apparatus, constructed for the purpose, illustrated this fact by showing that the horizontal oscillations at the deck and bottom of the ship respectively are in opposite directions at the same instant of time. At a certain height above the keel there is no horizontal oscillation, this being therefore the *locus* of the axis of torsion. Maxima and minima of the turning moment at each revolution depend on the number of cranks, the amplitude of the oscillations being mostly dependent on steam distribution. These oscillations are periodic, and likewise have their nodular points. The author next proceeds to treat the points mentioned mathematically in the case of a prismatic rod. He shows that the number of vibrations is proportional to the speed of progress of vibration. Substituting a ship's body, he finds that this speed of progress remains constant for similar ships, and also that the number of torsional vibrations varies in an indirect proportion with the length of the ship. For a better understanding of these points, we must refer our readers to the original paper and the diagrams by which it is illustrated. That engines of special construction will cause no vibration if placed just above the nodular point, is also true for torsional vibrations.

Mr. Mallock, who, it may be stated, has done much excellent work for the Admiralty in connection with this subject, dealt in his paper with "the determination of the direction and magnitude of the forces and couples which arise from the unbalanced moving parts of marine engines." Something may be done, the author said, towards balancing an engine by the proper disposition of the pistons, connecting-rods, and cranks; but it does not seem practicable to produce a complete balance in any ordinary engine without having recourse to counterbalance weights. In order to determine the weights required, the author has produced a geometrical construction showing, by the aid of arithmetic only, the resultant force and couple due to the unbalanced moving parts of any engine. Without the aid of the diagrams shown at the meeting, it would be impossible to make the explanation clear, even if space permitted us to give the details in full. It will be sufficient to say that the engine is divided up into its component parts, to each of which a value is given, and in this way the resultant force is found and the resultant couple determined. Having got all the information necessary to assign the magnitude and direction of the force and couple which will completely balance the engine, if the force could be applied at the centre of gravity of the moving parts, it would merely remain to decide what weights should be used to produce the required effect. In general the construction of the engine makes this inconvenient, if not impossible, and other positions for the counterbalance weights must be found. This aspect of the problem is then considered in detail by aid of the figures.

The second part of the paper was devoted to showing how the frequency of vibration of any ship, loaded in any manner, can be found by models, and that all the data for shaping these models can be readily obtained from curves which would be in the hands of the ship designer. An example of the apparatus used was shown, the author giving a practical illustration of its working. The course pursued is to make an exact copy of the ship on a very small scale, exactly proportional in all dimensions and identical in material. It is known by theory that the frequency of vibration of the model and ship will be inversely proportional to their lengths. The model is replaced by an exact copy on the same scale, made of some other material—wood—the frequency of the new model differing from that of the former in the ratio

$$\sqrt{\frac{q_s \rho_m}{q_m \rho_s}},$$

where q_m, q_s, and ρ_s are the respective elasticities (Young's Modulus) and densities of the wood and the material of the ship. Next the wooden model is replaced by a plank of the same wood of uniform thickness but variable breadth, the breadth being such that the stiffness of the plank against bending at every cross section is proportional to the stiffness of the model at the corresponding position. Weights are fixed to the plank in such a manner that the weight at any cross section is proportional to the weight at the corresponding section of the model. Then the frequency of the plank, compared with that of the model, can be ascertained by a formula.

In the apparatus shown the plank was supported by two rollers slung from two similar rollers, the latter resting on an overhead railway. The plank was kept vibrating by a magnetic apparatus, and a recording device was added. The rollers supporting the plank gave the position of nodes. It is only when the rollers are at the positions where the nodes would be, if the plank was free from all constraint, that the frequency of the plank will be related to that of the ship as given by the author's formula. The natural nodes are found by varying the position of the rollers until the frequency is a maximum for the type of vibrations under consideration.

The method here introduced by Mr. Mallock is interesting and ingenious, but how far it is applicable to the needs of the naval architect, or whether the average ship builder, if he wish to reduce vibration, will prefer the former method of adjusting the engine to the known conditions of the ship after she is built, are questions which experience alone can decide.

The paper of Mr. Robinson and Captain Sankey dealt largely with the question of vibration in connection with electric light engines, the problem of vibration in the hull of a vessel being thus eliminated. Here again a number of diagrams were used which we cannot now reproduce, and our abstract of this paper must be therefore brief. Investigation showed that in the case of an electric light station the high speed vertical engines, each 200 indicated horse-power, with two cranks set opposite each other, and run at 350 revolutions per minute, were mounted on a large slab of concrete. The engines being vertical, the moving parts had to travel through a greater distance during the upper half of the revolution of the crank pin, than during the lower half. Calculation showed that each line of parts singly tended to lift the engine at up-stroke by about 3·5 tons, and it tended to depress it 2·3 tons. Therefore twice in a revolution a net lifting power of one ton acted upon the engine, and changed an equal number of times into a depressing power of about 1·2 tons. The result was a "pumping action" on the water-soaked soil beneath the concrete slab, and in this way vibration was conveyed to surrounding buildings. The action, it will be seen, was due to the angular movement of the connecting rods, a feature which Herr Schlick said might be neglected; a point in which the authors, naturally, and also Mr. Mallock, by no means agreed with him.

An arrangement of two engines with their framings rigidly connected, and having three cylinders each, was proposed, the object being to neutralise the endways rocking or tilting tendency, and also to give freedom from tendency to vary the downward pressure.

A discussion followed the reading of these papers.

Mr. Hök's paper described a new way of carrying out a known investigation. Whether the new way is better than the old way, is a point which may be decided by experience. The last paper of the meeting, that by Mr. Martin, was of a disappointing nature. Marine engineers have long been asking for an explanation of the hitherto unexplained fact—if fact it be—that "induced draught" is so much better for boilers than "forced draught." Mr. Martin's experiments were quite beside the mark.

The summer meeting of the Institution will be held in Paris, commencing on June 11.

QUESTIONS BEARING ON SPECIFIC STABILITY.[1]

AT the suggestion of your President, I beg to submit three questions to the notice of this Society. They bear on a theoretical problem of much importance, namely, the part played in evolution by "organic stability."

The questions are especially addressed to those who have had experience in breeding, but by no means to breeders only; nor are they addressed only to entomologists, being equally appropriate to the followers of every other branch of natural history. I should be grateful for replies relating to any species of animal or plant, whether based on personal observation or referring to such observations of others as are still scattered through the wide range of periodical literature, not having yet found a place in standard works. The questions are for information on:—

(1) Instances of such strongly marked peculiarities, whether in form, in colour, or in habit, as have occasionally appeared in a single or in a few individuals among a brood; but no record is wanted of monstrosities, or of such other characteristics as are clearly inconsistent with health and vigour.

[1] A paper read at the Entomological Society, April 3, 1895, by Francis Galton, F.R.S.

(2) Instances in which any one of the above peculiarities has appeared in the broods of different parents. In replying to this question, it will be hardly worth while to record the sudden appearance of either albinism or melanism, as both are well known to be of frequent occurrence.

Note.—The question is *not* asked now, whether such peculiarities, or "sports," may be accounted for by atavism or other hypothetical cause.

(3) Instances in which any of these peculiarly characterised individuals have transmitted their peculiarities, hereditarily, to one or more generations. Especial mention should be made, whether the peculiarity was in any case transmitted in all its original intensity, and numerical data would be particularly acceptable, that showed the frequency of its transmission (*a*) in an undiluted form, (*b*) in one that was more or less diluted, and (*c*) of its non-transmission in any perceptible degree.[1]

It is impossible to explain to a general meeting the precise way in which the desired facts would be utilised. An explanation that would be sufficiently brief for the purpose could not be rendered intelligible except to those few who are already familiar with the evidence, and the technical treatment of it by which the law of Regression is established, and with the consequences and requirements of that law. Regressiveness and stability are contrasted conditions, and neither of them can be fully understood apart from the other.

I may as well take this opportunity of appending a list of my various memoirs on these subjects. They appeared from time to time in various forms as the inquiry progressed and as suitable openings occurred for writing or speaking. The more important of these are Nos. 1, 3, part of 6, 7, and 8 in the following list. Nos. 1 to 5 refer to regression only.

LIST OF MEMOIRS, BY MR. F. GALTON, ON REGRESSION AND ORGANIC STABILITY.

(1) Typical Laws of Heredity. *Journal* of the Royal Institution, 1877. (This was the first statement of the law of Regression, as founded on a series of experiments with sweet peas.)

(2) Presidential Address, Anthropological Section of the British Association, 1885. (Here the law of Regression was confirmed by anthropological observations.)

(3) Regression towards Mediocrity in Family Stature. *Journal* of the Anthropological Institute, 1885. (A revised and illustrated reprint of No. 2.)

(4) Family Likeness in Stature. *Proc. Roy. Soc.*, 1886.

(5) Family Likeness in Eye Colour. *Proc. Roy. Soc.*, 1886.

(6) Natural Inheritance. (Macmillan and Co., 1889.) (This volume summarises the results of previous work.)

(7) Patterns in Thumb and Finger Marks . . . and the Resemblance of their Classes to Ordinary Genera. *Phil. Trans. Roy. Soc.*, 1891.

(8) Discontinuity in Evolution. *Mind*, 1894. (An article on Mr. Bateson's work.)

A NEW DETERMINATION OF THE OHM.

A FRESH determination of the value of the ohm in absolute measure has been made by F. Himstedt (*Wiedemann's Annalen*, liv. p. 305). The method employed is that which the author had used in a previous determination, and consists of passing through a galvanometer all the make or break currents induced in a secondary coil when the current in a long primary helix is interrupted a known number of times per second. A known fraction of the primary current is then passed through the same galvanometer. The primary helix in these experiments consists of a single layer of uncovered copper wire, wound, by means of a screw-cutting lathe, in a regular spiral on a glass cylinder. The turns of wire are held in their place, and the insulation improved, by being coated with shellac. As the mean of a number of determinations, the author obtains the value 106·28 cm. as the length of the column of mercury at 0° C., having a cross section of one square millimetre, which has the resistance of 10^9 C.G.S. units. In connection with the above-described experiments, the author has been led to measure some coefficients of self-induction, using for this purpose a modification of the Rayleigh-Maxwell method. The great difficulty in measuring a coefficient of self-induction by this method is that, in order to get a throw of sufficient magnitude to be accurately measured, it is necessary to employ a somewhat strong current. The result is that the temperature of the coil, the self-induction of which is being measured, rises rapidly, and thus the balance of the Wheatstone's bridge for steady currents is upset. Herr Himstedt gets over this difficulty by using the commutator, which he employs in his determination of the ohm, to break the battery circuit a known number of times per second, and to cut the galvanometer out of circuit while either the make or break is taking place. In this way a steady deflection is obtained of sufficient magnitude to be readily measured, even when the current employed is between 0·001 and 0·002 amperes. The above method only differs from that employed by Profs. Ayrton and Perry in their secohm-meter, in that the author takes two separate readings, one with the bridge balanced for steady currents, the other when the commutator is working, instead of bringing the galvanometer deflector to zero by upsetting the steady current balance.

[1] Written communications should be addressed to F. Galton, 42, Rutland Gate, London, S.W.

THE SMITHSONIAN INSTITUTION REPORT FOR 1894.

MR. S. P. LANGLEY'S report of the operations of the Smithsonian Institution for the year ending June 30, 1894, has just reached this country, and it furnishes interesting reading on a number of points relating to the U.S. National Museum, the Bureau of Ethnology, the Bureau of International Exchanges, the National Zoological Park, and the Astrophysical Observatory.

The total permanent funds of the Institution are now 911,000 dollars, and interest at the rate of 6 per cent. per annum is allowed upon this by the Treasury, the interest alone being used in carrying out the aims of the Institution. The total receipts during the fiscal year covered by the Report amounted to 69,967 dollars, and the entire expenditure, including a sum of eight thousand dollars added to the permanent fund, was 67,461 dollars. The Institution also disbursed the Treasury grants of 14,500 dollars for International Exchanges; 40,000 dollars for North American Ethnology; 154,000 dollars for the U.S. National Museum; 50,000 dollars for the National Zoological Park; and 9000 dollars for the Astro-Physical Observatory.

It appears to be an essential portion of the original scheme of the government of the Institution that its secretary should be expected to advance knowledge, whether in letters or in science, by personal research; but the increasing demands of time for labours of administration has greatly limited the possibility of doing this. Mr. Langley has, however, found time to continue his researches upon the solar spectrum (see NATURE, November 1, 1894). This work, carried on in the Astro-Physical Observatory, is certainly of more than common importance. His investigations upon aerodynamics have also been continued intermittently. They are not complete, but they appear to point to conclusions of general and unusual interest.

A widespread interest seems to have been awakened in the Hodgkins competition, with reference to investigations appertaining to the nature and properties of atmospheric air. A letter printed in NATURE of June 21, 1894, announced that the time within which papers might be submitted was extended to the end of last year. The Report informs us that, up to June 30, 1894, 250 memoirs, printed and manuscript, had been received in connection with the competition, representing correspondents in the United States, Mexico, England, Scotland, Norway, Denmark, Russia (including Finland), France, Belgium, Germany, Austria-Hungary, Servia, Italy, and British India.

A few grants have been made from the Hodgkins Fund, in aid of certain important researches. In this connection we notice that Prof. E. W. Morley's work on the determinations of the density of oxygen and hydrogen, aided by special apparatus provided by the Institution, is approaching completion.

The investigations undertaken by Dr. J. S. Billings and Dr. S. Weir Mitchell into the nature of the peculiar substances of organic origin contained in the air expired by human beings, has been continued under a grant from the Hodgkins Fund, and also the researches by Dr. O. Lummer and Dr. E. Pringsheim, of Berlin University, on the determination of an exact measure of the cooling of gases while expanding, with a view to revising the value of that most important constant which is technically termed the "gamma" function.

Mr. Langley refers again to the unsatisfactory condition of the National Museum. The collections have increased so