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 ρe^{θ} , and taking the residue with changed sign of $\Sigma \rho^{-n} e^{-n\theta} \phi \rho e^{\theta}$, 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 $p, q, \dots, l$; 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 apodictically established.

The two limits in question will be algebraic functions of $p, q, \dots, l$, 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 $p, q, r, \dots, l$.

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.

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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 photogrameters 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 altitude 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 photogrameters. It results that the clouds measured by theodolites are principally high clouds. On the other hand very high clouds cannot be measured with kites, and the average

height by this method is too low. The average determined from reflected lights (845 metres) is probably most nearly correct.

I think it is apparent that the observations with theodolites and photogrameters at the international cloud-stations should be supplemented by other methods, if correct averages are to be obtained, and if clouds which cover the sky with a uniform veil are to be measured at all. Small balloons turned loose and followed with theodolites, suggested by Kremser, is a good method in such cases.

H. HELM CLAYTON.

Blue Hill Meteorological Observatory, December 7.

Radiography.

YOUR correspondent, Mr. G. M. Lowe, asks for information as to the best methods of working direct on to sensitised paper to save the time and expenses involved in taking glass negatives. "Nikko" paper, as supplied by the Eastman Company, is a good substitute for glass plates, and the results on it are much superior to the smooth bromide papers. Eikonogen is a suitable developer—say five or six ounces of water to the contents of an eikonogen cartridge, and fix in cleah hypo solution. Of course, to show the palma-surface of the hand when using a glass plate, the film side is *up* and the palm *down*; but in using paper direct, the film should be down, and the X-rays, therefore, pass through the paper before reaching the sensitive surface.

Radiographs made direct on paper are *negatives*, the bones being shown white. It has been stated that this is the correct way to show the bones, but it is quite a mistake. Bones are white by reflected light; by transmitted light they are black, more or less, and if X-rays are light rays, then the light is transmitted, and radiographs ought to be as usually shown printed from a glass negative; but for surgical purposes, for such as foreign bodies in the hand, negative or positive makes little or no difference. By the direct "Nikko" paper method the exposure must be longer, but to locate a needle in the hand thirty to forty seconds is sufficient. Two, three, or up to a dozen sheets of "Nikko" paper may be exposed at one time. Between the first and second sheet very little difference in exposure will be noticed, but between the first and, say, the twelfth the difference will be considerable. To extend this difference when only a few sheets are used, insert a piece of suitable black paper between each.

W. I. CHADWICK.

The Heating of Anodes in X-ray Tubes.

I SHALL be much obliged if any of your readers who work with the X-rays will give me their experience with the 10-inch coils. I have one by Apps, which is excellent in every way; but whether I take from it a 2-inch or a 10-inch spark, the anode of the tube invariably becomes red or white hot within a few seconds.

The tubes are by leading makers, and exhausted for 8 or 10-inch sparks; but, as I have said, even a 2-inch spark makes the anodes red hot.

On the other hand a German coil I have, does not perceptibly heat the anode of the tube even when I use a 5-inch spark.

Is this the experience of others; and why should a 2½-inch spark from one coil make the anode red hot immediately, when a 5-inch spark from another coil does not do so?

This difficulty at present prevents me employing the Apps 10-inch coil at all for X-ray work.

WALTER CHAMBERLAIN.

Harborne Hall, near Birmingham, December 19.

Units of Force.

IN your issue of December 10, Prof. O. J. Lodge makes several curious statements.

He speaks of "inertia multiplied by the square of a velocity." He might as well speak of "shapelessness multiplied by the cube of a length." Inertia is a word best left unused, but usually means a property of what is called matter—like whiteness, hardness, inextensibility.

He also speaks of natural formulæ "independent of every system of units that can be devised," and, though he only gives one formula, implies that every mathematical relation can be expressed in a similar manner. Will he be so good as to give a formula connecting the weight, volume, and specific gravity of a body which is "independent of every system of units"?

As to the poundal, the objection to it is that no one uses it in

actual work. There may be other objections, but that is a sufficient one.

As to teaching elementary mechanics, I am convinced that we should avoid "mass" as much as possible. When dealing with a particle, express Newton's Second Law by the formula $P/Q = f/a$, where P and Q are the forces producing accelerations f , a , respectively. This will usually take the form $P/W = f/g$. Then you may use any unit of force you choose, and the energy formula becomes $P \times s = W \frac{v^2}{2g}$, which may be in inch tons,

foot pounds, or what you please. Is it too much to hope that the poundal may be shortly relegated, even in text-books, to that place, wherever it is, where grades are employed for measuring angles?

C. S. JACKSON.

R.M. Academy, Woolwich.

The Distance of the Visible Horizon.

HAS not Prof. Lodge in his enthusiasm, which I fully share, for an absolute system of measurement rather overstepped the mark when in the equation $2R h = d^2$ for the distance of the visible horizon, he says that " h is not the number of feet, or of metres, or anything else, it is the actual height; d is not the number of miles or of inches to the horizon, but it is the distance itself; and similarly $2R$ is the diameter of the earth, and not any numerical specification of that diameter (see NATURE, vol. iv. page 125). Surely the equation as written is an algebraical equation, and, as such, the symbols it contains express numbers and not things. The multiplication as he implies of one length ($2R$) by another length (h), is abhorrent to the mind of "the Cambridge mathematician." The superiority of the formula over the mutilated apology for it which Prof. Lodge quotes, lies in the fact that the equation is true in terms of any conceivable unit of length in which the three lengths involved in it are measured. I am of course aware that the particular formula given may be regarded as an abbreviated statement of the approximate geometrical proposition that the rectangle contained by the diameter of the earth and the height of the observer above its surface equals the square on a line equal to the distance of the visible horizon, in which case, of course, Prof. Lodge's description of the symbols would be accurately true; but I do not think that the formula with this interpretation really illustrates his meaning.

I wish to associate myself with Prof. Lodge in his condemnation, for educational purposes, of all formulæ of the engineer's pocket-book type, should it unfortunately happen, that they gain a footing on the scientific side of school instruction it will do much to justify the slur, still too often cast, on science teaching at schools and at the universities, that it is not education. This must be my apology to Prof. Lodge for thus emphasising a mere *lapsus calami*.

L. CUMMING.

Rugby, December 12.

Position of Boughs in Summer and Winter.

THE following measurements may perhaps be of interest. They have been made with a view to ascertaining how much the weight of leaves and fruit depressed the branches of a tree. The first measurements were taken on August 3, the second on December 14, 1896:—

Height from Ground in inches.				
		August 3.		December 14.
Mulberry tree—				
Lowest twig	...	0 in.	...	31 in.
Higher branch	...	59 in.	...	72 in.
Another branch	...	20 in.	...	39 in.
Walnut tree—				
Lowest twig	...	15 in.	...	34 in.
Higher branch	...	60 in.	...	76 in.

In the case of the first branch of the mulberry tree, it was found in December that a weight of 35 pounds was not sufficient to lower it to its summer position.

AGNES FRY.

Failand, near Bristol, December 15.

The Cultivation of Woad.

WITH reference to the letter of Rosa M. Barrett, in NATURE of November 26, p. 79, I formerly lived for many years, and my father before me, in the part of Somerset to which your correspondent alludes, viz. the neighbourhood of Bath, and within a few miles of Mells, I never remember to have seen or