then along the axis of z , or their parallels; he conceived the vector OP as a fundamental figure, and any other vector say OQ as being derivable from it by his system of multiplication. Here, however, as Plücker had found, it is impossible to escape from the Cartesian system, for the simple reason that space is measurable by three coordinates; and as the vectors themselves were related to each other by the angle between them and this angle remains an invariant if one vector be rotated round the other so as always to lie on a cone, the vectors themselves had to be defined by the use of three coordinates, or vectors. The name quaternion implies this use, for it is suggested by the number four, formed of the four terms, the three vectors and a scalar. (Ladder)

Hamilton's system has a defect in that the so-called multiplication which he adopted is not commutative; nevertheless there was fertility in the suggestion of escaping, when possible, from the structural apparatus of the Cartesian coordinates. These are aids in dealing with a problem, but they are nevertheless foreign

to the essential character of the problems of physical relations and phenomena. This idea was the source of the development of the tensor calculus so much employed by the Relativitists.

To those who find an intellectual satisfaction in seeking general forms, from which various particular results may be obtained—and this Aristotle rightly took to be the great intellectual faculty in the ‘*dia-noetic*’ side of man’s activity—it is interesting to find affinities between this suggestion which leads to the tensor calculus, concerned with geometrical figures as little as possible, and the system of von Staudt, who tried to discard everything but these figures; the vectors of Hamilton are the syntheses of what is expressible in more complicated forms; and the lines of von Staudt’s constructions also synthesise mathematical formulae, and by their immediate visual relations show solutions of certain problems, usually laboriously worked out by formulae. These methods respond to Gergonne’s desire for processes as intuitive as possible; and in this regard, von Staudt, Hamilton, and Ricci, the pioneer of the tensor calculus, have something in common.

Before we arrive at Ricci’s work, however, we must take into account that of Riemann again. He sought to generalise in various directions. Gauss had, in regard to given surfaces, replaced the Cartesian coordinates by coordinates on the surface itself, and with the equation of the surface he required only two to determine any point. Riemann extended the forms beyond three variables, though using geometrical terms as a help, by analogy, towards comprehension; he defined the position of points by the use of geodetic lines

(lines of shortest length from point to point, as on a surface), giving the origin and the direction of these geodetics.

In this way he enlarged the scope of conceptions in dealing with problems involving algebraic expressions containing many variables, and he set mathematicians thinking in terms of the 'forms' themselves; and so there came into existence a branch of mathematics, The Theory of Forms, afterwards extensively developed by Clebsch and Aronhold and Petersen, a Dane, who has produced good works on these subjects.

Amongst the problems were those of ascertaining the invariants in the transformation of one form into another. It had gradually become impressed on mathematicians that, as expressed by the highly accomplished English mathematician, Cayley, the discovery of invariants was after all the real object of research in mathematical problems. Amongst those who studied the matter in accord with the conceptions of Riemann were Christoffel and Lipschitz,* and a memoir of Christoffel laid the foundation of methods extensively used since in modern mathematics.† Christoffel, working on a suggestion arising out of the researches of Lamé, was seeking an extension to a space of n dimensions, of the problem of surfaces applicable, one upon the other. This led him to the study of the conditions of invariance in the transformation of a quadratic differential form, with non-constant coefficients, into a similar form in other coordinates.

* R. Lipschitz, *Crelle*, vol. 70, 1869.

† Cf. E. B. Christoffel, *Journal f. d. rein. u. angew. Math.* (*Crelle*) 70, 1869. This memoir attracted little attention at the time, and it was only a result, not sought for by Christoffel, that of "covariant differentiation," which gave the impulse to new work.

A quadratic form is represented thus,

$$g_{ij}dx^i dx^j = g_{11}dx^1 dx^1 + g_{12}dx^1 dx^2 + \dots + g_{nn}dx^n dx^n,$$

the coefficients g being functions of the x^i .

Here may be noted a convention due to Einstein: where in a term an index, such as i or j above, is repeated, then a summation is intended, from 1 to n , whatever the n chosen may be in the particular case. Thus the left-hand side of the above equation denotes already the right-hand side consisting of the sum of n^2 terms. This, it may be remarked, and the use, not in accordance with its proper meaning, of the word 'tensor' for a form which we will subsequently meet, constitute, as far as I have been able to see, the original contributions of Einstein to the mathematical side of the theory of Relativity.

If we transform the expression in terms of another system \bar{x} preserving the invariance, we obtain

$$\bar{g}_{ij}d\bar{x}^i d\bar{x}^j = gab dx^a dx^b,$$

then since, by the usual law of transformation, the terms of the right are indicated by

$$gab \frac{\partial x^a}{\partial \bar{x}^i} \cdot \frac{\partial x^b}{\partial \bar{x}^j} d\bar{x}^i d\bar{x}^j \text{ we get } \bar{g}_{ij} = gab \frac{\partial x^a}{\partial \bar{x}^i} \cdot \frac{\partial x^b}{\partial \bar{x}^j}.$$

gab is a simple 'tensor,' the test being that the transformation yields an expression where gab has a coefficient as represented, the a and b being current, that is to say, having values from 1 to n , where here the i and j represent definite numbers, for they are not repeated in the term given.

If we differentiate \bar{g}^{ij} we get an expression

$$\begin{aligned} \frac{\partial \bar{g}^{ij}}{\partial \bar{x}^k} &= \frac{\partial gab}{\partial x^c} \cdot \frac{\partial x^a}{\partial \bar{x}^i} \cdot \frac{\partial x^b}{\partial \bar{x}^j} \cdot \frac{\partial x^c}{\partial \bar{x}^k} \\ &+ gab \frac{\partial^2 x^a}{\partial \bar{x}^i \partial \bar{x}^k} \cdot \frac{\partial x^b}{\partial \bar{x}^j} + gab \frac{\partial x^a}{\partial \bar{x}^i} \cdot \frac{\partial^2 x^b}{\partial \bar{x}^j \partial \bar{x}^k}. \end{aligned}$$

This, however, is not according to the definition a tensor, for the first expression on the right which has the tensor form is followed by two others in which derivatives of the second order occur.

By cyclic permutation of the indices, however, we get two other equations of a similar form, and by subtracting the first from the sum of these two and dividing by 2, we find at length

$$\frac{1}{2} \left(\frac{\partial \bar{g}^{ik}}{\partial \bar{x}^j} + \frac{\partial \bar{g}^{jk}}{\partial \bar{x}^i} - \frac{\partial \bar{g}^{ij}}{\partial \bar{x}^k} \right) = \frac{1}{2} \left(\frac{\partial g_{ac}}{\partial x^b} + \frac{\partial g_{cb}}{\partial x^a} - \frac{\partial g_{ab}}{\partial x^c} \right) \\ \times \frac{\partial x^a}{\partial \bar{x}^i} \cdot \frac{\partial x^b}{\partial \bar{x}^j} \cdot \frac{\partial x^c}{\partial \bar{x}^k} + gab \frac{\partial x^a}{\partial \bar{x}^k} \cdot \frac{\partial^2 x^b}{\partial \bar{x}^i \partial \bar{x}^j}.$$

The first term with brackets on the right-hand side is called Christoffel's 3-index symbol of the first kind, and it is denoted by the symbol $\Gamma_{ab, c}$.

If we multiply both sides of the equation by

$$\bar{g}^{lk} \frac{\partial x^d}{\partial \bar{x}^l} = g^{ac} \frac{\partial \bar{x}^k}{\partial x^c}$$

we obtain a modification involving a summation.

We find then a form, $g^{dc} \Gamma_{ab, c}$, and this expressed by $\{^a_b\}$, or by Γ^d_{ab} , is called Christoffel's 3-index symbol of the second kind.

At length we arrive at the form, which may be written

$$(1), \quad \bar{\Gamma}^r_{pq} = \Gamma^k_{ij} \frac{\partial x^i}{\partial \bar{x}^p} \cdot \frac{\partial x^j}{\partial \bar{x}^q} \cdot \frac{\partial \bar{x}^r}{\partial x^k} + \frac{\partial^2 x^i}{\partial \bar{x}^p \partial \bar{x}^q} \cdot \frac{\partial \bar{x}^r}{\partial x^i}.$$

Here we have only one term which is a second derivative; but we have not yet obtained a tensor. Now if V^i be a contravariant vector, that is to say, if its law of transformation be like that of differentials,

$$dx^i = \frac{\partial x^i}{\partial x^a} \cdot dx^a,$$

we have
$$\bar{V}^r(\bar{x}) = V^k \frac{\partial \bar{x}^r}{\partial x^k};$$

then, if we differentiate this, we get

$$(2), \quad \frac{\partial \bar{V}^r}{\partial \bar{x}^a} = \frac{\partial V^k}{\partial x^j} \cdot \frac{\partial \bar{x}^r}{\partial x^k} \cdot \frac{\partial x^j}{\partial \bar{x}^a} + V^k \frac{\partial^2 \bar{x}^r}{\partial x^k \partial x^j} \cdot \frac{\partial x^j}{\partial \bar{x}^a}.$$

Here also we have no tensor. The final expression on the right-hand side is of the second order, and is similar in form to that which appears in the equation (1). By combining equations (1) and (2), it is therefore possible to eliminate the expressions of the second order, and finally we get

$$V^k_{,j} = \frac{\partial V^k}{\partial x^j} + V^i \Gamma_{ij}^k.$$

This tensor is the 'covariant derivative' of V^k . If

V_i be a covariant vector we get $V_{i,j} = \frac{\partial V_i}{\partial x^j} - V_a \Gamma_{ij}^a$.

This tensor is the covariant derivative of V_i .

The great advantage of using the covariant derivative lies in the fact that it secures the elimination of terms of the second order from the equations.

The process of covariant differentiation is not commutative, in regard to a new differentiation.

It will be found that, carrying out the rule for covariant differentiation, first in the order jk , and then in the order kj , we get $V^i_{,j,k} - V^i_{,k,j} = V^a B^i_{ajk}$ where B^i_{ajk} designates the terms

$$\frac{\partial \Gamma^i_{aj}}{\partial x^k} - \frac{\partial \Gamma^i_{ak}}{\partial x^j} + \Gamma^b_{aj} \Gamma^i_{bk} - \Gamma^b_{ak} \Gamma^i_{bj}.$$

B^i_{ajk} is called the 'curvature tensor.' If we 'contract' a curvature tensor by making the superscript index the same as the middle subscript, we get the Ricci tensor. Thus B^a_{iaj} denoted by R_{ij} is called the Ricci tensor.

For a quadratic differential form we have $g^{ij}R_{ij}$ denoted by R , and this is the 'scalar curvature.'

Finally, $g_{ia}B_{jkl}^a$, denoted by R_{ijkl} , is called the 'covariant curvature tensor' or the 'Riemann-Christoffel tensor.'

So far we have had examples of particular tensors, so that a general definition is advisable of a 'tensor field,' that being the aggregate of tensor values at the points of the field. A tensor field is an invariant whose components, in any coordinate system represented by symbols x , are functions of the coordinates x , and which may be expressed in the form

$$T_{b_1 \dots b_m}^{a_1 \dots a_k}$$

the indices being whole numbers ranging from 1 to n ; and, what is of great importance, the law of transformation being thus expressed :

$$\bar{T}_{j_1 \dots j_m}^{i_1 \dots i_k} = \left| \frac{\partial x}{\partial \bar{x}} \right|^N T_{b_1 \dots b_m}^{a_1 \dots a_k} \frac{\partial \bar{x}^{i_1}}{\partial x^{a_1}} \dots \frac{\partial \bar{x}^{i_k}}{\partial x^{a_k}} \cdot \frac{\partial x^{b_1}}{\partial \bar{x}^{j_1}} \dots \frac{\partial x^{b_m}}{\partial \bar{x}^{j_m}}.$$

The tensor field so expressed is covariant of order m and contravariant of order k . The total order is $m+k$. The value of the tensor T at any point involves the determination of n quantities which are the components of the tensor at that point.

The development of the various branches of mathematics we have surveyed has not depended on any theory of Relativity, any more than the differential calculus depends on any conception of the nature of the propagation of light. The followers of Newton, Fresnel and de Broglie may all employ the differential calculus to prove their respective theories, but the fact that such a refined instrument of computation is used gives no guarantee of the excellence of the doctrine

considered. This remark must be kept in mind in regard to Relativity. Riemann had no thought of determining the physical conditions of the universe when he proposed a generalisation simply on formal lines of the system of coordinates, and Lipschitz and Christoffel, who were writing in Crelle's Journal a little later, had no thought beyond the research for mathematical relations. Thus it happened that it was not till nearly twenty years afterwards that the importance of covariant differentiation was recognised by Ricci, to whom the term covariant differentiation is due.*

Ricci developed the whole subject of what we now know as tensor analysis. Among the guiding suggestions is that which is the fundamental principle of algebra, the elimination, as far as possible, from the statement of mathematical relations, of all that is merely incidental in the expression. This is associated with a scheme of classification, recalling that of determinants, where the form of expression gives a clear view of its implications, and facilitates the mathematical operations with which it deals.†

Thus for instance in the expression of the terms of a quadratic differential form, it is only necessary to state g_{ij} , for that implies $g_{ij}dx^i dx^j$, and by giving to i and j the entire range of numbers from 1 to n , we obtain all the terms of the form. The manipulation of the indices, as we have seen, indicates the carrying out of operations which without this aid might have a very complicated appearance.

* G. Ricci, *Atti della R. Acc. dei Lincei. Rendiconti*, Ser. 4, Vol. 3, pt. I. (1887).

† Cf. G. Ricci and T. Levi-Civita, *Méthodes de Calcul Différentiel absolu et leurs applications*, Math. Annal, Vol. 54, 1900.

Yet out of all this complication Einstein is able to 'choose' a tensor to represent his law of gravitation, while absolving him from the need of considering the physical conditions under which the force operates. It may be remarked that his choice has been aided by the fact that, equating the tensor to zero (as he does), he obtains the condition of flat space-time in the Relativist world, and also, when the coordinates are reduced in number, in our ordinary world. In other words, he traces his theory on that already known and demonstrated as our ordinary law of gravitation; the difference escaping any control by experiment, because it lies in a transcendental world. But let us see the demonstration in detail.

CHAPTER XVII

RELATIVITIST GRAVITATION

I PROPOSE now to discuss "Einstein's Law of Gravitation" as set forth by Sir Arthur Eddington and elucidated by the mathematics of K. Schwarzschild; and as I believe that an attentive examination of the procedure will absolve me from following in detail other examples of their mathematical operations, I quote literally in all that is essential.*

"The contracted Riemann-Christoffel tensor is formed by setting $\varepsilon = \sigma$ in $B_{\mu\nu\sigma}^e$. It is denoted by $G_{\mu\nu}$. Hence by (34. 4)

$$G_{\mu\nu} = \{\mu\sigma, \alpha\}\{\alpha\nu, \sigma\} - \{\mu\nu, \alpha\}\{\alpha\sigma, \sigma\} + \frac{\partial}{\partial x_\nu}\{\mu\sigma, \sigma\} - \frac{\partial}{\partial x_\sigma}\{\mu\nu, \sigma\}, \dots\dots\dots(37. 1)$$

The symbols containing a duplicated suffix are simplified by (35. 4), viz.:

$$\{\mu\sigma, \sigma\} = \frac{\partial}{\partial x_\mu} \log \sqrt{-g}.$$

Hence with some alterations of dummy suffixes,

$$G_{\mu\nu} = -\frac{\partial}{\partial x_\alpha}\{\mu\nu, \alpha\} + \{\mu\alpha, \beta\}\{\nu\beta, \alpha\} + \frac{\partial^2}{\partial x_\mu \partial x_\nu} \log \sqrt{-g} - \{\mu\nu, \alpha\} \frac{\partial}{\partial x_\alpha} \log \sqrt{-g}, \dots(37. 2)$$

* Cf. Sir Arthur Eddington, *The Mathematical Theory of Relativity*, p. 81 .

Contraction by setting $\varepsilon = \mu$ does not provide an alternative tensor, because $B_{\mu\nu\sigma}^{\mu} = g^{\mu\rho} B_{\mu\nu\sigma\rho} = 0$, owing to the anti-symmetry of $B_{\mu\nu\sigma\rho}$ in μ and ρ .

The law
$$G_{\mu\nu} = 0 \dots\dots\dots(37.3)$$

in empty space, is chosen by Einstein for his law of gravitation.

We see from (37.2) that $G_{\mu\nu}$ is a symmetrical tensor; consequently the law provides 10 partial differential equations to determine the $g_{\mu\nu}$. It will be found later (§ 52) that there are 4 identical relations between them, so that the number of equations is effectively reduced to 6. The equations are of the second order and involve the second differential coefficients of $g_{\mu\nu}$ linearly. We proved in (§ 36) that tensors not containing derivatives beyond the second must necessarily be compounded from $g_{\mu\nu}$ and $B_{\mu\nu\sigma}^{\varepsilon}$; so that, unless we are prepared to go beyond the second order, the choice of a law of gravitation is very limited, and we can scarcely avoid relying on the tensor $G_{\mu\nu}$.*

Without introducing higher derivatives, which would seem out of place in this problem, we can suggest as an alternative to (37.3) the law

$$G_{\mu\nu} = \lambda g_{\mu\nu} \dots\dots\dots(37.4)$$

where λ is a universal constant. There are theoretical grounds for believing that this is actually the correct form; but it is certain that λ must be an extremely small constant, so that in practical application we still take (37.3) as sufficiently approximate. The introduction of the small constant λ leads to the spherical

* The law $B_{\mu\nu\sigma\rho} = 0$ (giving flat space-time throughout all empty regions) would obviously be too stringent, since it does not admit of the existence of irreducible fields of force.

world of Einstein or de Sitter to which we shall return in Chapter V.

The spur $G = g^{\mu\nu} G_{\mu\nu} \dots\dots\dots (37.5)$ is called the Gaussian curvature, or simply *the curvature*, of space-time. It must be remembered, however, that the deviation from flatness is described in greater detail by the tensors $G_{\mu\nu}$ and $B_{\mu\nu\sigma\rho}$ (sometimes called *components of curvature*) and the vanishing of G is by no means a sufficient condition for flat space-time.

Einstein's law of gravitation expresses the fact that the geometry of an empty region of the world is not of the most general Riemannian type, but is limited. General Riemannian geometry corresponds to the quadratic form (2.1) with the g 's entirely unrestricted functions of the coordinates; Einstein asserts that the natural geometry of an empty region is not of so unlimited a kind, and the possible values of the g 's are restricted to those which satisfy the differential equations (37.3). It will be remembered that a field of force arises from the discrepancy between the natural geometry of a coordinate-system and the abstract Galilean geometry attributed to it; thus any law governing a field of force must be a law governing the natural geometry. That is why the law of gravitation must appear as a restriction on the possible natural geometry of the world. The inverse square law, which is a plausible law of weakening of a supposed absolute force, becomes quite unintelligible (and indeed impossible) when expressed as a restriction on the intrinsic geometry of space-time; we have to substitute some law obeyed by the tensors which describe the world conditions determining the natural geometry.

38. *The gravitational field of an isolated particle.* We have now to determine a particular solution of the equations (37. 3). The solution which we shall obtain will ultimately be shown to correspond to the field of an isolated particle continually at rest at the origin; and in seeking a solution we shall be guided by our general idea of the type of solution to be expected of such a particle. This preliminary argument need not be rigorous; the final test is whether the formulae suggested by it satisfy the equations to be solved.

In flat space-time the interval, referred to spherical polar coordinates and time is

$$ds^2 = -dr^2 - r^2 d\theta^2 - r^2 \sin^2 \theta d\varphi^2 + dt^2. \dots(38. 11)$$

If we consider what modifications of this can be made without destroying the spherical symmetry in space, the symmetry as regards past and future time, or the static condition, the most general possible form appears to be

$$ds^2 = -U(r) dr^2 - V(r)(r^2 d\theta^2 + r^2 \sin^2 \theta d\varphi^2) + W(r) dt^2, \dots(38. 12)$$

when U, V, W are arbitrary functions of r .

Let $r_1^2 = r^2 V(r)$.

Then (38. 12) becomes of the form

$$ds^2 = -U_1(r_1) dr_1^2 - r_1^2 d\theta^2 - r_1^2 \sin^2 \theta d\varphi^2 + W_1(r_1) dt^2 \dots(38. 13)$$

where U_1 and W_1 are arbitrary functions of r_1 .

There is no reason to regard r in (38. 12) as more immediately the counterpart of r in (38. 11) than r_1 is.

If the functions U, V, W , differ only slightly from unity, both r and r_1 will have approximately the properties of the radius-vector in Euclidean geometry;

but no length in non-Euclidean space can have exactly the properties of an Euclidean radius-vector, and it is arbitrary whether we choose r or r_1 as its closest representative. We shall here choose r_1 and accordingly drop the suffix, writing (38. 13) in the form

$$ds^2 = -e^\lambda dr^2 - r^2 d\theta^2 - r^2 \sin^2 \theta d\varphi^2 + e^\nu dt^2 \dots(38. 2)$$

where λ and ν are functions of r only.

Moreover, since the gravitational field (or disturbance of flat space-time) due to a particle diminishes indefinitely as we go to an infinite distance, we must have λ and ν tend to zero as r tends to infinity. Formula (38. 2) will then reduce to (38. 11) at an infinite distance from the particle.

Our coordinates are

$$x_1 = r, \quad x_2 = \theta, \quad x_3 = \varphi, \quad x_4 = t$$

and the fundamental tensor is by (38. 2)

$$g_{11} = -e^\lambda, \quad g_{22} = -r^2, \quad g_{33} = -r^2 \sin^2 \theta, \quad g_{44} = e^\nu \dots(38. 31)$$

and $g_{\mu\nu} = 0$ if $\mu \neq \nu$.

The determinant g reduces to its leading diagonal

$$g_{11} g_{22} g_{33} g_{44}.$$

Hence

$$-g = e^{\lambda+\nu} r^4 \sin^2 \theta \dots\dots\dots(38. 32)$$

and

$$g^{11} = 1/g_{11}, \text{ etc.}$$

so that

$$g^{11} = -e^{-\lambda}, \quad g^{22} = -1/r^2, \quad g^{33} = -1/r^2 \sin^2 \theta, \quad g^{44} = e^{-\nu}. \quad (38. 3)$$

Since all the $g^{\mu\nu}$ vanish except when the two suffixes are the same, the summation disappears in the formula for the 3-index symbols (27. 2), and

$$\{\mu\nu, \sigma\} = \frac{1}{2} g^{\sigma\sigma} \left(\frac{\partial g_{\mu\sigma}}{\partial x_\nu} + \frac{\partial g_{\nu\sigma}}{\partial x_\mu} - \frac{\partial g_{\mu\nu}}{\partial x_\sigma} \right) \text{ not summed.}$$

If μ, ν, σ , denote *different* suffixes we get the following possible cases (the summation convention being suspended).

$$\left. \begin{aligned} \{\mu\mu, \mu\} &= \frac{1}{2}g^{\mu\mu} \frac{\partial g_{\mu\mu}}{\partial x_\mu} = \frac{1}{2} \frac{\partial}{\partial x_\mu} (\log g_{\mu\mu}) \\ \{\mu\mu, \nu\} &= -\frac{1}{2}g^{\nu\nu} \frac{\partial g_{\mu\mu}}{\partial x_\nu} \\ \{\mu\nu, \nu\} &= \frac{1}{2}g^{\nu\nu} \frac{\partial g_{\nu\nu}}{\partial x_\mu} = \frac{1}{2} \frac{\partial}{\partial x_\mu} (\log g_{\nu\nu}) \\ \{\mu\nu, \sigma\} &= 0. \end{aligned} \right\} \dots\dots(38.4)$$

It is now easy to go systematically through the forty 3-index symbols calculating the values of those which do not vanish. We obtain the following results, the accent denoting differentiation with respect to r .

$$\left. \begin{aligned} \{11, 1\} &= \frac{1}{2}\lambda' \\ \{12, 2\} &= 1/r \\ \{13, 3\} &= 1/r \\ \{14, 4\} &= \frac{1}{2}\nu' \\ \{22, 1\} &= -re^{-\lambda} \\ \{23, 3\} &= \cot \theta \\ \{33, 1\} &= -r \sin^2 \theta e^{-\lambda} \\ \{33, 2\} &= -\sin \theta \cos \theta \\ \{44, 1\} &= \frac{1}{2}e^{\gamma-\lambda}\nu' \end{aligned} \right\} \dots\dots\dots(38.4)$$

The remaining 31 symbols vanish. Note that {21, 2} is the same as {12, 2}, etc.

These values must be substituted in (37.2). As there may be some pitfalls in carrying this out, we shall first write out the equations (37.2) in full, omitting

the terms (223 in number) which now obviously vanish.

$$G_{11} = -\frac{\partial}{\partial r}\{11, 1\} + \{11, 1\}\{11, 1\} + \{12, 2\}\{12, 2\} \\ + \{13, 3\}\{13, 3\} + \{14, 4\}\{14, 4\} \\ + \frac{\partial^2}{\partial r^2} \log \sqrt{-g} - \{11, 1\} \frac{\partial}{\partial r} \log \sqrt{-g},$$

$$G_{22} = -\frac{\partial}{\partial r}\{22, 1\} + 2\{22, 1\}\{21, 2\} + \{23, 3\}\{23, 3\} \\ + \frac{\partial^2}{\partial \theta^2} \log \sqrt{-g} - \{22, 1\} \frac{\partial}{\partial r} \log \sqrt{-g},$$

$$G_{33} = -\frac{\partial}{\partial r}\{33, 1\} - \frac{\partial}{\partial \theta}\{33, 2\} + 2\{33, 1\}\{31, 3\} \\ + 2\{33, 2\}\{32, 3\} - \{33, 1\} \frac{\partial}{\partial r} \log \sqrt{-g} \\ - \{33, 2\} \frac{\partial}{\partial \theta} \log \sqrt{-g}.$$

$$G_{44} = -\frac{\partial}{\partial r}\{44, 1\} + 2\{44, 1\}\{41, 4\} \\ - \{44, 1\} \frac{\partial}{\partial r} \log \sqrt{-g}.$$

$$G_{12} = \{13, 3\}\{23, 3\} - \{12, 2\} \frac{\partial}{\partial \theta} \log \sqrt{-g}.$$

The remaining components contain no surviving terms.

Substitute from (38.5) and (38.32) in these and collect the terms. The equations to be satisfied become

$$G_{11} = \frac{1}{2}v'' - \frac{1}{4}\lambda'v' + \frac{1}{4}v'^2 - \lambda'1/r = 0, \quad \dots(38.61)$$

$$G_{22} = e^{-\lambda}(1 + \frac{1}{2}r(v' - \lambda')) - 1 = 0, \quad \dots(38.62)$$

$$G_{33} = \sin^2 \theta \cdot e^{-\lambda}(1 + \frac{1}{2}r(v' - \lambda')) - \sin^2 \theta = 0, \quad \dots(38.63)$$

$$G_{44} = e^{\nu-\lambda}(-\frac{1}{2}v'' + \frac{1}{4}\lambda'v' - \frac{1}{4}v'^2 - 1v'/r) = 0, \quad \dots(38.64)$$

$$G_{12} = 0. \quad \dots(38.65)$$

We may leave aside (38. 63) which is a mere repetition of (38. 62); then there are left three equations to be satisfied by λ and ν . From (38. 61) and (38. 64) we have $\lambda' = -\nu'$. Since λ and ν are to vanish together at $r = \infty$ this requires that $\lambda = -\nu$.

Then (38. 62) becomes $e^\nu(1 + r\nu') = 1$.

Set $e^\nu = \gamma$, then $\gamma + r\gamma' = 1$.

Hence integrating, $\gamma = 1 - \frac{2m}{r}$,(38. 7)

where $2m$ is a constant of integration.

It will be found that all three equations are satisfied by this solution. Accordingly, substituting $e^{-\lambda} = e^\nu = \gamma$ in (38. 2)

$$ds^2 = -\gamma^{-1} dr^2 - r^2 d\theta^2 - r^2 \sin^2 \theta d\phi^2 + \gamma dt^2. \dots(38. 8)$$

where $\gamma = 1 - 2m/r$, is a particular solution of Einstein's gravitational equations $G_{\mu\nu} = 0$. The solution in this form was first obtained by Schwarzschild."

It may be mentioned that, contrary to Sir Arthur's indication, the tensor formed by putting $\varepsilon = \sigma$ in $B_{\mu\nu\sigma}^e$ is not the contracted Riemann-Christoffel tensor; it is the negative of the Ricci tensor. The Riemann-Christoffel tensor is $R_{ijkl} = g_{ia} B_{jkl}^a$, sometimes called the covariant curvature tensor, B_{jkl}^a being the curvature tensor.

The first gem that seizes the attention in this exposition is the statement : The law $G_{\mu\nu} = 0$ in empty space, is *chosen* by Einstein for his law of gravitation. Hitherto all the physicists who have investigated the law of gravitation have first of all discovered that such a force as gravitation exists ; and then by observation and experiment they have endeavoured to ascertain the conditions of the universe that affect the force ; and

then finally they have sought to obtain the measures which enable them to formulate a law. This was the way of Galileo, of Kepler, and of Newton; and the ascertainment of the data here indicated was so important that a false estimation baffled Kepler and delayed for long the final triumph of Newton.

Einstein dispenses with all this. Out of the many formulae which may be derived from Riemann's coordinates—which do not, according to Riemann himself, apply, except in the simplest form, to our space—he 'chooses' a suitable expression. That word 'chooses' recalled to me another domain where amid the various displays of cerebral activity pure reason is least in honour. A Scottish Liberal desired to know from Mr. Asquith which, of several amendments, he 'fancied'; and on that occasion I was the only one who appeared to find enjoyment. I am therefore by no means sure not to find myself in a minority in astonished amusement, if one may adjoin these terms, at this mode of conducting mathematical operations. Here we are, standing before the universe of space, without *points de repère* (points of reference), according to the Relativitists, and with no instrument in our hands and no guidance beyond that of Riemann's coordinates; and yet, in this pathless void, Einstein chooses a complicated formula, which has no application to our space, and unerringly deduces the law of gravitation whose manner of action he has not taken the trouble to consider.

But then—'chooses.' On what ground does he choose? There is another way of arriving at the result, and that is—since that result is already known in actual space—to select, adapt, trim, and even

unscientifically constrain mathematical expressions to agree with Newton's law, except in a transcendental space of which we have no conception, and where no test is possible.

A malign intelligence devoid of candour might have adopted this inferior method, and step by step his processes would seem a reflex of Einstein's.

"Einstein asserts that the natural geometry of an empty region is not of so unlimited a kind." Searching for a geometry of a void space without *points de repère* seems to me more futile than Kant's quest, by acknowledgment hopeless, for the "*Ding an sich*", and I renounce at once. Nonsense for nonsense I prefer my negro waiter with his new religion, for although I no more understand him, his words have a more appealing warmth. Into this space, however, Einstein introduces an isolated particle; but M. Le Roux has demonstrated that no general law can be obtained even from two particles.* I will not at present discuss this point, but I assert that since gravitation implies an interaction between masses, to speak of gravitation in a space void except for an isolated particle, is meaningless. Sir Arthur says that the Relativitists are guided by the general idea of the type of solution to be expected of such a particle. Now as the particle itself is not con-

* M. Le Roux has made a deep study of this problem of gravitation, and his manner of obtaining a coordinate system of election is a fine feat of mathematical reasoning. The Relativitists tacitly assume such a system of coordinates, despite their own arguments. This is pointed out both by M. Painlevé and M. Le Roux. It may be remarked also that Carl Neumann, in the *Mathematische Annalen* (1869), in a note replying to a criticism of Clausius, pointed out the absurdity of trying to obtain a law of gravitation, without considering the attractive bodies involved; and Carl Neumann was a follower of Riemann, whose reputation the Relativitists, in their admiration, have so defaced.

cerned with offering any solution, I am here reminded again of my elusiveness of my opponent, especially as "the preliminary argument need not be rigorous."

We now come to the space-time interval, which is also meaningless unless reduced to actual space conditions, and in that case, as we have seen, faulty in expression. This is posited as

$$ds^2 = -dr^2 - r^2 d\theta^2 - r^2 \sin^2 \theta d\varphi^2 + dt^2 \dots (38.11)$$

and as the first four terms represent a distance in the ordinary sense, one asks, why then have we not used from the beginning the lucid conceptions of Lagrange? The answer is to be found in the last term dt^2 , which in a Lagrangian equation would look absurd.

Having obtained the form (38.11) Sir Arthur, standing sponsor for Schwarzschild, proceeds to modify it by giving it a general form; but if the Riemannian coordinates have any value at all it is in their more general representation; why, then, the lame appeal to the classical form already disdainfully discarded by the Relativitists?

However, let that pass. We do know, though precariously, because of the dt^2 , the meaning of (38.11), but in (38.12) since U and W are 'arbitrary functions,' we are now in the realm of conjecture. We simply cannot tell what has been done, but it is permitted to us to suspect a mathematical 'find the lady' trick, and that suspicion is deepened in the next step of the changing of r_1 to r again. The passage beginning "If the functions U, V, W , differ only slightly from unity . . ." is at length revealing. "It is arbitrary whether we choose r or r_1 as its closest representative." Here we come down to the veritable sleight of hand.

That is to say, our wicked, malign magician has come completely into his own; for in a verbiage less obscure even than when he tries to give us genuine science, Einstein virtually says: we must trace our plan on the old Newtonian copy, keeping away from the original so as to give a new version, but keeping close so that we cannot be detected by any available experiment.

That last effort (38. 2) shows the touch of the artist. The logarithms remind me of Fechner, but I do not want to confine Einstein to a narrow category of what the French call *faux esprits*. Einstein says that the gravitational field due to a particle diminishes indefinitely as we go to an infinite distance; and with the proviso that a field due to one particle in space is meaningless, and that he has made no sign of ascertaining the physical conditions involved in gravitation, we may let that pass. He does not make clear why all the $g^{\mu\nu}$ vanish when the suffixes are different, for $g^{\mu\nu}$ is the reciprocal of $g_{\mu\nu}$ which in that case vanishes; and we do not ordinarily write $\frac{1}{0} = 0$.

But let that pass.

The next point to notice is that whereas the condition first proposed was that $G_{\mu\nu} = 0$, we have a particularisation in that the components of the tensor are all equated to 0; but as Einstein has exercised a choice of tensor, at the beginning, his choice may include this condition. Soon afterwards, however, we come to something serious.

“From (38. 61) and (38. 64) we have $\lambda' = \nu'$ ”; this is not rigorous, but at least a solution has been obtained.

He deduces $\lambda = -\nu$. The reasoning is that on integrating $\lambda' = -\nu'$ we find that $\lambda + c = -\nu$, where c is a

constant. Now λ and ν vanish at ∞ , but neither λ nor ν has been determined; not even defined, and it is possible to form a function of r so that, where c is not zero, λ and $\lambda + c$ may vanish at ∞ .

The argument is however faulty for a deeper reason. In the formula (38. 11) $d\theta$ and $d\varphi$ are of the same order as r where r is finite, though not where r is infinite; and dt , though undefined, does not rise to infinity, especially when it is assumed that the action of gravity is instantaneous. At the limit, therefore, where r is infinite, if we follow the method of Euler for determining the asymptotes, we obtain a form $ds^2 = (vr)^2$ where v is undetermined, but is not zero. That is to say no conclusions can be drawn by considerations of the conditions at infinity, in regard to e^λ .

But, waiving for a moment this objection, we arrive finally at a value of e^ν and thence of λ and ν , and in this way we learn for the first time the nature of the functions of r , and finally of the tensor we have 'chosen.'

This solution is due to Schwarzschild, but I have attributed it to Einstein, because he has adopted it, and because, therefore, it is through Schwarzschild's ingenuity that Einstein has known how he must manipulate his original choice to obtain a result as near as possible to that of Newton. In this, however, there is no physical interpretation given to γ except by a further exercise of the tracing on Newton's plan.

The final result in the form of the 'interval' means nothing at all, for it is but a form embroidered on the classical expression for the distance.

Now let us wash our eyes, and reflect sanely a moment to review the matter. The expression for distance is much more familiar than the Riemann-

Christoffel tensor, for it flows by elementary considerations from the Pythagorean proposition, the principle of the Cartesian coordinate, and one of the most frequently used of the transformations of coordinates ; whereas the path by which we arrive at the Riemann-Christoffel tensor leads from the study of quadratic forms to Christoffel's first problem of obtaining the conditions of transformation of these forms ; then on that basis the establishment, by ingenious means, of his first symbol ; then with further modification, the expression of his symbol of the second kind ; then, as a result of curious observations, which are purely formal, of analogies with one of Riemann's results, the discovery of the process of covariant differentiation ;* then the further research as to the result of commutation of the order of differentiations in a further differentiation of these forms ; then the establishment of Ricci's tensor ; then the modification of this into the Riemann-Christoffel tensor ; then the contraction of this ; and then the *choice*, and then the development, the particularisation, and the illicit manipulation of the processes, until finally the preconceived result embroidered on Newton's known result is offered to our admiration. Consider that from the manner in which I have indicated its discovery or invention, no graphic picture could be gained of the import of the Riemann-Christoffel tensor, and consider always that these developments of Riemann had, as he himself declared, no application to our space !

* I am following the order of demonstration now usually given, but it was not that of Christoffel's original paper, where the covariant differentiation did not appear distinctly till its significance was, much later, pointed out by Ricci.

M. Painlevé's criticisms are marked by a scrupulous desire to concede to the theory, as a possibility, all that is not actually disproved. He remarks, however, that the assumption $fr \equiv r$ is not a consequence of the theory of Relativity; it is imposed *approximately* by a first confrontation with astronomic observations. Even then Einstein could not verify his "audacious and speculative" conceptions unless the law of gravitation derived from his doctrine were a "unique law." "It is nothing of the sort, and the same conception applied to a (possible) ds^2 would lead to the inverse conclusion." Einstein cannot conclude that the law is $fr \equiv r$. Let us posit $f = r[1 + \varepsilon(U)]$, where $U = \frac{\mu}{r}$. Einstein 'chooses' $\varepsilon = 0$. That, however, is an assumption, for one could add to ε an arbitrary term of a certain form. The study of the trajectories does not permit a choice of the function of r . Even on the basis of Einstein's first choice M. Painlevé concludes that an 'infinity' of solutions are possible!

M. Le Roux points out that in applying this theory of gravitation no account is taken of perturbations due to other bodies. M. Brillouin says that these are accounted for in the coefficients, the g 's. There is nothing whatever in Einstein's theory where he indicates how the coefficients are adapted to this service; and if they were, since Einstein does not consider any physical reactions in building up his theory, then he must rely on the Newtonian method for giving the proper values to the coefficients.

Weighing all this, is it not clear that here is no veritable work of physical research and mathematical elucidation?

CHAPTER XVIII

WAVE PROPAGATION AND THE MICHELSON-MORLEY EXPERIMENT

The Theory of Wave Propagation. There is a partial differential equation, which, to students of natural philosophy—to use an old and excellent term—recurs again and again like the *leit-motif* of a Wagnerian opera, and even as the trees that whisper round a temple become dear as the temple's self, so with this hieratic form does familiarity breed affection.

The equation is the same in problems on heat-conduction, nature of fluids, electrostatic and electromagnetic potential, vibrations of flexible strings and elastic solids or membranes; and Professor John Perry said in *The Electrician* nearly forty years ago, following, however, on a remark by H. Weber (*Mathematische Annalen*, 1869), “the true correlation of the physical sciences lies in the equation of continuity.

$$\frac{\partial u}{\partial t} = a^2 \left(\frac{\partial^2 u}{\partial x^2} + \frac{\partial^2 u}{\partial y^2} + \frac{\partial^2 u}{\partial z^2} \right).”$$

His words are sage and stimulating to thought, but I do not entirely agree with them; the secret of the correlation of the sciences is contained in the establishment of the Fundamental Processes of the Mind.

The continual reappearance of the equation of continuity is due rather to the constraint we place on the

actual conditions of the physical world to bring them to the limits of our acquaintance with the mathematical instrument. From the days of Euler and d'Alembert, who gave us a solution of the problem of the vibration of an elastic cord, we find that in every stage we are not dealing with nature but with schematic forms which we adapt so as to bring the problem within the scope of our mastery of the calculus. That being said, I find exceedingly profitable the study of a work of M. J. Le Roux, *Relativité Restreinte et Géométrie des Systèmes Ondulatoires*,* by which we gain a clearer view of the meaning of the Lorentz transformation than is possessed by the Relativitists themselves.

M. Le Roux gives the equation of the propagation of waves in a homogeneous and isotropic medium in rectangular coordinates as

$$\frac{\partial^2 V}{\partial x^2} + \frac{\partial^2 V}{\partial y^2} + \frac{\partial^2 V}{\partial z^2} - \frac{1}{c^2} \frac{\partial^2 V}{\partial t^2} = 0.$$

The constant c is the velocity of propagation.

The equation has been integrated, and we find, as a solution in the case of a fixed pole, or source of waves :

$$V = \frac{f(r - ct) + \varphi(r + ct)}{r}.$$

Suppose now that the pole itself is in motion along the axis; the differential equation undergoes a change; but by a change of variables, introducing the transformation of Lorentz, we reduce the equation to its original form, the r being replaced by r' , which is

* The study of this subject brings to our consideration the work of a long series of great thinkers, Euler, d'Alembert, Laplace, Poisson, Fresnel, F. Neumann, C. Neumann, Stelkoff, and in more recent times, Darboux, Bjerknæs, Dini, J. Hadamard and many others ; but the subject is far from being exhausted.

called the modulus of the integral V , and is such that the points, x, y, z , corresponding to a same value of the modulus in the mobile system, are situated on an ellipsoid of revolution having Ox for axis and the mobile pole for centre. This surface is flattened proportionately to the contraction of Lorentz.

If now we take a source of emission of waves and a source of absorption, and if the system of mobile axes be parallel to fixed axes, we will get a surface of inference represented by the equation of the ellipsoid as before. But now if we measure the distances by the radii taken as units, each for its own direction, the axes of the ellipsoid will be measured by the same numbers.

Here is an interpretation of the transformation of Lorentz, for as a consequence the rectilinear distances are measured in units of the radii of the ellipsoid in the directions considered. The Relativitists would not accept this interpretation, they would tell us that the ellipsoid, which represents a physical reality, has been changed into a sphere; and this is the secret of the wonder, and the paradox of the whole doctrine.

The doctrine that the velocity of the luminous undulations represents the maximum of velocity in the universe comes here again under the searchlight. M. Brillouin, the distinguished physicist of the College de France,* declares that this doctrine is based on the supposition that the equations of Lorentz apply to all phenomena, and he points out that this assumption is not a consequence of a scientific exposition but is an "avowal of ignorance in physical science and of mathematical impotence." M. Le Roux shows that it does not

* M. Brillouin, *Propos Sceptiques au Sujet du Principe de Relativité*, Scientia, 1913.

follow even as a consequence of Lorentz's demonstrations. The difficulty arose from the assumption that the velocity of translation of the mobile pole must be inferior to that of light, as otherwise no graphic representation could be obtained of the surface of interference. Why Lorentz hesitated at such an objection as unintelligibility it is not easy to conjecture, except by Pascal's aphorism that the heart has reasons that the reason cannot understand. The ratio of contraction in Lorentz's formula becomes $\sqrt{a^2 - 1}$ where a is the ratio of the velocity of the pole to that of light. The ellipsoid of interference becomes converted into an hyperboloid. Moreover, as Le Roux points out, and as we shall see clearly later, this transformation of Lorentz has nothing specially to do with light, it might be used in regard to any system of waves. He remarks: "Mathematical theories have no mysterious power to influence phenomena. The claim of Einstein to edict a restrictive condition which the laws of nature must obey is difficult to admit."

I quote this not to stand behind the authority of M. Le Roux, but because it expresses what I have again and again shown as an unallowable fault of the Relativitists; and also because the French language permits a certain politeness of correction which I confess I could not unaided have applied; and, if I feel any inferiority here, I solace myself by remembering that M. Le Cornu, M. Brillouin, M. Lallemand, M. Bouasse, M. Le Roux himself and others—eminent French critics of Relativity—give vent at times, in sheer exasperation, to verbiage which makes my own tame language pale its ineffectual fires; while with those who preserve their restraint, Poincaré, Painlevé and Picard, who

speaks of a "rupture of commonsense"—delightful phrase—I seem to feel that their effort of resistance in defence of courtesy is even more ominous than the impetuous sallies of their confrères.

The manner in which the transformation of Lorentz occurs as a particular case of a general scheme of transformation is shown in elegant form by M. Le. Roux. If we have before us the problem of replacing a quadratic differential form by another of the same kind, we must make use of linear transformations with constant coefficients. Let us consider, therefore, such transformations. The equation

$$u^2 - x^2 - y^2 - z^2 = 0$$

may be taken as representing in homogeneous coordinates the sphere of unit radius. In order to transform the expression to that of a new set of coordinates, so as to obtain

$$u'^2 - x'^2 - y'^2 - z'^2 = 0$$

we take for the new system a tetrahedron conjugate, in regard to the sphere, to the first. Of the four summits of this tetrahedron, one, which we will call O' , is interior to the sphere. The polar plane of O' may be designated as $u' = 0$. Consider next a cone, 'imaginary' in this case, with summit O' , circumscribed about the sphere. If the frame $O'x'y'z'$ be tri-rectangular, as well as conjugate in regard to this cone, we take the axis of x , for example, as coincident with the axis of revolution OO' of the cone, and in order to have a reciprocal transformation we make one of the axes of the first system coincident with OO' . So far we have already a particularisation, and the result represents a sort of hyperbolic rotation of the variables x and u ,

and a circular rotation of y and z . If then we leave out of account this circular rotation, we obtain the ordinary form of the Lorentz transformation. The cone itself is represented by the equation

$$(u - a_x)^2 - (1 - a^2)(u^2 - x^2 - y^2 - z^2) = 0,$$

and this reduces to

$$(1 - a^2)(x'^2 + y'^2 + z'^2) = 0.$$

The second factor here is the square of what is called the 'modulus' of the waves with mobile pole. The modulus is the radius of the sphere, which by the application of the coefficients of Lorentz may be reduced to the ellipsoid of interference. The abscissa of the polar plane is denoted by $\frac{1}{a}$. We have therefore a geometrical representation, quite apart from all doctrine of Relativity, and shown in more general form than that of Lorentz, of all the elements of this mode of transformation. They all depend on the point O' .

The transformation of Lorentz may be exhibited in a still more general relation, and at the same time we will see a remarkable 'metric' quite distinct from that of Riemann, for though the Relativitists have misconstrued the meaning of those processes, they have nothing essentially to do with Relativity. The metric is due to Cayley,* but he derives the suggestion from Laguerre,† who in turn uses the system of anharmonic ratios developed by Steiner. We have already seen that if we take a point interior to a sphere as the summit of a conjugate tetrahedron, this summit determines all the elements of the Lorentz transformation.

* Cayley, *A Sixth Memoir on Quantics*, 1859, (Collected works).

† *Sur la Théorie des Foyers*, *Nouvelles Annales de Mathématiques*, t. 72, 1855.

Taking the centre O as the origin of the fundamental system of reference (S), then if we take another point P interior to the sphere, the vector OP will give the measure of the pseudo-velocity of translation in regard to the system (S). The pseudo-velocity may be defined thus :

$$\text{Let} \quad dx^2 + dy^2 + dz^2 = ds^2,$$

and take as definition of the interval $d\sigma^2 = du^2 - ds^2$,

then $\frac{ds}{du}$ is called the pseudo-velocity.

Now, if we consider another point P_1 , the pseudo-velocity which corresponds to this point in regard to (S) will be measured by OP_1 . But if we take P as pole of the system (S') then the pseudo-velocity relative to P_1 will not be measured by PP_1 . Now as the form of $d\sigma^2$ suggests hyperbolic functions, we take

$$du = d\sigma \operatorname{ch}\varphi, \quad ds = d\sigma \operatorname{sh}\varphi, \quad \text{and} \quad \frac{ds}{du} = \operatorname{th}\varphi,$$

the pseudo-velocity. We may make use of this to obviate the difficulty of passing from one system to the other.

$$\operatorname{th}\varphi = \frac{e^{2\phi} - 1}{e^{2\phi} + 1} = \nu \quad (\text{say})$$

therefore
$$\varphi = \frac{1}{2} \log \frac{1 + \nu}{1 - \nu}.$$

This suggests anharmonic functions.

The straight line OP cuts the sphere in two points which we will call M and M' . The anharmonic ratio of the four points M, M', O, P , reduces to $\frac{1 + \nu}{1 - \nu}$.

If we denote this ratio by the symbol $(MM'OP)$, then

$$\varphi = \frac{1}{2} \log (MM'OP).$$

This anharmonic ratio, however, may be applied to

other sets of four points, and if the straight line P_1P_2 cuts the sphere in N, N' , the hyperbolic argument φ of the corresponding transformation will be

$$\varphi = \frac{1}{2} \log (NN'P_1P_2).$$

This expression serves as index of the Cayleyan distance. The pseudo-velocity which corresponds to the argument φ is always $th\varphi$ no matter what pole may be chosen. Thus the Cayleyan metric gives a graphic form for the composition of pseudo-velocities. It has another remarkable quality; the Cayleyan distance of a point in the interior of a sphere to its surface is infinite.

Cayley was not only a mathematician of great calibre, a master in various fields, but he was perfectly sane; he even smiled at the enthusiasm of Sylvester who looked on mathematical relations with the eye of a poet. Hence, when he found that a short distance might be represented by infinity, he regarded that only as a result, in schematic form, of the application of a metrical system that not only put a new instrument of calculation into our hands, but always gave intelligible conclusions. He did not impute paradoxes to nature; he expressly states that the symbols must not be misapplied. But think of the opportunities he missed. Imagine the wonder that he might have produced in Fleet Street, in Shoreditch, and finally, in Oxford, if he had announced that a short distance was really the same as infinity; that a walk down Piccadilly was just the same as a voyage to Sirius, and that by the mere fiddling with mathematical terms he had annihilated time and space! The Relativitists have on their part confused these terms, and they are the phœnixes of the modern world!

The metric of Cayley furnishes, as Felix Klein pointed out, a representation of the non-Euclidean geometry of Lobatschewski, which has certain affinities with the theory of Relativity. Imagine a circle, the fundamental circle C as the base of a hemisphere. Any point P interior to the circle may be taken as the vertical projection of a point P' on the hemisphere, and a chord of the circle will be represented as the projection of a semicircle of the hemisphere. The generating lines of the sphere at P' will project into the imaginary tangents, issuing from P , of the circle. The anharmonic ratio of two straight lines passing through P and the imaginary tangents of P is also that of the corresponding straight lines in the tangent plane of the hemisphere at P' , and the Cayleyan angle of the two straight lines at P is equal to that formed on the sphere by the semicircles of which the straight lines passing through P are the projection.

Consider two points, P_1, P_2 , within the circle. Call the points where the line through P_1, P_2 , cuts the circle, M, M' . Corresponding to the straight line $M'P_1P_2M$ will be a semicircle passing through the points P_1', P_2' , which project into P_1, P_2 . If from the centre of this semicircle we form the radii to P_1', P_2' , and call the angles which these radii make with the line $M'M$, respectively φ_1 and φ_2 ; then the anharmonic ratio ($MM'P_1P_2$) reduces to the square of the ratio of the tangents of $\frac{\varphi_1}{2}$ and $\frac{\varphi_2}{2}$, so that the hyperbolic argument

of the Cayleyan distance is equal to $\log \frac{\tan \frac{\varphi_1}{2}}{\tan \frac{\varphi_2}{2}}$.

If now we consider the straights joining M' to M , to

P_1' , and to P_2' , and then as a fourth straight the tangent to the semicircle at M' , then the anharmonic ratio of this pencil of four straights is equal to that of the four points M, M', P_1', P_2' , of the semicircle; and as, for the same arcs, the angles at the circumference are half of those at the centre, this anharmonic ratio will be

$$\frac{\tan \frac{\varphi_1}{2}}{\tan \frac{\varphi_2}{2}}; \text{ and thus we have } \varphi = \log (MM'P_1'P_2').$$

From the pole of the hemisphere effect a stereographic projection. The semicircles whose plan is perpendicular to the fundamental circle C , will be projected as arcs of circles normal to C . The points M, M' are unaffected, and the projections of P_1', P_2' may be called P_1'', P_2'' . The anharmonic ratio of $(MM'P_1''P_2'')$ on the stereographically projected arc of the circle is equal to that of $(MM'P_1'P_2')$. It is therefore equal to that of the square root of the anharmonic ratio $(MM'P_1P_2)$.

As the angles are unaltered in the stereographic projection, the Euclidean angle of two arcs normal to the fundamental circle is equal to the Cayleyan angle of their chords. Here we have the representation of the fundamental elements of the non-Euclidean geometry.

Finally, consider a point P within the fundamental circle, and P' its representation of the hemisphere. A small circle around P' as pole becomes projected on the plane as an ellipse. The ratio of the axes of this ellipse gives the Lorentz coefficient of contraction for the velocity of translation corresponding to P .

We have now reached a point where we can see clearly that the so-called transformation of Lorentz is but

a special case of a mode of transformation which has much wider applications than the scope of phenomena dealt with by the Relativitists, and that its real meaning makes evident the error of their interpretations and their conclusions, as, for instance, when they take the velocity of light as a maximum constant of nature. We observe also that the transformation gives at times an appearance of concordance to formulae, which in the nature of things refer to different conditions of phenomena; and that to assume an invariant expression for the laws of phenomena without an examination of the physical conditions is unscientific and of course unwarrantable.

What has been said of the interference of waves from two origins applies also to the reflexion of waves at a plain mirror which has a movement of translation in the medium. The plane of the mirror is a plane of stationary interference for the incident wave and the reflected wave. The incident ray, the reflected ray and the 'pseudo-normal' * are in the same plane. The two rays are conjugate harmonics in regard to the pseudo-normal and the line of intersection of their plane with the plane of the mirror. It is therefore the ellipsoid of interference which must be considered in the phenomenon of reflexion. In order that the incident ray may be reflected on itself it must have the direction of the pseudo-normal.

That being so, we may return to the experiments which first suggested the theory of Relativity.

We find many interesting observations, but none

* The pseudo-normal is not perpendicular to the wave front; it is parallel to the diameter conjugate to that front in the ellipsoid of interference.

that bear out the assumptions of this theory. The theory of undulations which has been briefly indicated here might have led us to anticipate a flattening of the surface of interference into an ellipsoid; but Michelson and Morley appear to have found that the surface of interference was a sphere. In this case the modulus r represents a veritable distance, and we have the equation

$$r^2 = (u - u_0)^2.$$

When u is taken as constant this gives the surface of interference as a sphere, as we have noted. When t is taken as constant, the equation represents the wave of progression, and, as the parameter of radiation * u is a linear function of the coordinates, the surface will in general be an ellipse, with the moving pole as focus.

M. Le Roux calls attention to an interesting result, indicated by Henri Poincaré † and found again by M. Ch. E. Guillaume, ‡ and dealt with in another form by M. Sagnac. § It is the wave of progression which gives the law of the variation of velocities relative to propagation. Accordingly we have not the right to conclude that the velocities are the same in all directions. The isotropy of the wave of interference does not imply the isotropy of the wave of progression. The ellipsoidal form of the wave of progression does not allow us to assume that the field of propagation within the limits of the experiment is isotrope. In that field

* In the propagation of waves the parameter of radiation is ct , when the pole is fixed, and for a mobile pole, $c\theta$; θ being $= \beta t - \frac{v x_1}{c^2 \beta}$ where β is $\sqrt{1 - \frac{v^2}{c^2}}$, and $x_1 = x - vt$.

† *Science et Méthode*, p. 239.

‡ *Comptes rendus du Congrès Int. des Math. de Strasbourg*, p. 602.

§ *Comptes rendus de l'Académie des Sciences*, t. 174, 1922, p. 29.

the influence of the earth is evident, and the propagation of light is influenced. "The only legitimate result to be inferred from the experiment is that of the isotropy of the wave of interference."

The whole character and the conditions of the experiments should, however, be submitted to the most searching examination.

The observations were true only within the limit of error of experimentation, and as we have already noted that in 1887 an essential condition of the problem was neglected; and that again in later experiments the figures were varied; and when we reflect also that the velocity of the earth is one ten millionth of that of light, and that the experiments indicated, on the usual interpretation, that there was no difference of one hundred millionth of the velocity of light discernible, we see on what hazardous ground we stand in drawing conclusions.

There are various smaller factors not taken into account. There is also an assumption that the velocity of light is the same in all directions, and there are a host of physical conditions neglected by imputing the diagrammatic form of our geometrical figures to the actual physical structures of nature; as, for instance, the plane surface of the mirror, when we are dealing with the agitation of molecules and those of the atoms of the molecules, fairly comparable in diameter with the wave lengths of light. Various assumptions are made as to the nature of ether, while yet the Relativists deny its existence. If the ether be real, and if, as is probable, it possesses any degree of viscosity, the whole basis of Relativity falls to the ground. No matter what physical constitution or reactions it

possesses, the geometry employed ignores them, as from the very beginning when d'Alembert restricted the physical conditions so as to reduce the difficulties of the problem of undulations. Finally, out of these various possible explanations of the Michelson-Morley experiments, the one that is not feasible is that of the Relativitists, which leads them inevitably to a series of assumptions which run counter both to common sense and the facts presented by natural phenomena.

Michelson himself, who lectured at the University of Paris in 1922, ignored the theory of Relativity, but consistent with the line I have taken from the beginning, I do not quote him on his mere authority.

Nor, though I have frequently cited M. Le Roux, do I put him forward for any other reason than for the rigour of his arguments, and for his demand that the theory of Relativity should win its place, if possible, by virtue of scientific demonstration. Wherever I have referred to M. Le Roux it is because I have been convinced by his reasoning and he has given his conclusions in language so clear and explicit that I have been content to quote him textually, as now I do: "The principle of special Relativity, in the sense of Einstein, constitutes sometimes a redundancy, sometimes an absurdity, according to the domain of application."

CHAPTER XIX

THE ' VERIFICATIONS ' OF RELATIVITY

THE Relativitists offer three great verifications of their theory, and they claim that on this basis the whole system is justified. These verifications are, as I shall show, non-existent. When examined closely, they simply reveal their now familiar method of argument, of adding assumption to assumption, and making the chief virtue of their reasoning a faculty for reaching unwarranted conclusions.

It must be remembered that if the theory has any validity it replaces that of the Galilean constitution of the world, as exemplified by Newton, Laplace, and a thousand other great students of astronomy and physics. Therefore let us suppose for a moment that of these rival theories Relativity was the first to be known, and that it had gained evidence on the basis of three verifications. If then the new theory of the Galilean universe had begun to assert itself, and if, even without the aid of bemusing absurdities, it had obtained favourable consideration among thinking men, what would it have to show by way of verification? It could point out that on the basis of this theory not a star passes the meridian but it constitutes a proof of the rightness of the theory and of its astonishing accuracy, an accuracy more and more impressive

as our instruments become more delicate and reliable. It would point to the fact, that M. Bouasse brings vividly to mind, that if a great book of physics be opened there will be found in it scores of experiments and observations of all sorts accurately verifying the laws enunciated, and many of them owing discovery, or prediction, to the validity of the theory. Here we would have arguments more convincing beyond comparison than the proofs offered by the Relativitists, even if these exhibited veritable scientific credentials. That being so, however, let us see these verifications as the Einsteinists present them. I quote textually from Sir Arthur Eddington's book.*

“ Displacement of the Fraunhofer lines. Consider a number of similar atoms vibrating at different points in the region. Let the atoms be momentarily at rest in our coordinate system (r, θ, φ, t) . The test of similarity of the atoms is that corresponding intervals shall be equal, and accordingly the *interval* of vibration of all the atoms will be the same. Since the atoms are at rest we set $dr, d\theta, d\varphi = 0$ in (38. 8) so that $ds^2 = \gamma dt^2$. Accordingly the *times* of vibration of the differently placed atoms will be inversely proportional to $\sqrt{\gamma}$.

“ Our system of coordinates is a static system, that is to say the $g_{\mu\nu}$ do not change with time. (An arbitrary coordinate system has not generally this property ; and further, when we have to take account of two or more attracting bodies, it is in most cases impossible to find a strictly static system of coordinates.) Taking the observer at rest in the system (r, θ, φ, t) , a wave emitted by one of the atoms will reach him at a certain time dt after it leaves the atom ; and owing to

* *The Mathematical Theory of Relativity*, p. 91 et seq.

the static condition this time-lag remains constant for subsequent waves. Consequently the waves are received at the same time-periods as they are emitted. We are therefore able to compare the time-periods dt of the different atoms, by comparing the periods of the waves received from them, and can verify experimentally their dependence on the value of $\sqrt{\gamma}$ at the place where they were emitted. Naturally the most hopeful test is a comparison of the waves received from a solar and a terrestrial atom whose periods should be in the ratio of 1.00000212 : 1. For the wave-length 4000 \AA , this amounts to a relative displacement of 0.0082 \AA of the respective spectral lines. The verdict of experiment is not yet such as to secure universal assent ; but it is now distinctly more favourable to Einstein's theory than when ' Space, Time and Gravitation ' was written.

" The quantity dt is merely an auxiliary quantity introduced through the equation (38. 8) which defines it. The fact that it is carried to us unchanged by light-waves is not of any physical interest, since it was *defined* in such a way that this must happen. The absolute quantity, ds , the interval of vibration, is not carried to us unchanged, but becomes greatly modified as the waves take their course through the non-Euclidean space-time. It is in transmission through the solar system that the absolute difference is introduced into the waves, which the experiment hopes to detect. The argument refers to similar atoms, and the question remains whether, for example, the hydrogen atom on the sun is truly similar to the hydrogen atom on the earth. Strictly speaking, it cannot be exactly similar, because it is in a different kind of

space-time, in which it would be impossible to make a finite structure exactly similar to ours existing in the space-time near the earth. But if the interval of vibration of the hydrogen atom is modified by the kind of space-time in which it lies, the difference must be dependent on some invariant of the space-time. The simplest invariant which differs at the sun and the earth is the square of the length of the Riemann-Christoffel tensor, viz. :

$$B_{\mu\nu\sigma}^\epsilon B_\epsilon^{\mu\nu\sigma}.$$

“The value of this can be calculated from (38. 8) by the method used in that section for calculating the $G_{\mu\nu}$. The result is $48 \frac{m^2}{r^6}$.

“By consideration of dimensions it seems clear that the proportionate change will be of the order $\frac{\sigma 4m^2}{r^6}$

where σ is the radius of the atom ; there does not seem to be any other length concerned. For a comparison of solar and terrestrial atoms this would be about 10^{-100} . In any case it seems to be impossible to construct from the invariants of space-time, a term which would compensate the predicted shift of the spectral lines, which is proportional to m/r .”

“And that’s why your daughter is dumb,” as the quack doctor of Molière concluded, though his arguments seem to me a model of cohesion and clarity compared with this of Einstein. It may be my own deficiency, and if, dear reader, you have made good sense out of this, I admit that your intellect soars at a range inaccessible to me.

Yet I have not always shrunk before what Carlyle calls ‘tough reading,’ and I have appreciated Byron’s

saying that he liked "something craggy to break my mind on." I have spent two years reading a half-page of Clerk-Maxwell, that is to say I had in the meantime to gain understanding of the subjects—differential equations in general, harmonic functions, quaternions, on which he drew for his exposition. I have spent longer on the differential equations of Sophus Lie in order to search out the secret of his transformations and to get the 'germinal idea' of it all. In all these examples I felt from the first, and the more I studied the stronger became my conviction, that I was dealing with wonderfully fine things wonderfully handled. But here!

From first to last there is no suggestion offered as to what is an atom, how it vibrates, how the vibrations produce a certain influence which we must not here call waves—although Sir Arthur uses the term—because Einstein and his faithful disciples have abolished ether and supplied its place with mathematical formula.

Yet even at this stage all these matters are so important that generations of physicists have successively introduced explanations and hypotheses which are certainly not completely acceptable, since each of them is combated by all the others. The favourite theory of the production of radiation, that of Böhr, is only comprehensible by assumptions that are still more inexplicable than the original problem. The radiations, whatever they are, are accompanied by the Doppler effect, the Zeeman effect, the Stark effect, the Compton effect, the Raman effect, for all of which we are still seeking satisfactory explanations, for again the theories are contradictory. And as these effects are comparatively

recent, and in most cases accidental, discoveries, are we sure that even now we know the last secrets of nature? The Relativitists refuse to answer. They disdain even to consider these mere physical conditions.

The waves travel scores of millions of miles from the sun to the earth, and, through media of which the temperatures vary from 2000° to the zero of space, they come, in the atmosphere of the earth, under the influence of all sorts of forces; yet here, where the vibrations may be changed in the diameter of a test tube, Einstein, and with him Sir Arthur, have not the slightest concern for these physical conditions; they blandly surmise that any changes must be dependent on "some invariant of space-time." That physical incongruities should be solely produced by mathematic forms, even invariants when they vary, already gives one furiously to think; but, as I opine, these are not scientific utterances, they are messianic. Which of the disciples is Sir Arthur's eponym I cannot say, but he resembles Saint Peter in his lapses of fidelity that blend so interestingly with his inexpugnable faith.

The picture of the solitary immobile atom is charming, as compared with the physicists' conception of the myriads in a cubic centimetre with actions and reactions of high velocity, incessant shocks, vibrations and rotations, of which we have nothing but conjectural fancies; and all this, moreover, from those who have discarded abstractions. What is here meant by vibration? There may be vibrations of all sorts in an atom, even according to the generally accepted structure, but it is reassuring to be told that their corresponding intervals are equal, and therefore 'the *interval* of vibration of all the atoms is the same.' If,

for once, he had told us what the interval is, what a vision we would have of the arcana of nature; and that, not by any dependence on tedious science, but by a special kind of incantation.

The time-lag remains constant; here again a picture of the Eddington light atom remaining demurely in place so as to toll out its vibrations, made regular for the needs of his calculations!

“The absolute quantity ds , the interval of vibration, is not carried to us unchanged, but becomes greatly modified as the waves take their course through the non-Euclidean space-time.”

“And now,” said Madame Steinheil to the judge who was infatuated with his own reasoning, “you know so much about it, perhaps you will tell us who was the murderer.”

I confess I should not have known, without Sir Arthur's guidance, that the ds had become so changed; but considering that it passed through the mysteries of 'space-time,' we must be glad to find it arrive at all. After all, we only know our own Universe, and little of that, and it is in that Universe that we have been able to notice that there has been a shift of the spectral lines.

And to think that all this aberration was produced in the darkness of the unknowable, and by the action of a misbegotten, or at least, a miscalled invariant!

If we come to the domain of physical facts, ascertained by experiment and observation, we find that different results are obtained according as the light is taken from the limb or the centre of the sun. The lines themselves are not stable in the spectrum; they change place according to the gas employed, or in the same gas. We are dealing with phenomena not

only unexplained but not even well defined. The Relativitists treat these facts with disdain.

To criticise this 'verification' in detail would invest with undue gravity something that is essentially ridiculous.

In the use of the Riemannian metric I am reminded of a story of Volta in his later days. A visitor to the house where he lived in retirement told him of some new triumph of the Voltaic pile. "Yes," replied the old gentleman contentedly, "nothing surprises me about it—there is nothing it cannot do."

But Riemann's metric is even more wonderful. One of the subjects of study to which he devoted himself just before his premature death was that of the mechanism and function of the ear, anatomically and physiologically, and in regard to the vibrations that affect it. Yet why did he give himself so much trouble, when he might simply have whipped out his own metric and played appropriate variations on that? But, you cry, that is sheer absurdity; the Riemannian metric had nothing to do with the matter!*

I agree, but I assert in return that in regard to the function of an organ which is there in concrete form and all completeness before us, which we can examine and measure, there is no greater absurdity to use this metric than to take it to decide questions of still greater complexity where the structures are hidden, the conditions extraordinarily complex, and our scanty

* Riemann in 1860 dealt with a theory of undulations ("On the propagation of plane air vibrations") in which the problems, though difficult, are incomparably simpler than that of the shift of the Fraunhofer lines; but it never entered his head to depend on the Riemannian metric. Had he done so the high esteem in which he is held as a physicist as well as mathematician would have suffered a momentous drop.

knowledge is eked out by hazardous guesses and arbitrary assumptions ; and to assert that in the renderings of this metric, which Riemann never dreamt of so applying, we can trace with marvellous minutiae the course of undulations, through a fictive medium, modified through millions of miles by physical agencies, never discoverable by this metric, and of which we know so little.

“ But,” reply the faithful, for Relativity must become a religion, or nothing, “ what Riemann never attempted, Einstein has done ; is he not greater than Riemann ? ”

“ Greater than Riemann ? He is greater than Cagliostro ! ”

The Deviation of Light. With regard to the deviation of light passing near the sun, remarks similar to those in regard to the Fraunhofer lines apply. The problem is one of physical conditions, and Einstein makes no attempt to assess these or even to investigate their existence. If the corpuscular theory were accepted, an explanation might be sought in the force of gravity, but that force would be insufficient to produce the effect. What would be the effect of gravity in the ether conveying the undulations we do not know, for all data concerning the ether are woefully deficient. We may assume, however, that gravity has no hold over mathematical formulae, and these, for Einstein, take the place of the ether. In this case, the grounds for deviation really disappear in a Relativist world, except, of course, for those tenebrous happenings behind the veil of ‘ space-time,’ which become, even though vaguely, known to us by the searchings of the Riemann metric.

But I will not delay too long in argument, for the pretended verification is illusory. The results of observations are shown on a chart by a series of dots, and by tracing connexions between these dots it is possible to obtain a 'curve' from which the law of deviation is inferred. But the actual charts in that case, if the observations be numerous enough, show only an irregular group of dots, through which, if it be possible to draw a curve that seems to confirm the theory of Relativity, it is equally possible to draw a curve which runs counter to the theory. Neither curve has any justification; in fact the observations yield nothing of value in this respect; the method is altogether fallacious. M. Danjon, the well-known French physicist, who observed the eclipse, and took measures, said that the observers were unable to determine the scale of their charts, and that the coefficient of the Einstein effect must be considered as unknown. The corrections of the scale of different missions varied largely, and the instrumental errors were large.

Finally, I say, what to some may at first appear to be an enormity, although it is entirely scientific: this expression of opinion would be unmoved even if the observations gave a much more convincing result in favour of Einstein; for the very manner of his reasoning is such that no conclusions could be drawn as to the rightness of the theory.

In other words, it is possible to produce a correct result by erroneous deductions, as for instance, in a simple problem of algebra, where one fault may balance another. In all the sciences we find examples of this. The truth of Darwin's theory does not imply that the arguments, such, for instance, as those of Sir Arthur

Keith as President of the British Association, are necessarily cogent ; they read to me like an historical recital, such as Cuvier, who was a ' Fundamentalist,' might have accepted ; whereas what we require in science is a closely-reasoned, cogent system of arguments, exhibiting step by step causal relations. That is lacking in much of our orthodox science ; in Relativity its presence would be fatal.

The Perihelion of Mercury. One of the great triumphs of the theory of Relativity is, according to the assertions of the disciples, the explanation of the movement of the perihelion of Mercury. There are two facts, however, to be noted at the threshold of the inquiry, the first being that the discrepancy between observation and calculation on the Newtonian theory is known accurately, and the second is that Einstein's instrument of rectification is the theory of gravitation, of which we have already shown the unwarrantable assumptions, the deficiencies, and the fatal lack of rigour and precision in the actual mathematical demonstrations. Einstein knows in advance the result he aims at, and as he ' chooses ' the formula that suits him, and conducts the argument *ad hoc* to secure a fairly close concordance with observation, it is not surprising that he appears to have accomplished his object. The so-called concordance is, however, illusory, and the falsity of his method would be still more evident had he reached apparently still greater precision. A little explanation will make this clear.

The secular advancement of the perihelion of Mercury is found by observation to be 574 seconds. The theory of Newton, when the known perturbations are taken into account, is sufficient to explain the apparent

discrepancy up to a limit of 536 seconds. That being so, there is a gap of 38 seconds to be accounted for. The calculations of Schwarzschild based on theory of Relativity, give a secular advancement of 42.9 seconds. But that theory, which, in dealing with physical phenomena, shows a sovereign disregard for physical conditions, has not taken the perturbations into account, and so, instead of furnishing a complete explanation, it leaves unaccounted for a residue of 531 seconds. It is true that the Relativitists may claim that the perturbations are accounted for in the coefficients; but as they offer no grounds whatever on which these can be determined, we must conclude that they simply take these from the Newtonian astronomers, or ignore them altogether. I adopt here also the conclusions of M. Le Roux, who says, "the theory of Einstein, as it stands, does not allow of any explanation of prediction, even of the roughest kind, of approximation of the secular movement of the perihelion of Mercury."

The closer, however, we look to the actual physical facts, the less tenable seem the claims of the Relativitists, even if, for the sake of argument, we pass over the errors of their calculations. In the first place, even with the Newtonians we are not on any sure ground for obtaining accurate data. I learn from M. Esclançon, the Director of the Observatory of Paris,* that Mercury is a 'difficult planet' for astronomers. All the observations on which the discrepancies arise are old, and the observations taken at different times of the day are not consistent.

* Cf. also "Les Preuves Astronomiques de la Relativité," *Le Mois*, Décembre 1931.

Next with regard to the perturbations, although the consideration of the known causes gives a far closer approximation than obtained by the Relativitists, yet neither the observations, nor the accepted data, nor the mathematics employed, are adequate. Since these observations, on which the figures for the advancement of the perihelion are based, new discoveries, and new methods of precision have been brought to light.*

The famous Danish physicist, Böhr, made the suggestion, as I heard from Professor Svante Arrhenius, that the discrepancy in regard to Mercury would be reduced if additional terms were taken in the mathematical series utilised ; that aspect of the matter has not been definitely dealt with, but it is now being studied.

Further, the Relativist theory and the mode of calculation when applied to Venus yield results much less precise than that of the classic system.

Reviewing, therefore, the whole series of the ' verifications ' claimed for Relativity, we find that a close examination of each in turn forces us to conclusions entirely unfavourable to the theory.

* The planet Pluto has recently had its orbit better determined by new methods by T. Banachiewicz. (*P. S. B.*, Sept. 1930.)

CHAPTER XX

THE SEARCH FOR THE GERMINAL IDEA

THE proof of essential inconsistency in any mode of reasoning is of course sufficient to destroy its value, and so is the demonstration of inconsistency with the facts of nature. We have met with both cases in the examination of the theory of Relativity ; but in order to appreciate a work, whether of science or literature, it is well to pierce down to the "germinal idea," whence it has sprung.

What is the germinal idea in Relativity? We cannot mark the chronological sequence of a multitude of suggestions that have arisen in connexion with this doctrine, but the germinal idea will be found to be that of Simultaneity, particularly in the observation and record of phenomena. Einstein in his work as a physicist has been continually brought in contact with optical and electro-magnetic phenomena, and he appears to think of nature almost exclusively in these terms. The "transformation of Lorentz" has played an important part in the discussion of Maxwell's theories, as we have seen. We have also seen later that this use of such a transformation is only a particular case of a form that has much wider applications, and that it is only a mode of mathematical expression without objective reality.

The idea of reducing all our observations to simultaneity is at first sight attractive ; for it would appear to lead to a doctrine of invariance of natural laws unembarrassed by the structural forms of the measurements, the system of coordinates, that we set up. But phenomena in nature, except in experimental cases, or otherwise under artificial conditions, do not appear simultaneously. Further, though in a large scope of things and with clear views of the causes of phenomena, we gradually reach to a conception of the correlation of the sciences, yet the invariances that we would ultimately find are not the formal invariance of expression of our laws. Einstein seeks to make the expressions invariant in form. The one remark of M. Painlevé in discussing the Relativist theory of gravitation is sufficient to show that this is illusory. Even if it were not, invariance should be demonstrated, not declared by edict, for edicts though impressive are not science.

Associated with the idea of simultaneity is that of absolute reliability of measure, and for this purpose Einstein has brought in his rods and clocks. I have shown that the assumption of any absolute precision rests on ignorance of the actual conditions of measurements by rods or clocks. Another idea associated with simultaneity is that of freeing the man of science from what are called, with an implication not justified, abstractions. Here too I have pointed out that this idea of Einstein's reveals a profound ignorance of the structural form of all reasoning, as clearly shown in the analysis of the Fundamental Processes of the Mind, for no reasoning is possible without abstraction. His own rods and clocks are 'ideal,' that is to say, abstractions, and of course he uses forms of abstraction at every turn.

The demand for simultaneity at the place of observation, in regard to phenomena being impracticable, Einstein seeks for a definition, by means of reflected beams of light and clocks, verifiable at the place of observation, both in the case of a system at rest and of a moving system. Here at the very outset he makes assumptions which are far from the 'absolute' character of his devices for precision.

Here also we find a false argument deeply hidden which vitiates the remainder of the work, and which might be said to be the crux of the doctrine of 'special Relativity.' It is said that of three systems *A*, *B*, *C*, *A* and *B* may be taken as in movement relatively to each other, though both are at rest in regard to *C*. The source of error is not immediately evident; but analysis brings this to light: The system *C* is space. If *C* were a material system, or if space were occupied by something, if not very tangible, yet real such as ether, the fallacy would be apparent; but Einstein, by another assumption, completely unwarranted, disposes of the ether and puts nothing in its place; therefore he eliminates all points of reference that would allow us to speak of relative movement between *C* and *A*, or *C* and *B*. I have examined this argument minutely, even to the degree of tedium; for of all the weak links of his theory, it is this which allows us to point out definitely the flaw.

Never once in the course of my study of this doctrine did I feel that I was dealing with a great mind working out a valid theory, which might at times seem obscure because the subject was recondite and the thoughts so subtle that a delicate acumen and concentrated attention were necessary. Something of the kind I have

often found in reading Locke, or Hume, Herbert Spencer or Alexander Bain, amongst philosophers; or Ampère, or Fresnel, or Clerk-Maxwell amongst physicists; or Gauss or Abel, or Galois, or Hamilton, or Riemann, or Sophus Lie, amongst mathematicians; but with Einstein I have had always a suggestion of an acute and lively mind, prone like the Sophists of old to paradoxes which had no real profundity.

For that reason it was that, as in the case of Achilles and the Tortoise, I was not content with the Diogenes-like refutation, definite though it was; but I was resolved to pursue to the end the analysis that would enable me to see also the exact nature of the false step involved. Here then, in this elucidation of his meaning of simultaneity, it is to be found. Definitely, though some of his disciples deny it, Einstein asserts that a velocity as between two bodies may remain 'invariant' even when one of the bodies receives a new impulse towards the other. I have called this an absurdity, and if the term seems harsh and violent in a scientific discussion, I plead that in certain cases it is unscientific not to indicate an absurdity.

Einstein instead of recognising this absurdity makes play again with his factor of Lorentz, and his followers seem content.

The next step, though most of his disciples and his critics regard it as the actual beginning of the theory, is the explanation of the Michelson-Morley experiment. Einstein accepts a certain interpretation of the result which again leads him to a series of unwarrantable assumptions to escape manifest contradictions of nature; though he is already fortified against such a contingency by his conclusions in regard to simultaneity.

Here again we find an essential link faulty, for one weak link does not exclude another. Henri Poincaré and Emile Picard—and I cite them only because I adopt their reasons—say that if his interpretation of the experiment be wrong, the whole doctrine of Relativity falls to the ground. There is at least another explanation, in no disaccord with science, which explains the experiment without paradox. I have entered into this question at some length, taking into consideration various physical conditions and also the mathematical exposition of the undulations of the light-bearing medium. Part of these considerations I have taken from a work, which might appropriately be called *illuminating*, of M. Le Roux of the University of Rennes, who in his mathematical side, in other directions also, has riddled, even ridiculed, the Relativist doctrine.

Einstein escapes from all this by the transformation of Lorentz, and again his disciples are content. Some of them have proved themselves Ruperts of science, for they have outrun the master. Thus Professor Fitzgerald of Dublin, endorsed by Sir Oliver Lodge, imagined that bodies were contracted in the direction of their motion, though the acceptance of this explanation renders Einstein's reasonings futile.

So far we have dealt mainly with special 'Relativity.' Generalised Relativity is the flower in full bloom. We have here a non-Euclidean world invented; and the principal, unwitting artisan of this edifice is Riemann, a mathematician of genius, who gave a generalisation of the method of the Cartesian coordinates. These were only mathematical expressions, and Riemann says expressly, that, though he uses words derived from the real 'Euclidean' space,

he does so only by a conventional stretching of the terms of expression. In one of his memoirs, however, he spoke of space being limitless but finite, and the Einsteinists, while misapprehending the meaning, have seized on the suggestion with joy.

Hence we have had that extraordinary flood of talk of fourth and other dimensions, curved space, the speed-limit imposed on the Universe, the interchange of mass and velocity, and finally the coalescence of time and space. I have put these to a simple but crucial test; I have analysed down to the elements of which these concepts are composed, and found them not concordant. In the examination of the arguments I have been compelled to repeat myself, even to wearisomeness, because the Relativitists repeat themselves even to exasperation.

When I have looked at this world of so-called science I have felt like Figaro looking at the world of society, I must laugh so as not to weep. Bernard Shaw, it is true, has chosen a midway path, for he extols Einstein to the skies; but as I have not found that he has made any study of these matters at all, and as I have my own measure of value of philosophy, I laughed the more.

On the popular side of this doctrine laughter has been my main solace.

We have now reviewed the doctrine of Relativity in regard to the three branches of sciences on which it rests, psychology, physics, mathematics, while taking into account necessarily that stronger basis of support, the curiosity or admiration of the people who least understand its principles.

Einstein sought his point of departure in what he regarded as essential—and in this I agree with him—a

profound study of the philosophical, or psychological, bases of our accepted ideas. In this search, however, he showed neither originality nor philosophic acumen ; he simply went to the traditional teachings of Kant, and accepted them without investigation or criticism. Therefore I have been at pains to show that the psychology of Kant has no scientific warrant, and that Kant himself never used it as a foundation for developing his system. I am in agreement as to the necessity of establishing the Fundamental Processes of the Mind, and I have indicated modes of analysis more deeply searching, more rigorous in reason, and more fertile than those of Kant. By dealing with complex matters, not by introducing further complexities, but by reducing them to the elemental forms of which they are composed, and from that foundation, carrying out the argument with constant care for rigour, I have indicated the use of this method as an instrument of analysis, and incidentally shown the absurdities of certain cardinal notions of Einstein and Minkowski.

In the physical world, I have insisted on that appeal to Nature which is the animating spirit of science, and we have been able to observe that Relativity seems plausible only in proportion as it ignores that appeal to nature.

In the domain of mathematics I have insisted again on the necessity of complete rigour in argument. We have seen that in the whole course of the development of that wonderful apparatus from the Greeks to Descartes, and from Descartes to our own time, there has been no inspiration obtained from anything associated with this doctrine of Relativity. Einstein and his disciples have added nothing to the power of the

mathematical instrument they use, and the principal source of their achievements, or their fame in this sphere, has been the faculty of misconstruing mathematical expressions and attempting to give an objective reality to what was merely the statement of mathematical relations.

Finally, the 'verifications' of the theory have been examined in turn, and here the closer the criticism, the more baseless have seemed the claims.

As to the popular fame and mere wonder of 'sensationalism,' I confess that I am less sure of myself, for the instrument on which I rely is, after all, that weak—in comparison with the powers of ignorance—and not greatly honoured, LAMP OF REASON.

Yet, as I cast my eye over the whole course of science, I behold instances of false science, even more pretentious and popular than that of Einstein, gradually fading into ineptitude under the searchlight; and I have no doubt that there will arise a new generation who will look with a wonder and amazement, deeper than now accompany Einstein, at our galaxy of thinkers, men of science, popular critics, authoritative professors, and witty dramatists, who have been satisfied to waive their common sense in view of Einstein's absurdities. Then to these will succeed another generation, whose interest will be that of a detached and half-amused contemplation, and in the limbo of forgotten philosophies they may search for the cenotaph of Relativity.

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