tional experiments, such as the work done by Madame Montessori and the founders of the Little Commonwealth.

(6) The *hygiene and technique of mental work* is itself being based upon scientific investigation. Of the numerous problems in the conditions and character of mental work generally, two deserve special mention—fatigue and the economy and technique of learning.

But of all the results of educational psychology,

perhaps the most valuable is the slow but progressive inculcation of the whole teaching profession with a scientific spirit in their work, and a scientific attitude towards their pupils and their problems. Matter taught and teaching methods are no longer exclusively determined by mere tradition or mere opinion. They are being based more and more upon impartial observation, careful records, and statistical analysis—often assisted by laboratory technique—of the actual behaviour of individual children.

Popular Relativity and the Velocity of Light.¹

By SIR OLIVER LODGE, F.R.S.

IN using the phrase "popular relativity" I indicate that what I am criticising is not Einstein's equations—which seem to have justified themselves by results—but some of the modes of interpreting them in ordinary language. Especially do I attack that proposition which asserts that to every observer the velocity of light will not only be constant in reality, but will also superficially appear constant even when he ignores his own motion through the light-conveying medium—a proposition or postulate or axiom which has been shown to lead to curious and, as I think, illegitimate complications, threatening to land physicists in regions to which they have no right of entry, and tempting them to interfere with metaphysical abstractions beyond their proper ken.

Not that a physicist's proper ken is limited to what he immediately observes; he is entitled, and indeed required, to interpret appearances rationally by taking into account every relevant adventitious circumstance, including complications due to his own unobserved, and perhaps unobservable, travel through space.

In a relativity discussion at the Physical Society recently a member is reported to have asked the pertinent question: "Does an observer merely observe, or does he think as well?" If he thinks, I urge that he can allow for changes in his measuring instruments and any other consequences of possible motion, and can refrain from making deductions about space and time on the strength of experiments on matter.

He will know that his senses are material senses, and that all his experiments are made ultimately by their aid. He will know that he can only experiment even on the æther of space indirectly by means of matter, for he has no other means of getting a grip on it. Possibly he may be unable to grip it even thus, but matter gives him his only chance; he certainly cannot experiment on abstractions like space and time.

On the basis of material experiments he may be able to make deductions or draw inferences about the æther, because that certainly has some inter-relations with matter; but it is probably illegitimate, on the basis of material experiments, to make deductions about space and time at all; they are not likely to be affected by anything that matter can do, and it is only matter with which we can directly deal.

The relation between space and time that represents the velocity of light gives us directly one property of the æther, viz. the product of its electric and magnetic constants, both of which separately are at present unknown. Every student who accepts the æther of space as a reality is probably ready to admit that the velocity of light through free æther is an absolute

constant, not dependent on anything that either the observer or the source is doing, has done, or may do.

But this admission has been erected into a fetish by the theory of relativity, at least when expressed in ordinary words, and is interpreted as requiring that to every observer, whatever he may be doing, the velocity of light in every direction will *appear* the same.

That is not only a different, it is a contradictory, proposition. Given the constancy of the real velocity of light—if an observer travel to meet it, it must appear to arrive more quickly than if he travel away from it, provided he has any means of making the observation at all. He may be unable to make the observation, but suppose he can make it, say by the aid of Jupiter's satellites, and detected a discrepancy, he need not infer any real change in the velocity of light; because, if he thinks, he can attribute any observed difference to his own motion, and thereby emerge with clear and simple views. If he sets out with the gratuitous notion that he can never become aware of his own motion, or that his own motion has no meaning, he will indeed encounter a puzzling universe, and will presently long for a Copernicus to unravel the subjective complexities of observation.

But it may well be extremely difficult for an observer to measure the velocity of light through the æther except with the aid of some return signal which the æther likewise has to transmit in the opposite direction; and in that case he may find that the to-and-fro pair of journeys take exactly the same time in every direction.

This, as everyone knows, has been done for a to-and-fro journey of a beam of light. And the timing is exact, not only to the first order of small quantities, as might readily be expected, but to the second order also—an exactitude which, if rigid unchangeable materials could be used, would not be expected, and ought not to occur. But if the dimensions of the material object used as the foundation-stone of his apparatus are subject to change by reason of motion, and if the changes are in accordance with the electrical theory of matter, as suggested by FitzGerald and elaborated by Lorentz, then everything becomes clear again until we come to astronomical and gravitational applications, and the precisely negative result of Michelson and Morley is precisely explained.

A mathematical doctrine of relativity may be based upon this experimental result, and may be convenient for reasoning purposes, but no such doctrine is required by the facts. The facts are patient of the doctrine; they do not compel it, nor do they justify it. Any comprehensive mathematical expression is liable to permit other modes of interpretation, as well as the simplest and truest or the one most directly applicable to the problem in hand. It is devised to cover one set of facts, but in its generality it is apt

¹ The substance of this controversial note was communicated to Section A of the British Association at the Centennial Conference, 1917.

to cover more. Why, then, proceed to build up on an equation an elaborate metaphysical structure? And, especially, why imagine that the success of the Einstein equation proves the observed velocity of light to be the same whatever the motion of the observer? If the observer thinks, and if he is aware of the FitzGerald-Lorentz contraction, he will know that such a proposition is not true; he will know that the velocity of light is not equal in all directions in a relatively drifting medium, that the wave-front is not concentric to the observer, and that the Michelson experiment gives no proof of anything of the kind.

The uniformity of the æther makes the obtaining of positive results difficult; there seem to be always compensations. Some day we may be able to evade this experimental difficulty, but meanwhile, if we

choose to make the supposition that motion of the observer can never have any directly observable effect, or that one set of axes of reference is necessarily equivalent to every other and indistinguishable by any kind of superficial observation, then we seem to be in accord with present experience. From that supposition definite consequences can, with adequate skill, be deduced, and the deductions have been subjected to successful verification.

But if on the strength of that remarkable achievement some enthusiasts proceed to formulate propositions which by ignoring the motion of the observer and all its consequences complicate the rest of the universe unduly, then, however much we may admire their skill and ability, I ask whether they ought not to be regarded as Bolsheviks and pulled up.

Emil Fischer's Contributions to Organic Chemistry.¹

By DR. M. O. FORSTER, F.R.S.

EMIL FISCHER was born on October 9, 1852, at Euskirchen, and his death on July 14, 1919, occurred at a time when every element of constructive and harmonising influence was most sorely needed. Since 1892, when he succeeded von Hofmann, he had fulfilled the duties of professor and director of the chemical institute in the University of Berlin with increasing distinction. Physically commanding, his authority rested on the solid foundation of natural dignity. The brisk, upright carriage marked the man of action; the glowing eyes revealed his attitude of constant, keen inquiry; it was impossible to escape his contagious enthusiasm.

Fischer addressed himself to organic chemical research at the opening of its brightest epoch. Having described the preparation of phenylhydrazine in 1875, he devoted many succeeding years to developing the transformations of that remarkable substance. During this period he also collaborated with his cousin, Otto Fischer, in elucidating the constitution of rosaniline bases, their first joint paper appearing in 1876. It is noteworthy that, in spite of his early interest in the chemistry of these and other colouring matters, and notwithstanding his association with von Baeyer, beginning in Strasbourg and continuing until he left Munich to occupy the chair of chemistry at Erlangen in 1882, he nevertheless resisted the temptation to succeed Caro as director of research in the Badische factory, although at this time (1883) the colour industry was in the early flush of its active growth.

When reviewed as a chapter which is closed, Fischer's work must be regarded as having established upon a firm basis the fundamental science of biochemistry. The assimilation of carbon dioxide and water by plants, the variety and complexity of saccharide molecules proceeding therefrom, the degradation of the proteins, the probable course of their synthesis from amino-acids, and the power of assemblage or of disruption, exerted by enzymes on all these building materials of the animal and vegetable kingdoms are subjects which Fischer not merely illuminated, but was the first to place in coherent arrangement and intelligible sequence. Recognition of the fact that all this was accomplished, not by revolutionary processes or theories, but by skilful development of the thoughts and operations expanded by Liebig, von Hofmann, Pasteur, and von Baeyer, is perhaps the highest tribute which can be paid to his genius.

Fischer's association with the branch of chemistry

which first brought him fame began in 1884, when he discovered phenylglucosazone, produced from glucose, fructose, and mannose by the action of phenylhydrazine. At that time only two aldohexoses (glucose and galactose) and two ketohexoses (fructose and sorbose) were known and recognised as straight-chain pentahydroxy-derivatives. According to the requirements of van't Hoff's theory, a pentahydroxy-aldehyde of this class, in which five carbon atoms are each associated with one hydroxyl group, should appear in sixteen stereoisomeric forms, eight of these being enantiomorphs of the remainder. The bare statement that Fischer and his collaborators elucidated the configuration of twelve such isomerides, most of which they synthesised for that purpose, although perhaps an accurate summary of his opening achievement, conveys but a nebulous impression of the character and amount of the labour involved. Moreover, his discovery of γ -methylglucoside in 1914, and the consequent recognition of cyclic relations distinct from that occurring in α - and β -glucose, have opened the way to a multitude of contingent isomerides, those of *D*-glucose alone numbering ten. Thus Fischer not only elaborated his own sugar chemistry, but also added to this the foundation of a new carbohydrate classification.

The directive influence on Fischer's work in this field was the discovery, in association with Tafel in 1887, of α - and β -acrose. The former sugar he identified with *dl*-fructose, whilst β -acrose is now recognised as *dl*-sorbose. The above-mentioned synthetic operations, and many others connected with pentoses, tetroses, and artificial sugars containing more than six atoms of carbon, were effected by means of the cyanohydrin reaction, Pasteur's method of separating optical antipodes, and the discovery that when a monobasic sugar-acid is heated with quinoline at 140° C. the configuration of the carbon atom adjacent to the carboxyl group becomes epimerised (1890).

One of the most remarkable achievements in a series unsurpassed by any organic chemist was Fischer's synthesis of the principal constituent of Chinese tannin. In 1852 Strecker had shown that gall-nut tannin is a compound of grape-sugar and gallic acid, but latterly the conclusion had become discredited, and tannin was regarded as consisting mainly of digallic acid. This was synthesised in 1908 by Fischer and found to be crystalline, although astringent, and in 1912 he showed that the principal constituent of Chinese tannin does give glucose on hydrolysis. By a series of complex synthetic opera-

¹ Synopsis of the Emil Fischer Memorial Lecture delivered before the Chemical Society on October 28.