wider acceptance than our knowledge of the facts would seem to justify.

Assuming the continents to be simply so much matter, of half the earth's mean density, on the surface of our otherwise closely centrobaric spheroid, it may be shown that individually they will draw the sea-surface up towards their centres by considerable amounts (about a thousand metres at most), leaving corresponding though not equal depressions opposite those centres; and that collectively they will produce a wavy sea-surface, in which the maximum radial distance from crest to hollow is about six hundred metres. The theory, and the equations assigning the form and position of this wavy surface, have been developed by Helmert in his excellent treatise on geodesy ('Die mathematischen und physikalischen theorieen der höheren geodäsie'), from which the above figures have been taken. If we dropped our examination of the question at this point, we might infer the reality of the wavy surface just described. The existence of such assumed continents, however, implies a proportionate variation of gravity along the sea surface and along the same level surface extended through the continents. They would, for the most part, produce an excess of gravity over the continental and a deficiency over the sea areas. But this conclusion is in direct contradiction with the results of pendulum experiments. The assumption, therefore, that the continents are superficial masses, unbalanced in their attractive effects, is, as clearly shown by Helmert, inadequate, and must, together with the conclusions based thereon, be modified or rejected.

Some writers, notably Fischer and Listing, have proved the existence of a highly irregular sea-surface by a still more unsound process than that indicated above would be if we neglected to examine its fundamental assumption. This process, in brief, rejects in an equation a term of the same order as those retained, and arrives at a simple relation between the variation of gravity and the radial distance from the actual sea-surface (or geoid) to the mean spheroidal surface. Helmert fitly characterizes this relation as entirely worthless (*ganz wertlos*), since it fails in every case to give the proper sign when the increments of gravity and radial distance due to the combined action of the continents are substituted in it.

Those desiring to examine minutely the merits of this question should consult the above-named treatise of Helmert, who gives a critical review of the cognate works of Fischer, Listing, Bruns, and others. For the benefit of the general reader, it may be stated, that, although the sea-surface is undoubtedly somewhat irregular, geodesy and geology have as yet furnished no adequate evidence of irregularities amounting to more than ten metres. Additional information, of which it must be admitted there is great need, may disclose the existence of a surface having hills and hollows separated by an interval of fifty or possibly a hundred metres; but irregularities of any greater extent appear to be quite improbable.

The suggestion of your correspondent, that the barometer would indicate any large elevations or depressions in the sea-surface, is not well grounded. The surfaces of equal pressure in the atmosphere must approximate to parallelism with the sea-surface, however irregular it may be. In a state of quiescence the air-surface in contact with the sea is necessarily a surface of equal pressure. The barometer would therefore, if moved from one point to another along the sea-surface, register only such variations in pressure as are due to changes of temperature, winds, etc., and hence afford no indication of the elevations and depressions in question, if they exist.

R. S. WOODWARD.

Washington, D.C., June 17.

Barometer exposure.

Mr. Clayton's letter concerning the influence of wind on the indication of the barometer broaches a subject of great importance to theoretic and practical meteorology, and I trust it may lead to the execution of the experiments essential to the intelligent treatment of the difficulty. As his conclusions are called in question by President LeConte, I take the liberty of rehearsing some investigations of my own which tend to sustain Mr. Clayton's conclusions.

In June, 1873, an elaborate series of synchronous barometric observations were made by the signal office at four stations on the summit and slope of Mount Washington. In testing a special method of barometric hypsometry, I had occasion to discuss these observations, and I discovered an important anomaly which was correlated with the velocity and direction of the wind. The discussion cannot be repeated here, for lack of space; but it may be said that its method and material were such as to leave no reasonable doubt that the wind was the disturbing factor, while they afforded quantitative results far more precise than can be reached by any method of reduction to sea-level. The reader who cares to examine them should consult the 'Second annual report of the U. S. geological survey,' pp. 521–534 and 562–565. One of the specific conclusions was, that a northwest wind of fifty miles per hour, by drawing air out of the summit observatory, presumably through the chimney, caused the mercury in the barometer to stand .13 of an inch too low; and it was estimated that a wind-velocity of a hundred miles would lower the mercury more than half an inch.

I think President LeConte is mistaken in supposing that the matter could be simply tested by comparing the indications of a barometer in a room with those of a barometer out of doors. If the out-of-door barometer were placed on the windward side of a building or other obstruction, and close to it, it would be immersed in compressed air, and read too high. If placed under the lee of an obstruction, it would be surrounded by relatively rarified air, and read too low. If placed in a position uninfluenced by obstructions, the locus of difficulty would be transferred from the surrounding atmosphere to the instrument itself, for the air chamber above the mercury in the cistern of the barometer would itself be influenced by the wind so as to receive a tension abnormally high or low. These statements, based on familiar physical laws, are not individually susceptible of ready verification, because, while the wind blows, all local tensions are disturbed, and we have no standard air-pressure for comparison. I have, however, determined experimentally that the reading is higher in front of an obstruction than behind it. A difference of .15 of an inch was found between barometer-readings on opposite sides of the apex of an acute mountain-peak.

In my opinion, the proper method of escaping the difficulty is, not to place the barometer out of doors, where observation during a wind is itself a matter

of difficulty, but to so arrange the observatory that the influence of the wind shall be either measured and subtracted, or avoided altogether. Place the barometer in an air-tight box, made partly of glass for purposes of observation, and connect this box by a tube with an opening on the roof so adjusted that it shall always sustain the same relation to the wind. It is possible that a form of opening can be devised such that the wind will neither compress nor dilate the air within the box; but, if this cannot be done, it is certainly possible, by a proper system of experiments, to determine for a given arrangement of aperture the proper correction to apply to the barometer-reading for each measured velocity of wind. The matter should receive thorough investigation.

G. K. GILBERT.

Washington, June 19.

I infer from Prof. John LeConte's letter in your last issue (*Science*, vol. vii. p. 550) that he does not feel entirely satisfied with the explanation I have offered of the slight fluctuations of the barograph observed at Blue Hill during high winds. He says, "The observed facts are, that fluctuations of wind-velocity correspond with fluctuations of air-pressure. In some cases it may be difficult to decide which is cause, and which is effect." In this case, the fact, as stated in my last letter, that I could produce these fluctuations at will by merely opening and closing a hatchway in the top of the building, seems to me to prove conclusively that the wind was the cause, and the change in the pressure the effect. In regard to his suggestion that a comparison should be made between a barograph inside and one outside of the building, I think, before satisfactory results could be obtained, it would first have to be proven that the wind in blowing across the top of the barometer cistern, or at right angles to the crevices of such cistern, would not have the same effect of lowering the readings of the barometer outside as well as inside of the building.

Mr. E. B. Weston of Providence has informed me that he has noticed during high winds small oscillations of his barograph, similar to those observed at Blue Hill, and has prevented them by opening the windows, so as to give a free draught of air. I tried the same at Blue Hill during a late high wind, and found that the oscillations, which at most were slight, were reduced by it.

In regard to those large differences between the observed and estimated pressure on Mount Washington, referred to in my last as collected by Professor Loomis, it is probable, that, in these extreme cases, other causes than that suggested by me become factors in the result; such, for instance, as a lagging of the time of minimum pressure at the top as compared with the base, and a more violent cyclonic circulation of the wind at that height, — causes which have been suggested by Professor Loomis in his twentieth paper (*Amer. journ. sc.*, vol. xxviii. July, 1884).

H. HELM CLAYTON.

Blue Hill meteor. observ., June 19.

A most extraordinary structure.

Referring to P.Z.S. 1885, p. 908, pl. lxi. fig. 3, *h*, where my amiable young friend Dr. Shufeldt describes and figures the humerus of a humming-bird as 'a most extraordinary structure,' I may be permitted to suggest that some of the alleged 'eccentricities' of this 'unique' bone might seem less if he had not got the bone turned hind part before by one of those strokes of genius which a prosaic world, steeped in materialism, is slow to appreciate.

A THEOSOPHIST.

Smithsonian institution,
Washington, June 15.

Aspects of the economic discussion.

I have just read Professor Newcomb's article (*Science*, vii. No. 176) on the new school of political economy. It seems to me that the professor asks for too much in the way of results from the new school. As I understand it, this is simply a question of methods. The new school professes the historical method, as opposed to the deductive method of the so-called orthodox school. If the historical method is right, the results eventually arrived at will, nay must, be right. But to stop them on their way as if with a revolver, and demand a categorical statement of their views on such disputed points as state interference before they are allowed to finish their journey, is certainly unwarranted.

Whatever results the new school may reach, it is tolerably certain that they will eliminate from the books that monster of imagination the 'economic man,' and that other *chimaera bombans in vacuo*, the hypothetical 'consumer,' who does nothing in this world but eat.

When they shall have rebuilt the science on their new foundation, it will be soon enough to demand from them an account of their views on such questions as Professor Newcomb propounds.

WM. A. INGHAM.

333 S. 16th St., Philadelphia, June 18.

Distribution of colors in the animal kingdom.

In the notice of Camerano's 'Distribution of colors in the animal kingdom' (*Science*, vii. p. 557) I notice the astonishing statement that green 'never occurs among mollusks.' On the contrary, it is one of the most common colors of mollusks, especially among fresh-water species. Examples will occur to the most superficial observer in the genera Anodonta, Unio, Campeloma, Anculotus, etc. Among land-shells the arboreal helices of tropical countries are noted for their magnificent greens. Among marine shells, it is notable in many species of Mytilus, Modiola, Tellina, Prasina, etc., among pelecypods; Neritina, Chlorostoma, Turbo (where the calcareous operculum, also, is often stained with green), Haminea, and many other gastropods; not to speak of the nudibranchs, which frequently exhibit different shades of green. The rarest color among mollusks is pure blue (as distinguished from the rather common bluish violet), but even this color is found of great brilliancy in some cases. The assertion objected to is one more bit of evidence to the general neglect among biologists, otherwise well equipped, to gain any general knowledge of the Mollusca, except that supposed to be afforded by theoretical views taken from out worn text-books. There are perhaps a dozen first-class general conchologists in the world, none of whom are young. The prospect now is that the next generation will not have any. The reasons seem to be, among others, the shocking state into which amateurs and superficial students have brought the nomenclature, and the fact that the scientific training to be had in our best colleges leads in altogether different directions.

WM. H. DALL.