

It is probable, indeed, that in telegraphing from one suburb to another the message has to be oftener retransmitted than in going from the City of London to India or America, because a direct transmission from any one part of London to another would involve almost an infinite number of line-wires in all directions. For this reason there must be a limit to the applicability of the Electric Telegraphs in populous districts, and it behoves us to examine whether another agent may not be preferable in dealing with a traffic of this description. The pneumatic tube seems to be well adapted to these circumstances, and having been first applied for short distances by Latimer Clark, and subsequently modified and extended by others, it will fall within the province of our society to examine fully into this and kindred methods that may be devised for effecting rapid interchange of intelligence in towns.

### THE BRITISH COAL-FIELDS

ONE distinguished geologist, at least, disbelieves in the speedy exhaustion of our coal-measures, so frequently predicted of late. At the annual meeting of the Dudley and Midland Geological and Scientific Society, Prof. Ramsay delivered an address on the existence of coal beneath the New Red and Permian strata, in the course of which he observed that for fifteen years he had been preparing to attack this subject, but it was not until he became a member of the Royal Coal Commission he had given it a really searching consideration. There could be no doubt that the various coal-fields of the Northern and Midland districts once formed one great coal-field, but had been separated by extensive denudation. Another great coal-field was formed by the now distinct fields of Devonshire, South Wales, Somersetshire, and the Forest of Dean. Between these two great divisions, the north and the south, there was no connection formed by the coal-measures, the poorer measures possibly having been deposited there, but not the rich deep ones in the carboniferous era. Referring more especially to the Midland district, he thought it highly probable that coal-measures would be found to exist between the present boundary of the South Staffordshire district and the Forest of Wyre; but it was questionable whether it would be of a workable depth. On the west side of the South Staffordshire boundary, in the direction of Bridgenorth, Shropshire, he also believed coal to exist beneath the Permian strata, at a depth of 1,500 feet, or possibly more in some places. At the north of the South Staffordshire boundary, a line drawn from Wyrley right across to the Shropshire district would, he believed, include some valuable coal-beds, a considerable part, but not all, of which would be at a workable depth. He entertained no doubt that the coal-measures were continuous between the South Staffordshire and Shropshire districts, which, although in some places disturbed by denudations, might, throughout the greater part of the area, be profitably worked. In the North Warwickshire coal-field were found, in the direction of the Staffordshire boundary, five beds of coal, which gradually amalgamated, until on nearing Coventry they formed only two measures. The shale and sandstone were split up in like manner. These features constituted most important evidence in support of the theory that the Warwickshire, Staffordshire, and Shropshire districts were united by continuous coal-measures, the peculiarities referred to in the coal, shale, and sandstone strata being identical in all three districts. In that theory Prof. Ramsay was a firm believer. From Warwickshire to the south end of the South Staffordshire boundary, there was, he believed, coal, but not profitable. Towards the northern end of the South Staffordshire boundary, however, a line drawn from Coventry would include rich and valuable coal-measures. Between Staffordshire and Leicestershire the

measures were also, he believed, continuous. From Wales to the Forest of Wyre there was profitable ground; but from Wyre on to Charnwood Forest, and east of that, there were no coals of value. The speaker expressed opinions equally assuring as to the presence of coal under the area lying between the north of the South Staffordshire boundary and the mountain limestones of Derbyshire. In one part of that district—viz., north-west of Cannock Chase—Prof. Ramsay said he should not feel the slightest hesitation in recommending a search for coal; and his belief in the presence of coal at a workable depth in the neighbourhood of Uttoxeter was equally strong. Now, supposing that his calculations were only approximately correct, the result would be surprising. It would amount to this—the coal now reckoned as available in the South Staffordshire and Shropshire districts was, in round numbers, 3,201,000,000 tons. If his belief were a true one, this supply would be further augmented by 10,000,000,000 tons. In Warwickshire the proved coal-measures are estimated to yield 458,000,000 tons, and the measures he believed to exist in addition would be 2,494,000,000, or five times more than the present estimate. The Leicestershire field was calculated to possess 836,000,000 tons, and this would be supplemented to the extent of 1,790,000,000. What was the case in regard to these districts was, he believed, equally applicable to many other parts of Great Britain. The South Wales, Forest of Dean, Bristol and Somerset districts were exceptions to this rule, the coal there lying in basins caused by denudations, the surrounding measures being destroyed. In the Midland districts these small basins are not found, the whole forming one great basin. Lancashire, Derby, and the Yorkshire coal-fields were, however, subdivided by the process of denudation. Still, he had no hesitation in believing that the estuary of the Dee and the Mersey have lying between them beds of coal, although probably at too great a depth to be of practical value.

### MR. TODHUNTER ON THE ARC OF THE MERIDIAN MEASURED IN LAPLAND

MR. TODHUNTER has forwarded us a reprint from the "Transactions of the Cambridge Philosophical Society," in which he discusses the observations made in connection with the measurement of the arc of the meridian in Lapland in the last century. He states that having recently had occasion to study the details of the two measurements of the arc, he has been surprised to find that the accounts of these operations, although written by very distinguished astronomers, contain numerous and serious errors. We must refer our readers to the memoir itself for a complete account of the various points raised, for it is too long for adequate notice in the space at our disposal. A curious point, however, is raised as to the effect of theory upon observation in a paragraph which we quote *in extenso* :—

"It would be a curious subject of speculation whether the theoretical opinions of persons engaged in geodetical surveys could have exercised any influence on their observations; I mean of course unconsciously, for it would be wrong to suspect any deliberate unfairness in any of the operations which I have examined. From a passage in the article 'Figure de la Terre,' by D'Alembert in the original *Encyclopédie*, it would appear that the school of Cassini originally believed that in consequence of the oblate form of the earth, the length of a degree of the meridian would decrease from the equator to the pole. It seems strange, perhaps, now to suppose that such an error could be seriously maintained; but there can be no doubt of it; for example, the error was vehemently maintained by Keill, a man of some reputation, who was ultimately a

Savilian professor at Oxford. See Keil's 'Examination of Dr. Burnet's Theory of the Earth,' page 140. It is certainly a remarkable coincidence that the school of Cassini starting with the erroneous theoretical notion that the degrees of the meridian *ought* to decrease from the equator to the pole arrived at the same result by observation and measurement.

"There can, I think, be no doubt that at least Maupertuis and Clairaut, who were the most eminent of the French party, held the correct Newtonian theory as to the figure of the earth; and their result was rather too decided in its confirmation of this theory. Now the geodetical angles could scarcely be influenced by the theoretical opinions of the observers; because it would not be obvious in what way the result would be affected by an error in an angle. But in measuring the base it would of course be obvious that the larger was the value obtained, the stronger was the evidence for an oblate form. Similarly in estimating the amplitude, the smaller the value obtained the stronger was the evidence for the oblate form. In these two parts of the survey then it would be necessary to be on the watch lest the conviction of what the result ought to be should influence the impression of what the observation really gives.

"It is curious that Maupertuis and his party seem to have thought at first that their success was too decided, and therefore their amplitude too small; and that on their second determination they should have made it between 3" and 4" larger than at first."

#### THE BEGINNINGS OF LIFE \*

##### I.

AFTER a careful perusal of this important and suggestive work, a prominent feeling is one of regret that its value and popularity should be endangered owing to purely technical faults of composition and arrangement. It is so full of curious and novel facts and experiments, it contains so much excellent reasoning and acute criticism, and it opens up such new and astounding views of the nature and origin of life, that one feels it ought to and might have ranked with such standard works as the "Origin of Species" and the "Principles of Biology," if equal care had been bestowed upon it as a literary composition. But, unfortunately, it altogether lacks their powerful condensation and lucid arrangement. Its vast masses of facts are stated too diffusely, and are often so scattered as to lose the cumulative force that might have been given to them; while the arguments are broken up and weakened by a too minute classification of the subjects treated, leading to repetition and confusion rather than to clearness. Haste of composition is further indicated by the quantity of additional matter given in foot-notes that should have found a place in the text; and we often find it difficult to follow the special argument in hand, or to see the connection and relevance of much of the detailed evidence brought forward.

Notwithstanding these defects, which will undoubtedly diminish its popularity, it is a book which will make its mark, and must produce a powerful sensation.

It brings together a large body of facts, either new or hitherto almost ignored, which, unless they can be otherwise explained, prove much more than the mere production of low living organisms from dead matter; for these low forms have been seen to combine and give rise to higher forms, and these again to still higher and more complex organisms. Vegetable cells or their contents develop into various low animals; while animal as well as vegetable organisms of specialised forms and some elaboration of structure seem to be mutually transformable by processes quite unlike any of the hitherto accepted modes

of multiplication or reproduction. These processes have been traced stage by stage, so that there seems no possibility of mistake; and they do not rest on the observations of Dr. Bastian alone. Facts of this nature have been repeatedly published for more than twenty years by many Continental and English naturalists, but, being so entirely opposed to current theories, have been all silently ignored, just as true facts and careful observations relating to the antiquity of man were so long ignored. Our author has, however, repeated and tested many of these observations, and finds them to be strictly accurate; and they harmonise perfectly with the views on the origin of life founded on his own experiments, and so energetically advocated by him.

Looked at merely as curiosities of science, and as an unveiling of mysteries hitherto thought to be inscrutable, these observations are of supreme interest; while their importance in connection with modern theories of development and the origin of species can hardly be overrated. Setting aside all the prejudices and dogmas of the existing schools of biology, it must be admitted that the views here presented of the perpetual origination of low forms of life now, as in all past epochs, is in perfect harmony with the doctrine of evolution, and does away with many of the physical and geological difficulties which are undoubtedly among the most serious which beset those special views of the origin of life which Mr. Darwin holds, but which are by no means necessary inferences from his theories.

‡ The present work is essentially one that to be judged soundly cannot be judged hastily. The subject is of overwhelming importance to the future progress of scientific biology, and the facts and observations on which it is founded are so numerous and so precise, and have been tested by such a body of distinct and competent observers, that no *a priori* arguments and no authoritative dicta can have any weight against them. Observation alone can demonstrate whether they are facts or delusions. They will no doubt be fully criticised by those whose special studies render them competent to do so; but if the past history of science has any value whatever, the result cannot be doubtful. Facts observed and tested by a succession of careful and accurate observers, such as those whose evidence is adduced by Dr. Bastian, have never yet proved to be fallacies.

We now propose to lay before our readers a sketch of the more interesting matters treated of in these volumes, citing a few of the most striking of the new facts and the most important of the arguments founded upon them.

More than half of the first volume is devoted to an account of the Nature and Source of the Vital Forces and of Organisable Matters, and we have an excellent summary of modern views on the correlation of vital and physical forces, on the vital principle, on theories of organisation, and on the modes of origin of reproductive units and cells. As bearing upon subjects to be discussed further on, there is an important remark on the origin of germs or specks of living protoplasm in the fluids of the living body. These fluids, it is maintained, are not alive, and, therefore, the living germ does originate in a dead organic fluid. Even if it is held that blood and all the other secretions are alive, yet as they have been formed out of dead matter taken into the stomach there must be some point at which the particles of dead matter become transformed into living matter, and the circumstance of this occurring *within* an organism does not alter the fact of its occurrence, or render it at all more easy to conceive or explain. Why, then, should it be so absolutely incredible that specks of living protoplasm should arise in suitable fluids out of a living body? It is certain that as soon as the fact that they do so arise is established, the one will be as easy to conceive and be as credible as the other. The only other point that calls for notice in this part of the work is the discussion on the supposed "vital force," in which the views of the "vitalists" seem to be

\* "The Beginnings of Life: being some account of the Nature, Modes of Origin, and Transformations of Lower Organisms." By H. Charlton Bastian, M.A., M.D., F.R.S. (2 vols. London: Macmillan and Co. 1872.)