denying this he seems to me to reduce the force exerted *by* the train *on* the locomotive to the quantity R alone; and since R is less than F, the pull exerted *by* the locomotive *on* the train, he thus abandons the doctrine that "all force is of the nature of stress," and that "the stress is measured numerically by the force exerted on either of the two portions of matter."

The quotation which Professor MacGregor makes from Poisson I shall not attempt to discuss at length; for I am not familiar with his writings, and do not know exactly what meaning he attached to the word *résistance*. If he used this word as I understand Professor MacGregor to use it, to indicate an *opposing force*, and if he was at the same time committed, as I understand Professor MacGregor to be, to the view that one force always implies an equal and opposite force, then I can only say that I think Poisson was wrong in one part or the other of his doctrine.

E. H. HALL.

Cambridge, March 5.

Comparative taxation.

While I cordially accept all Mr. Henry B. Gardner's statements in regard to the insufficiency of my study of the comparative taxation in Europe and America, I cannot accept his conclusions. He says, in fact, "The inadequate scope of the work has in large measure destroyed the value of the study." To this I cannot agree; and my witness is Mr. Gardner himself. My work has brought out his intelligent criticism, and has turned the attention of himself and of very many other persons to the importance of developing the science of comparative statistics, which is what I have aimed at.

It is very true that I have not attempted to compare the relative taxation of cities, towns, and other subdivisions of states in Europe with those of America; it is very true that some of the cities of this country are excessively taxed as compared to those of Europe: all the more reason for a complete study of the subject. Where are the materials for such an investigation? I have given, to the best of my ability, the relative burden of *national* taxation. I stated that this part of the taxation of countries should be considered separately from that of the towns and cities, for the reason that in Europe a very large part of the national taxation is expended for *destructive* purposes or for the support of privileged classes; while, with the exception of a few cities in this country, the revenues derived from local taxation are paid out for *constructive* purposes both there and here; and on the whole, in spite of the cumbersome nature of the collective work of cities, counties, and towns, the people of this country get about seventy-five cents' worth on a dollar for what they pay in municipal taxes.

Moreover, although Mr. Gardner may not be able to find exact returns of taxation in European countries corresponding to the *per capita* figures which I have submitted, yet I claim to have proved them after as complete examination as is open to a private and unofficial person who does not read German. I hold that the revenue of state forests, mines, and other instrumentalities of subsistence which are often controlled in Europe by governments, constitute as true a tax upon the people as if they had been assessed directly upon their property; and I am of opinion that I have understated the burden of national taxation in Europe rather than overstated it. Suffice it that the figures have attracted attention; and it may be that within one, two, or three years a complete comparison of national as well as state, county, and town taxation may become possible. I should be glad to see Mr. Gardner try his hand, not so much in criticising my work, as in preparing more accurate and more complete tables. EDWARD ATKINSON.

Boston, March 5.

On the flight of birds.

The wing is extended upward from the horizontal position by the deltoid and the latissimus dorsi muscles to a line which is perpendicular to the body, and is quickly again depressed to the horizontal position by the pectorales. This constitutes the first stage of the 'stroke.' 'Recover' is initiated by an inward rotation of the humerus, semiflexion of the wing at the elbow (the pinion remaining extended and directed obliquely downward and outward), and is carried well forward to a degree sufficient, when seen in profile, to conceal the head. In this position the primaries are semirotated so as to present the least amount of surface to the air in the direction in which the bird is moving. The impetus excited by the stroke carries the bird upward and forward. In the second stage of 'recover,' the humerus is rotated outward, the arm is quickly raised, the primaries restored to the position seen in the bird at rest, and the wing is a second time in the position for the 'stroke.' In the eagle and the hawk the legs are in the position of the 'stroke' when the wings are similarly placed. During the 'stroke' the legs move backward. This motion continues during the 'recover' of the wing, so that the time of the 'recover' of the wing is also that of the 'recover' of the leg. The action of both wings and feet, since both pairs act together, is what I propose to call 'synadelphic.'

The study of the flight was confined to the eagle, the hawk, the pigeon, and the parrot, in the series of instantaneous photographs taken by Mr. Edward Muybridge, under the auspices of the University of Pennsylvania. HARRISON ALLEN.

Philadelphia, March 7.

On the serpentine of Syracuse, N.Y.

An especial interest attaches to this rock for two reasons: 1°, because of the almost total absence of rocks of this class, or indeed of any intrusive rocks, from the undisturbed paleozoic strata of New York; and, 2°, because of the importance which has been recently attributed to it by Dr. T. Sterry Hunt, as affording evidence in favor of his chemical precipitation theory of the origin of serpentine.

The Syracuse serpentine was discovered in 1837, and was described by Vanuxem in his third annual report in 1839 (pp. 260 and 283), and in his final report on the geology of the third district in 1842 (p. 109). It is also mentioned by Beck, in his 'Mineralogy of New York,' as a 'dike or bed' (1842, p. 275). Dr. Hunt published an analysis of this rock in the *American journal of science* for 1858 (xxvi. p. 236), and has laid great stress upon it in his recent essay on the geological history of serpentines.

Through the courtesy of Prof. A. H. Chester of Hamilton college, the writer has been enabled to study a very complete suite of this rock and its associates, which was collected by the late Prof. Oren

Root while he was principal of the Syracuse academy. Mr. J. Forman Wilkinson of Syracuse, who was at this time one of Professor Root's pupils, has contributed several interesting points relating to the occurrence of the serpentine. In a recent letter to the writer, he says, in speaking of the different localities mentioned by Vanuxem and Beck, "The exact place was upon the lawn now owned and occupied by Howard G. White. . . . The specimens that you have were gathered some time between 1837 and 1845, probably nearer the earlier period. We used to go to the bed sometimes with a pick (oftener not) to gather and sort out the specimens. They were found in a bed of decomposed green rock, which was soft, and readily gave way under the pick. This bed of green disintegrated rock extended all along the side of the hill from the middle of James Street, nearly to the place where Howard White's house was built. The specimens were, I think, all found at the north or James Street end. . . . *When a trench was opened for water-mains opposite, and near to this deposit of serpentine (about fifty feet away), the cutting was through gypsum.*" The outcrop has not been accessible for over forty years.

It will be readily seen that the main point of interest connected with this rock is its mode of origin, — whether aqueous or igneous. It is included between two beds of porous limestone or dolomite. Among the dozen or more specimens in the possession of the writer, there are some which show angular fragments of this limestone embedded in the serpentine. In one case these are so abundant as to afford a breccia with a serpentine matrix. By far the best proof of the eruptive nature of the rock from which the serpentine has been derived is, however, afforded by its microscopic structure. The hand specimens agree exactly with the descriptions of Vanuxem and Beck. There are two principal varieties,— one a compact, dark-green rock, in which a few bronzy crystals are seen; and a mottled one, occasionally stained with blood-red spots. A microscopical examination shows that both of these rocks are most typical representatives of the class known as peridotites; the former with a slightly, the latter with a very pronounced, porphyritic structure. The original structure is still perfectly preserved, although most of the constituents are changed to serpentine or a carbonate. The groundmass contains, beside these two minerals, magnetite, a brown mica peculiarly characteristic of certain peridotites, green amphibole, and yellowish octahedrons which may prove to be anatase. The porphyritic crystals have the typical crystal forms of olivine or enstatite, both so perfect and so sharp that they could only be the early crystallizations from a fluid magma. The blood-red spots are seen to be due to the common staining of altered olivine crystals by iron hydroxide. The more porphyritic specimens are doubtless from the edge of the mass, and the coarser-grained variety from its centre.

The evidence of the eruptive origin of the Syracuse serpentine appears, therefore, to the writer to be: 1°. The microscopic structure, which shows that the original mineralogical composition and arrangement of the rock were such as are only found in masses of an eruptive nature; 2°. The included fragments of the adjacent limestone; 3°. The last remark quoted from Wilkinson's letter, that fifty feet away, on the strike of the deposit, only gypsum was encountered.

There seems to be nothing in any of the published descriptions of this deposit which indicates that its origin was aqueous. Such an idea, expressed by both Vanuxem and Hunt, is purely a matter of opinion, unsupported by any facts.

The writer hopes soon to publish in more detail the results of his study of this rock. It seems to bear a strong resemblance to the carboniferous peridotites recently described from Kentucky by Mr. J. S. Diller, of the U. S. geological survey, — an opinion with which Mr. Diller himself wholly concurs.

GEORGE H. WILLIAMS.

Baltimore, Md., March 7.

Thought-transferrence.

It is always a rash course to attack other people's work on the strength of second-hand reports of it, and doubly so when the reports have themselves been those of hostile critics. This rashness I am forced to impute to 'J. J.,' the writer of a paper on 'Some miscalled cases of thought-transferrence,' in your supplement for Feb. 4, as I cannot for a moment believe him capable of the deliberate *suppressio veri* and *suggestio falsi* which his attempt to explain our English results by 'number-habits' would otherwise involve. The idea that the argument for thought-transferrence has depended entirely, or mainly, on experiments in which one person chose a number at will, and another person tried to guess it, could not survive the most cursory study of the published evidence. Yet that idea, picked up by 'J. J.' from an article in the *National review*, is the one on which his own criticism is expressly and exclusively founded, and which every one of his readers, if unacquainted with the original evidence or some trustworthy version of it, must at this moment be holding.

As a matter of fact, this type of experiment (though, as I shall show, 'J. J.' has greatly exaggerated its defects) has hardly ever been employed by us, and its results are a negligible quantity in our case. Our published records do not include a single instance in which the object to be guessed was a single digit chosen by the agent. Where the number contains two digits, the risk of appreciable disturbance of the results by 'number-habit' is of course far less; and trials of this type form between a sixth and a seventh part of the tabulated Creery aggregate.[1]

But their importance in the cumulative result of those experiments is very much smaller than this fraction would indicate; since the success obtained in them, though very remarkable, was less so than in some other types. If 'J. J.' likes to omit them, one and all, as 'vitiated,' he is welcome to do so; and he will, at any rate, have the satisfaction of striking a certain number of noughts off the odds — estimated at about a hundred million trillions to 1 — against obtaining by accident the amount of success re-

[1] This aggregate consists of results where the object of which the idea was to be transferred was known only to some member or members of the investigating committee. See the table in 'Phantasms of the living,' vol. i. p. 25, as to which it should be noted, that in the experiments with single digits, included under the second head of Dublin experiments, the numbers were drawn at random out of a bag. Trials with "letters of the alphabet, and names of people and towns," by the way, find no place in this crucial list; but I am curious to know whether 'J. J.' would account, e.g., for the correspondences of names recorded on p. 27, by 'independent similar brain-functioning.'