The fundamental principles of chemistry, and the nature of chemical action, are laid down in the first twenty pages of the book, after which the non-metals and some of their common compounds are described. As a companion in the laboratory, containing details of many instructive experiments, the book should find favour.

On page 8 we read: "Quite recently it has been found that Helium, one of the bodies which had already been observed in the corona of the sun, occurs in the gases extracted from certain minerals by heating them in vacuo." Helium is a constituent of the solar prominences, but not of the corona.

Mr. Trotman's book follows very much the same lines as that of Dr. Bailey; but it is more suitable for use in connection with elementary classes than for the laboratory. It is an attractive little volume, simply worded, clearly printed, and plainly illustrated. We regret to notice the absence of an index.

Hygiene for Beginners. By Ernest S. Reynolds, M.D. Pp. xiv + 235. (London: Macmillan and Co., Ltd., 1896.)

THERE are a number of good elementary books on hygiene, but this one will find a place among the best of them. The author's "Primer of Hygiene" is very well known, being widely used in Evening Continuation Schools, Technical Institutes, and County Council courses. A knowledge of elementary anatomy and physiology is, however, essential before the main principles of hygiene can be intelligently grasped. Recognising this, the author has introduced chapters on the structures and functions of the various parts of the human body, and has considerably enlarged his "Primer" in other directions. The first hundred pages of the present volume comprise nine chapters on elementary anatomy and physiology; the remaining nine chapters are devoted to that extensive and varied knowledge concerned in the prevention of disease. The book is thus thoroughly in touch with the syllabus of elementary hygiene of the Department of Science and Art. We are not given to praising books moulded to particular syllabuses, but the present volume does not slavishly follow the lines laid down by the examiner in the subject with which it deals, and the independence is a sign of the author's ability to judge for himself the best arrangement and scope of the matter. It would be to the advantage of the community if every individual had to pass an examination in the subjects dealt with; and we venture to say that every householder, and every mother having the care of children, should be acquainted with as much of the elementary principles of hygiene as is contained in this volume. As to teachers of South Kensington classes in hygiene, they only need to see the book to appreciate its admirable qualities.

The Parasitic Diseases of Poultry. By Fred. V. Theobald, M.A., F.E.S. Pp. xv + 120. (London: Gurney and Jackson, 1896.)

POULTRY are subject to many parasitic diseases, and the object of this manual is to inform poultry-keepers of the life-histories of these pests, so that means of prevention may be successfully carried out. Mr. Theobald is zoologist to the Agricultural College at Wye, while his knowledge of the characteristics and habits of the parasites he describes has been gained from observation of many diseased birds. Poultry-breeders and fanciers may, therefore, safely trust themselves to be guided by him; and they will learn from his book how to distinguish and cope with the animal and vegetable parasites which often cause them such serious loss. Entomologists will discover in the work some new points on the life-histories of the parasitic forms dealt with, as well as a list of the parasites found upon fowls.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of NATURE. *No notice is taken of anonymous communications.]*

The Letters of Charles Darwin.

I AM preparing to publish a supplementary series of Charles Darwin's letters. My projected volume will include a full selection from those letters of a purely scientific interest which I was unable to print in the "Life and Letters," as well as from any fresh material that may now be entrusted to me.

I would, therefore, ask those of my father's correspondents who have not already done so to allow me to make copies of any letters of his which they possess. I venture to remind those who may be inclined to help me, that letters of apparently slight or restricted interest are often of value. FRANCIS DARWIN.

Wychfield, Cambridge, December 26.

On the Goldbach-Euler Theorem regarding Prime Numbers.

IN the published correspondence of Euler there is a note from him to Goldbach, or, the other way, from Goldbach to Euler, in which a very wonderful theorem is stated which has never been proved by Euler or any one else, which I hope I may be able to do by an entirely improved method that I have applied with perfect success to the problem of partitions and to the more general problem of demonstration, *i.e.* to determine the number of solutions in positive integers of any number of linear equations with any number of variables. In applying this method I saw that the possibility of its success depended on the theorem named being true in a stricter sense than that used by its authors, of whom Euler verified but without proving the theorem by innumerable examples. As given by him, the theorem is this: *every even number* may be broken up in one or more ways into two primes.

My stricter theorem consists in adding the words "where, if $2n$ is the given number, one of the primes will be greater than $\frac{n}{2}$, and the other less than $\frac{3n}{2}$. This theorem I have verified by innumerable examples. Such primes as these may be called mid-primes, and the other integers between 1 and $2n-1$ extreme primes in regard to the range $1, 2, 3 \ldots, 2n-1$.

I have found that with the exception of the number 10, Euler's theorem is true for the resolution of $2n$ into two *extreme* primes; but this I do not propose to consider at present, my theorem being that, with exception of $2n = 2$, every even number $2n$, may be resolved into the sum of two mid-primes of the range $(1, 2, 3 \ldots 2n-1)$. As, *ex. gr.*

$$4 = 2 + 2 \quad 6 = 3 + 3 \quad 8 = 5 + 5 \quad 10 = 3 + 7$$
$$12 = 5 + 7 \quad 14 = 7 + 7 \quad 16 = 5 + 11$$
$$18 = 5 + 13 = 7 + 11 \quad 20 = 7 + 13$$
$$40 = 11 + 29 = 17 + 23 \quad 50 = 13 + 37 = 19 + 31$$
$$100 = 29 + 71 = 41 + 59$$
$$200 = 61 + 149 = 73 + 127 = \&c.$$
$$500 = 127 + 373 = 193 + 307 = \&c.$$
$$1000 = 257 + 743 = \&c.$$

And so on.

My method of investigation is as follows. I prove that the number of ways of solving the equation $x + y = 2n$, where x and y are two mid-primes to the range $2n-1$, *i.e.* twice the number[1] of ways of breaking up $2n$ into two mid-primes + zero or unity, according as n is a composite or a prime number, is exactly equal to the coefficient of x^{2n} in the series

$$\left(\frac{1}{1-x^p} + \frac{1}{1-x^q} + \ldots + \frac{1}{1-x^l}\right)^2$$

where $p, q, \ldots, l$ are the mid-primes in question. This coefficient, we know *à priori*, is always a positive integer, and therefore if we can show that the coefficient in question is not zero, my theorem is proved, and as a consequence the narrower one of Goldbach and Euler. By means of my general method

[1] This number may be shown to be of the order $\frac{n}{(\log n)^2}$ and a very fair approximate value of it is $\frac{\mu^2}{n}$ where μ is the number of mid-primes corresponding to the frangible number $2n$

of expressing my rational algebraical fraction, say ϕx, as a residue, by taking the distinct roots of the denominator, say ρ, and writing the variable equal to $\rho\epsilon^{\theta}$, and taking the residue with changed sign of $\Sigma\rho^{-n}\epsilon^{-nt}\phi\rho\epsilon^{t}$, we can find the coefficient of x^{n} or (if we please to say so) of x^{2n} in the above square, and obtain a superior and inferior limit to the same in terms of $p, q, \ldots, l$; and if, as I *expect* (or rather, I should say, *hope*) may be the case, these two limits do not include zero between them, the theorems (mine, and therefore *ex abundantia* Euler's) will be apodictally established.

The two limits in question will be algebraic functions of $p, q, \ldots, l$, whereas the *absolute* value of the coefficient included within these limits would require a knowledge of the residues of each of these numbers in respect to every other as a modulus, and of $2n$ in respect of each of them. In a word, the limits will be algebraical, but the quantity limited is an algebraical function of the mid-primes $p, q, r, \ldots l$.

J. J. SYLVESTER.

Athenæum Club, December 20.

P.S.—The shortest way of stating my refinement on the Goldbach-Euler theorem is as follows:—"It is always possible to find two primes *differing by less than any given number* whose sum is equal to twice that number."

Another more instructive and slightly more stringent statement of the new theorem is as follows. Any number n being given, it is possible to find two primes whose sum is $2n$, and whose difference is less than $n, n-1, n-2, n-3$, according as n divided by 4 leaves the remainders $1, 0, -1\ -2$ respectively.

Major MacMahon, to whom and to the Council of the Mathematical Society of London I owe my renewed interest in this subject, informs me that in a very old paper in the *Philosophical Magazine* I stated that I was in possession of "a subtle method, which I had communicated to Prof. Cayley," of finding the number of solutions in positive integers of any number of linear equations in any number of variables. This method (never printed) must have been in essence identical with that which within the last month I have discovered and shall, I hope, shortly publish.—J. J. SYLVESTER.

Telegraphy without Wires, and the Guarding of Coast Lines by Electric Cable.

IT appears from an article in *Commerce*, December 16, that Mr. W. H. Preece, in a lecture on "Telegraphy without Wires," at Toynbee Hall, said, that from experiments at the Goodwin Lightship it had been found impossible to get a message on board, and "that the intervening sea-water performed much the same function as an iron plate," I would like to call the attention of the readers of NATURE to my paper laid before the Royal Society of Edinburgh in January 1893, when it was shown that neither salt nor fresh water had any appreciable effect on the transmission of these electrical waves. Take this case—an iron steamer afloat above a cable lying on the sea-bottom. If the steamer have on board suitable apparatus, messages sent along the cable from a single Leclanche cell can be and have been read on board ship by ordinary sailors. If it is possible to so convey messages to a vessel not moored by an anchor, it is surely possible to do the same to a moored ship such as a lightship. Mr. Preece's failure at the "Goodwin" is not due to the action of salt water, for, if electric vibrations work through salt water in the Firth of Forth, they will equally do so at the "Goodwin."

One word as to Prof. Boase and Mr. Marconi's systems. Although it may be impossible to say what system may be found best for the detection of the electric vibrations, there is one thing certain that it is needless refinement to try to send the vibrations for lighthouse work ten miles. The vibrations require to be sent only 600 feet, as it is possible to lay a cable guarding a stretch of fifty miles of coast, ten miles off the shore, in at most fifty fathoms of water, and send the vibrations along it, and whenever the ship comes within two hundred yards of the cable the detector on board would give the alarm. Further, the advantage of the cable system is great, as the vessel would know her exact distance off; whereas, by sending the vibrations from a point on shore, this would be impossible.

CHARLES A. STEVENSON.

84 George Street, Edinburgh, December 21.

The Origin of the Stratus-Cloud, and Some Suggested Changes in the International Methods of Cloud-Measurement.

IN his "Instructions for Observing Clouds" (London, 1888, p. 12), Hon. Ralph Abercromby defines *stratus* as "a thin uniform layer of cloud at a very low level," and as an illustration reproduces a photograph of a low sheet of cloud which he says is exceedingly characteristic of east winds in London. In his book "Weather," p. 48, he shows by a diagram that the position of the *stratus* is in the south-west quadrant of the anticyclone. By carefully plotting the observations made at the Blue Hill Meteorological Observatory during the past ten years, I find that this type of cloud has the same position in the anticyclones on the eastern coast of the United States that Abercromby found for England. Moreover the continuous records, made by instruments lifted by kites at the Blue Hill Observatory, furnish a very evident explanation of its origin. In a number of cases the recording instruments were lifted into or through such clouds, and in every case the temperature and humidity rose suddenly as the thermograph entered and passed through the stratus-cloud. This rise of temperature is not shown when the thermograph is lifted into cumulus or nimbus clouds. Hence it is evident that the stratus described by Abercromby is found at the plane of meeting between a cold current and a warmer, damp current overflowing it. The cause of the stratus is undoubtedly the mixture between the two currents and the consequent condensation of moisture in the warmer current.

There is, however, another conception of stratus described by Prof. H. H. Hildebrandsson in his "Classification des Nuages employée à l'Observatoire météorologique d'Upsala," where he says: "One sees that the stratus of Howard is nothing but a fog; at Upsala we designate also, under the name of stratus, fog lifted above the earth, and which exists ordinarily as isolated fragments at a slight distance above the ground." In the Hildebrandsson-Köppen-Neumayer cloud-atlas a picture of one of these isolated fragments is given above the name of *stratus;* and the primary definition of stratus given in large letters is "Lifted Fog."

These two definitions of stratus by Abercromby and Hildebrandsson have apparently been taken as identical by their authors; but I think the facts mentioned indicate that they have no more in common, either in origin or appearance, than have cirrus or cumulus. When the International Committee met at Upsala it recognised the inadequacy of the illustration of stratus given in the Hildebrandsson-Köppen-Neumayer atlas, and, like Abercromby, *pictured* stratus as a thin sheet of low cloud, but *defined* it as "Lifted fog in a horizontal stratum." This compromise between two entirely different conceptions of stratus results in an absurdity. Lifted fog rarely or never forms in a horizontal stratum. Certainly, during ten years of daily observations of clouds, I have not seen such a phenomenon, nor have I seen it described by writers on the subject. Moreover, if lifted fog ever does form in a horizontal stratum, how can an observer know, when he sees a stratus, whether it is lifted fog or is a cloud formed by mixture? I trust at some future meeting of the International Committee this definition may be changed. Probably the authors of the definition will not object to the change, now that the observations with kites have thrown a new light on the origin of stratus.

Another point to which I think the attention of those engaged in the international scheme of measuring the heights and velocities of clouds should be called, is the fact that measurements of cloud-heights by theodolites or photogrammeters give erroneous averages for certain forms of clouds. At Blue Hill Observatory, using every opportunity to measure the altitude of nimbus with theodolites, we find the average height by such measurements to be 2077 metres; yet in our measurements of cloud-heights, made by sending kites into them, we find that on more than half the days when nimbus is present its base is at an altidude of less than 1000 metres, and usually less than 500 metres. The average height determined from the kite-measurements is 497 metres, and by the angle above the horizon of the light reflected at night from the clouds over distant cities it is found to be 845 metres. Similar differences are found in the case of strato-cumulus. The reasons are that low clouds are so indefinite in outline, or they cover the sky with such a uniform veil, that they cannot be measured with theodolites or photogrammeters. It results that the clouds measured by theodolities are principally high clouds. On the other hand very high clouds cannot be measured with kites, and the average