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.'

corded. Our only other published instance of trials where double numbers were chosen, is that described in 'Phantasms of the living,' vol. i. p. 34; and here, as soon as we heard of certain remarkable results which were being obtained by two of our friends, we took the precaution (which 'J. J.' regards as beyond the capacity of such as us, though likely to occur to 'psychologists and writers on probabilities') of insisting that the numbers should be *drawn*, and not *chosen*, by the agent. This precaution has, of course, been invariable in our principal class of experiments, where the objects to be guessed have been playing-cards. Of two long series recorded in 'Phantasms' (vol. i. p. 34, and vol. ii. p. 654), where double numbers were similarly drawn, one gave as the total of completely correct guesses a result against the accidental occurrence of which the odds were over two millions to 1; the other, where account was taken of cases where the two right digits were guessed in reverse order, and of cases where one only of the digits was guessed rightly and in the right place, gives a total result against the accidental occurrence of which the odds were nearly two hundred thousand million trillion trillions to 1.

I have perhaps said enough to indicate the extent of 'J. J.'s' misrepresentation; but I may further briefly point out how defective his reasoning would be, even supposing that experiments of the sort attacked had really occupied the place in our evidence which he supposes. 1. His own remark, that the discovery of 'number-habit' was "brought about by noticing that quite constantly an undue number of successes occurred at the *beginning* of many sets of number-guessings," might have suggested to him how slightly it was likely to affect long series, where all the numbers appear again and again. To make out his case, he must get a few uninitiated persons each to write down a series of, say, fifty digits, and must ascertain by comparing the first, the second, the third items, and so on, of each pair of lists, whether the number of correspondences in each pair far exceeds the ten (one-tenth of the total), which is the theoretic most probable number, and, if so, how far such excess is connected with the predominance of one or two particular digits. How the correspondences could be produced by a '*varying* predilection for *different* numbers,' I must leave it to him, or the writers whom he quotes, to explain. 2. The cases he adduces where 'persons were asked to choose a number, *no limits being set*,' and then, as a rule, chose numbers under 20 or under 10, are quite irrelevant. We never, on any occasion, gave this unlimited choice, which would have precluded the knowledge of exactly what it was most essential to know, — the degree of probability that chance would produce the results obtained. 3. The fact that many people, when asked to choose a number with three figures, choose a number containing the digit 3, is quite irrelevant: for, in the first place, we have never experimented with numbers of three digits; and, in the second place, the fact that 3 sensibly predominates in a number of *first* choices does not even tend to suggest that it would sensibly predominate in a *series* of choices. 4. To experiments with double numbers (when chosen and not drawn), 'J. J.' objects that people are apt to choose multiples of ten with disproportionate frequency, and that they tend to choose numbers near the higher limit. A glance at the double-number results recorded in 'Phantasms of the living' (vol. i. p. 34) will show the futility of making a serious objection to them out of the slight preference[1] for multiples of ten; for the number of successes (obtained before the plan of drawing from a bowl was introduced) exceeded what chance was likely to give, even supposing that the agent's choices and the percipient's guesses *had throughout been restricted* to multiples of ten — restricted, that is, to nine out of the ninety numbers over which they freely ranged. As regards the alleged predilection for later numbers, I need only remark that in a series of any length it ceases to be apparent;[2] while, even if it continued, the later numbers in a set of ninety are sufficiently numerous to insure, at each trial, large odds against accidental success.

In conclusion, I cordially agree with 'J. J.' in recommending (as my colleagues and I have recommended publicly and privately times without number) such forms of experiment as leave the issue between chance and thought-transferrence perfectly clear. I am also glad to find him, and the writers whom he quotes, so completely sound on another point which I have specially urged, — the fallacy of extracting evidence for thought-transferrence from the frequent simultaneous utterances of thought and feeling by relatives and intimate associates. Such fallacies cannot be too often exposed; for telepathy suffers far more from friends who accept and proclaim it on insufficient grounds than from its most strenuous critics and opponents. Whether 'J. J.' would continue to hold *our* grounds insufficient, if he took the trouble to learn what they are, I cannot tell; meanwhile he must pardon my feeling a certain sense of alliance with one who so clearly perceives that the novel doctrine, though evidence may prove it, could never be proved by casual experiments or by loose, popular arguments. How soon the proof will be generally recognized as complete, depends on something which we, unfortunately, can neither foresee nor control, — the degree in which sympathy with our objects and methods takes the form of help.

By chance, I have only just seen *Science* for Jan. 21, in which I read that Dr. Minot has lately introduced some trick-experiments with cards as similar to some of our thought-transferrence trials. In Dr. Minot's cases the card was forced on the drawer by a confederate of the professing 'percipient.' In all our card-experiments the card was drawn at random from the pack by one of our own investigating group. For these cases to resemble Dr. Minot's, it would be necessary that the percipient, or some one connected with the percipient, should have held the pack while the card was drawn. To permit such a procedure would have implied a degree of incompetence on our part which it did not occur to us explicitly to disclaim. However, I take this opportunity of disclaiming it, by stating that the pack was invariably held by one of ourselves; almost always, in fact, by the person who made the draw.

Dr. Minot is further reported to have objected that "in many of the English experiments there existed

[1] I have just examined the details of 1,191 of these trials, which I have under my hand, and find that the cases where multiples of ten were chosen form rather more than an eighth, instead of a ninth, of the whole.

[2] I have examined three hundreds, taken at random, of the series just mentioned. In the first hundred, 53 of the numbers chosen were nearer the higher limit than the lower; in the second and the third hundred, 55 were nearer the lower limit.

evident opportunities for fraud." Quite true — not in many only, but in all; and not only in psychical but in physical experiments of all sorts, which people accept without verifying the results for themselves. But *whose* fraud? We have always been content to rely on the very large class of cases in which the fraud would have had to be *our own*, — fraud in which the investigators actively shared, not merely which they failed to detect. I am far from saying that Dr. Minot or any one else is bound to accept this condition as crucial. But it is surely obvious that he who carries his experiments to the point where they can only be impugned by impugning his good faith, has done — as far as the *quality* of his results is concerned — all that any experimenter in any branch of science ever can do. Nothing remains, after this, but to try to increase the *quantity* of the results, whereby the responsibility for them may be spread over other shoulders.

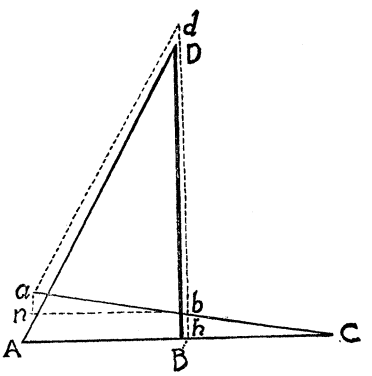
EDMUND GURNEY.

London, Feb. 17.

On tiptoe.

About two years ago Mr. F. A. Pond requested me to work out for him the problem of the human foot regarded as a lever. He thought the essential feature of the case — namely, the attachment of the calf-muscle to the leg below the knee, as well as to the heel, by a tendon — had been ignored.

The question has been of interest to a number of people; and it may be well to bring the true state of the case before writers on anatomy and physiology, inasmuch as it appears to be generally stated that the foot is a lever of the second order when used in rising 'on tiptoe.'



It will do to assume the change of position so small that the foot may be treated as a straight lever. Let *A B C* be the foot-lever: *A*, the point of attachment of tendon to heel; *B*, the ankle pivot; and *C*, the point where the foot rests upon the ground. At *B* erect a perpendicular, *BD*, to represent the leg-bones, the calf-muscle being attached at *D*. Now let the muscle contract, and raise *B* to *b*. The work done is equal to the weight of the body (supposing one foot used) multiplied by the perpendicular distance through which *B* is raised, that is, *bh* of the figure. The power exerted by the muscle is equal to its pull multiplied by the diminution of the distance *AD*. As *B* rises to *b*, let *A* rise to *a*, and *D* to *d*. Through *b* draw *bn* parallel to *AC*, and drop *an*. Now, *bC* is to *bh* as *ba* is to *an*. The line *an* is very approximately the amount of shortening of the muscle. The sign of the 'mechanical advantage' will be positive, zero, or negative, according as *AB* is greater than, equal to, or less than, *BC*. A lever of the 'second order' implies advantage of positive sign; that is, so-called 'mechanical advantage.' A lever of the 'third order' implies mechanical disadvantage. A lever of the 'first order' is capable of affording mechanical advantage or mechanical disadvantage, as the ratio of the arms determines: hence, when one rises on tiptoe, the foot is a lever of the first order.

An attempt has been made to regard the case as of the second order, by calling the upward pull at *A*, *y*, and the pressure of the body at *B*, *x*. The pull *y* will be transferred as a downward thrust of *y* to *B*; so that we have (if, for instance, $AB = BC$) an upward force of *y* at *A*, and a downward force of $x + y$, equal to $2y$, at *B*. But the traverse of *y* is not twice the traverse of $2y$. Thus the 'principle of work' limits the case to the 'first order.'

F. C. VAN DYCK.

New Brunswick, N.J., Feb. 28.

Increase of the electrical potential of the atmosphere with elevation.

Very many observations of the electrical potential of the atmosphere have been made at different places in this country during the past year, under the auspices of the U.S. signal office. Among others, at Washington, D.C., a series of simultaneous observations has been carried on at the instrument room of the signal office and at the top of the Washington monument, the highest known edifice. The object of the present paper, published by permission of the chief signal officer, Gen. A. W. Greely, is to present in brief some of the results of those observations, particularly those bearing on the value of the intensity of the electrical force of the atmosphere at an elevation of five hundred feet, and the variations of the potential under different conditions of weather.

Beccaria, De Romas, Henley, and Cavallo, all noticed that the more elevated the position of the collecting apparatus, the greater the degree of electrification. Schübler (*Schweigg. journ.* ix. 348) was the first to make measurements of the difference, and found that a positive electrification increased, at least up to a height of 50.5 metres. His results with an electroscope were as follows: —

Height (metres).....	9.7	16.2	24.4	47.1	49.4	55.6	58.5
Deflection (degrees).....	15	20	26	50	53	58	64

Sir William Thomson, it is sometimes stated, found an increase of from 200 to 300 volts for three metres. This value, however, was one obtained with a portable electrometer on a flat open sea-beach on the island of Arran, the height of the match being nine feet above the earth. The readings varied from 200 to 400 volts, so that "the intensity of electric force, perpendicular to the earth's surface, must have amounted to from 22 to 44 Daniell elements per foot of air" (Thomson, reprint of papers, xvi. 281). It is also intimated that on other dates this value might have been twice as large, or yet much smaller. Mascart and Joubert found that if two water-collectors were placed in the same vertical line, the one five, the other ten metres high, the indications were in the main alike, and in the ratio of 1 to 2. Some experi-