

the earth. The coloured illustrations showing the characteristic appearances of the various zones are as successful as anything of the kind we have seen, although, what perhaps cannot be avoided in coloured illustrations of this kind, there is a little too much of "the light that never was on sea or land" upon them.

LETTERS TO THE EDITOR

The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.

Necessary Truths—Physical and other

I AM not about to continue a controversy which I regret having been provoked into by the misrepresentations of one who ignored the contents of works he professed to review. Reply and rejoinder may go on endlessly. I could not, to much purpose, argue with Mr. Hayward, who, instead of taking such unconsciously-formed preconceptions as those resulting from the infinite experiences of muscular tensions and their effects, proposes to exemplify unconsciously-formed preconceptions by a consciously-formed hypothesis concerning the relation between weight and motion. Nor should I care to discuss any question with my new anonymous assailant; who, when certain examples given show the "exact quantitative relations" spoken of to be those of direct proportion, describes me as "intensely unmathematical" because I subsequently use the more general expression as equivalent to the more special—which, in the case in question, it is.

The first of my objects in now writing is to remind "some bystanders, who may from their antecedents be presumed competent to judge," that the essential question is not a mathematical one, but a logical and psychological one, in respect of which I am not aware that senior wranglers, as such, can claim any special competence. Further, even admitting the assumption that the question is mathematical, I have to warn the reader that he will be much misled if he infers that there are not "some bystanders who may from their antecedents be presumed" *more* "competent to judge," who concur in the opinion that the laws of motion cannot be demonstrated experimentally.

My second object is to inclose, for publication in NATURE, a passage now standing in type to be added to future impressions of "First Principles" in further elucidation of necessary truths, and our apprehensions of them.

HERBERT SPENCER

"The consciousness of logical necessity, is the consciousness that a certain conclusion is implicitly contained in certain premisses explicitly stated. If, contrasting a young child and an adult, we see that this consciousness of logical necessity, absent from the one, is present in the other, we are taught that there is a *growing up* to the recognition of necessary truth, merely by the unfolding of the inherited intellectual forms and faculties.

"To state the case more specifically:—Before a necessary truth can be known as such, two conditions must be fulfilled. There must be a mental structure capable of grasping the terms of the proposition and the relation alleged between them; and there must be such definite and deliberate mental representation of these terms as makes possible a clear consciousness of this relation. Non-fulfilment of either condition may cause non-recognition of the necessity of the truth; and may even lead to acceptance of its contrary as true. Let us take cases.

"The savage who cannot count the fingers on one hand, can frame no definite thought answering to the statement that 7 and 5 make 12; still less can he frame the consciousness that no other total is possible.

"The boy adding up figures inattentively, says to himself that 7 and 5 make 11; and may repeatedly bring out a wrong result by repeatedly making this error.

"Neither the non-recognition of the truth that 7 and 5 make 12, which in the savage results from undeveloped mental structure, nor the assertion, due to the boy's careless mental action, that they make 11, leads us to doubt the necessity of the relation between these two separately-existing numbers, and the sum they make when existing together. Nor does failure from either cause to apprehend the necessity of this relation make us hesitate to say, that when its terms are distinctly represented in thought, its necessity will be seen; and that apart from any multiplied experiences, this necessity becomes cognisable when

structures and functions are so far developed that groups of 7 and 5 and 12 can be intellectually grasped.

"Manifestly, then, there is a recognition of necessary truths, as such, which accompanies mental evolution. Along with acquirement of more complex faculty and more vivid imagination, there comes a power of perceiving to be necessary truths what were before not recognised as truths at all. And there are ascending gradations in these recognitions. Thus a boy who has intelligence enough to see that things which are equal to the same thing are equal to one another, may be unable to see that ratios which are severally equal to certain other ratios, that are unequal to each other, are themselves unequal; though to a more developed mind this last axiom is no less obviously necessary than the first.

"All this, which holds of logical and mathematical truths, holds, with change of terms, of physical truths. There are necessary truths in Physics, for the apprehension of which, also, a developed and disciplined intelligence is required; and before such intelligence arises, not only may there be failure to apprehend the necessity of them, but there may be vague beliefs in their contraries. Up to comparatively recent times, all mankind were in this state of incapacity with respect to physical axioms; and the mass of mankind are so still. Various popular notions betray inability to form clear ideas of forces and their relations, or carelessness in thinking, or both. Effects are expected without causes of fit kinds; or effects extremely disproportionate to causes are looked for; or causes are supposed to end without effects. But though many are thus incapable of grasping physical axioms, it no more follows that physical axioms are not knowable *a priori* by a developed intelligence, than it follows that there is no necessity in logical relations because many have intellects not developed enough to perceive the necessity.

"The ultimate physical truth of which clear apprehension is eventually reached, is that force can neither arise without an equivalent antecedent, nor disappear without an equivalent consequent. Along with power of introspection there comes recognition of the fact that existence cannot be conceived as beginning or ending: the Laws of Thought themselves negative any such mental representation. And if it be asked why this intuition, which all physical axioms indirectly imply, and which is postulate in every physical experiment, is to be taken as authoritative because its negation is inconceivable, the answer is that no argument which sets out to discredit it can do this without logical suicide; since there is no other warrant for asserting the dependence of any conclusion on its premisses than the inconceivability of its negation."

This passage forms part of a revised version of the chapters on Matter, Motion, and Force, which I have contemplated making for this year past. When those chapters were written and stereotyped in April 1861 (see Preface), the modern doctrines concerning Force and its transformation were so imperfectly developed, that some of the leading technical words now currently used were not introduced. The reorganisation of "First Principles," which I made in 1867, for the purpose of more truly presenting the general Theory of Evolution, did not implicate these chapters, and I believe I did not even re-read them: the stereotyped plates, in common with those of many other chapters, with the numberings of pages and sections altered, were used afresh, and continue still to stand as they originally did. But while now rectifying defects of statement which it was scarcely possible to avoid thirteen years ago, I find no reason for changing the essential conception set forth in those chapters; nor is the need for changing it suggested to me by those on whose judgments I have the best reasons for relying.—H.S.

Royal Society Soirée

WITH reference to your account of the Royal Society's soirée (NATURE, vol. ix. p. 502), will you allow me to explain that all I "promised" concerning the missing pair of Paradise-birds was to deliver them when sent for.

They were not sent for, owing to some mistake, and consequently not exhibited.

May 5

P. L. SCLATER

Father Secchi's Work on the Sun

WITH great surprise I read in NATURE, vol. ix. p. 390, the following note:—

"Father Secchi is preparing at Gauthier Villars a second

edition of his work on the Sun, on an enlarged scale. He has quoted so largely from Mr. Lockyer's 'Solar Physics' that an intended translation of this work is abandoned for the present."

I have the honour to inform you that the complete original of my second edition has been in the hands of M. Gauthier for more than a month, so far as that part which may have something in common with Mr. Lockyer's work is concerned, and that I had not seen Mr. Lockyer's work until a fortnight ago, when I bought it from M. Loescher here in Rome. Mr. Lockyer of course is quoted, but only from his original memoirs, and not from his new publication, nor in such a manner that his publication will render my work useless.

Rome, March 23

P. R. SECCHI

[The following explanation has been sent us by the Paris correspondent who furnished us with the note referred to by Father Secchi:—

"I was told by his (P. Secchi's) editor himself, when I spoke to him about publishing a French edition of Mr. Lockyer's 'Solar Physics,' the substance of what I have written to you. I think that the note I have written is a recommendation of Father Secchi's work; but not so his statement that he did not possess 'Solar Physics' until it was too late to use it. There is nothing whatever dishonourable in quotation."—ED.]

Spontaneous Generation Experiments

SINCE October 1870 I have, as opportunity offered and other work permitted, made a series of experiments bearing on the question of spontaneous generation. They seem to me to tell so plain a story that I am anxious to relate it.

The thoughts which led to the experiments were briefly these:—

The occasional or even frequent presence of living growths in fluids after they have been exposed to a temperature of 212° F. and are contained in closed tubes or flasks is rather an indication of the imperfection of a method than the proof of a theory; for under like circumstances living organisms ought either always or never to develop; the conditions being uniform, the results should be uniform.

When the tubes are closed at a blow-pipe flame after boiling, steam cannot be escaping from the aperture at the time of actual closure, and it is conceivable that in the momentary collapse of the contents which then occurs some atmospheric air containing organic matter may pass into the tube and invalidate the experiment.

The contained air, if any there be after the sealing of the tubes, must be vastly rarefied, and the ordinary atmospheric conditions, other than purity, which are essential, must be absent or greatly modified.

I attempted to devise an experiment which would be free from these possible sources of error; one in which the atmospheric pressure should be normal, in which the physical structure of the air should be unaltered, and in which there should be no chance of organic contamination after heating. Further it seemed a good thing to be able to show at the same time and in the same apparatus two distinct specimens of the boiled fluid, the one exposed only to cleaned air, the other exposed also to common air; and also to use a fluid which would indicate to the naked eye by change of colour, or of clearness, or of consistence, the time at which living growths made their appearance.

The latter condition was secured by using a fluid (for the idea of which I am indebted to Mr. Heisch's experiments on water-impurities) composed of 10 cc. of urine, 1 gramme of white sugar, and 90 cc. of distilled water. This when boiled and filtered is a clear transparent liquid, which becomes milky on the occurrence of organic growth during fermentation in thirty to forty hours, according to the heat to which it is exposed.

The other conditions were effected by using a glass tube of the shape of the capital letter M, with curved bends instead of the angles; a tube which may be described as having four straight legs joined to each other by two loops on the upper side and one on the lower; the first leg closed and the last leg open and short.

This tube, so bent, was made very hot, so as to expel as much air as possible from it; the open end was then plunged into the boiled and filtered urine-sugar fluid, and such a quantity allowed to flow in on the cooling of the tube as left the first, second, and third legs about half full when the tube was held upright. The tube was again heated to the boiling of the contained fluid in order to expel as much air as possible by the generation of

steam. It was then allowed slowly to cool, so that the first leg was about one-third filled with fluid; and such an amount was left in the lower loop as would rise in the second and third legs to about the same extent as the tube cooled (and the cooling was designedly prolonged); air passed through the fluid in the lower loop to fill the space in the first upper loop, between the two masses of fluid, left vacant by the condensation of steam.

The tube was then hung up, away from direct sunlight, and exposed to the ordinary changes of temperature of my study.

If I have been able to describe intelligibly this very simple matter, it will be seen that I had here two portions of the same fluid separated from each other; both having been heated to the same temperature and both exposed to atmospheric air.

The conditions were precisely similar with one exception; intentional and crucial. The air in the first upper loop, to which air only the fluid in the first leg was exposed, had passed through and been washed by the fluid in the lower loop; and the fluid of this loop was on one side exposed to the washed air and on the other side to the ordinary atmosphere.

In experiments with this apparatus the phenomena were, in eight cases, as follows:—On the second or third day the fluid in the loop was milky, and the fluid in the first leg was bright. At the end of a week, a month, four months, indeed as long as the tube was kept, the one continued clear, the other was turbid. At the expiration of a time, varying in different experiments from four days to four months, I tilted over the least drop of the turbid fluid in the loop into the clear fluid in the first leg, when at once the milkiness began, and in a day the whole of the leg fluid was turbid also.

In many cases I examined the two fluids, clear and turbid, with a twelfth-inch object-glass, and found Bacteria in the turbid fluid; nothing in the clear fluid.

Twice I left (once unintentionally, once intentionally) so little fluid in the loop that, there being a small aperture, it did not fulfil its purpose as a filter and a valve, and in both cases the two masses of fluid became turbid at the same time.

In six other experiments I used urine; in four instances the loop fluid showed symptoms of putrefaction, and became turbid in four or five days, but the leg fluid remained clear. On the closure of the experiment, at varying periods from a week to four months, the bright urine appeared, on microscopic examination, to contain no organic growth, but underwent putrefaction as ordinary urine when exposed to the air.

In the two other experiments both urines putrefied at the same time. In one case I hastened the cooling by cold; in the other I left very little fluid in the loop.

In four experiments I used Dr. Charlton Bastian's turnip-cheese fluid. In all cases the solution was milky when made; twice it was filtered and twice unfiltered, and in all cases, when examined by the microscope after the lapse of some days or weeks, the fluid in both leg and loop contained organic growth.

The experiments on urine and urine-sugar fluid show, in my view, both positively and negatively, that there is something in the ordinary air which is a necessary condition of the origin of organic growth in these liquids.

Positively this position is demonstrated when, after six months, the fluid in contact with unwashed air is seen to be full of organic growth, and the fluid in contact with washed air is still unchanged.

Negatively it is supported when both fluids are seen to grow turbid at the same time from imperfect washing of the air, by reason of too rapid cooling or too scanty a supply of fluid for the washing.

The experiments with Dr. Bastian's turnip-cheese fluid were for some time a puzzle to me, and made me fear that there was an undetected fallacy in my other experiments. But now it is clear that the contradiction is only apparent. Dr. Burdon Sanderson has shown that this fluid contains within itself the elements of organic growth which are not destroyed at 212° F., the temperature at which my experiments were necessarily conducted.

I am anxious not to press these experiments unduly, but they seem to me to range themselves unequivocally in opposition to the theory of spontaneous generation; although they touch no great extent of the subject.

That the something in the ordinary air necessary for the origin of organic growth in the fluids used is a gaseous impurity of the air is supported by no fact of which I am aware; but whether it be living organised germ or dead unorganised matter, these experiments do not explain or attempt to explain.

LEONARD W. SEDGWICK