almost a necessity. As regards, however, the extension of the system throughout the State, where individual work in the field should be done in order to familiarize the worker with special and local conditions, I fully recognize the incompleteness of the evidence. It deserves to be emphasized, however, that the student of a carefully prepared map has always at hand the accumulated knowledge acquired by the corps of topographers whose painstaking labor it represents—labor which the modern school of physiographers has been quick to use as the basis of their conclusions. It is not assumed that along every trough line of the map lies the course of a fault. In my article it is stated (p. 478):

"The term 'trough lines' * * * may, for the present, be given no further signification than lines so favored by nature that the waters of the region have been induced to adopt them for their channels over longer or shorter distances. On a map of this scale the trough lines, if rectilinear, should be slightly curved, but inasmuch as the present river courses, because of the many accidents of their history, can only roughly approximate to the directions initially given them, it would be an over refinement to introduce a correction of this nature."

Evidence obtained from the examination of a map by this method can only be of value when cumulative. A single stream which persists in a given direction even for a long distance affords little support to the theory, when compared with that yielded by a number of smaller streams each approximating to a rectilinear course for a shorter distance, *provided the rectilinear courses are parallel.* A harder layer of rock, or a barrier of drift may conduct one stream or the other in its course, but it is inherently improbable that one of these causes or the other, or both combined, have controlled the parallel river series in an area of such geological structure as we find in the State of Connecticut. As was pointed out in the paper, it is worthy of note that so few of the master streams of the area follow the slope of the plain of erosion. As regards the larger area of the State the theory may, perhaps, as Professor Davis says, 'be regarded as standing in an interrogative rather than in a demonstrative attitude,' but it would be doing injustice to the facts to consider the trough lines as isolated lines while ignoring their arrangement in parallel series.

Wm. H. Hobbs.

PHYSIOLOGICAL EFFECT OF DIMINISHED AIR PRESSURE.

To the Editor of Science: The interesting communications of Messrs. Clayton and Ward upon the physiological effects of the diminished air pressure due to mountain climbing recall some records which I made in 1896 during an ascent of El Misti, Peru, similar to that described by Professor Ward. As the effect of the high altitude upon my condition was in part different from that experienced by him, it may be of interest to describe it. The journey from the observatory at Arequipa, elevation 8,050 feet, to the summit of El Misti, elevation 19,200 feet, was made on four occasions. The distance is about 25 miles. It is possible to ride on horseback or muleback to the very summit, following a caravan trail across the pampa to the base of the mountain, and ascending by a winding path constructed with great skill by Professor S. I. Bailey when the meteorological station was established. The journey from the observatory to a hut at an elevation of about 15,400 feet occupies one day, during which the rider is usually obliged to endure the scorching rays of the sun. The night is passed at the hut, and the final ascent to the summit made on the second morning. This occupies several hours, as the animal stops to rest every fifteen or twenty feet at this altitude. On two occasions I was obliged to walk a short distance to cross snow which had drifted across the path, and realized the extreme difficulty of breathing during the exertion required. The return from the summit to the observatory is easily made on the second day, but on two occasions I spent a second night at the hut.

The effect of the altitude upon me was chiefly to cause headache, sleeplessness and partial loss of appetite. On one occasion while at the summit I experienced a decided feeling of faintness for a short time. During the nights at the hut the temperature was about 32° Fahr., but it seemed impossible to keep the body warm, in

spite of arctic sleeping bag and blankets and overcoats so numerous that one could hardly lift the weight in breathing. On the trips in which a second night was passed at this height, more sleep was secured, indicating that the body was getting adjusted to the altitude. The headache disappeared and the appetite revived on the return trip. On the first of these trips, records were made of the pulse and respiration, as accurately as could be made by a person upon himself. They are as follows: Aug. 18, 10 p. m., at observatory on retiring, pulse 80, respiration 16; August 19, 6.00 a. m., on rising, pulse 80, respiration 16; 3.30 p. m. at tambo de los huesos, elevation 13,300 feet, pulse 96, respiration 12; Aug. 20, 6.25 a. m., at hut 15,400 feet, on rising, pulse 90, respiration 12; 12.30 p. m., at summit, 19,200 feet, pulse 86, respiration 11; Aug. 21, 10 a. m. at hut, 15,400 feet, pulse 86, respiration 11. The rather small increase in the pulse and the decrease in the respiration are noteworthy. While I made no records on other trips, I noticed frequently that my tendency was to breathe more slowly than usual, except when moving about.

WINSLOW UPTON.

PROVIDENCE, R. I.,
December 7, 1901.

SCIENTIFIC ORNITHOLOGY.

THE following remarkable misuse of terminology occurs in Mr. Robert Ridgway's 'Birds of North and Middle America,' Part I., The Finches, just from the press of the Government Printing Office. He says in his Introduction: "There are two essentially different kinds of ornithology: *systematic*, or *scientific*, and *popular*. The former deals with the structure and classification of birds, their synonymies and technical descriptions. The latter treats of their habits, songs, nesting, and other facts pertaining to their life-histories." And he continues: "Popular ornithology is the more entertaining with its savor of the wildwood, green fields, the riverside and seashore, bird songs and the many fascinating things connected with out-door nature. But systematic ornithology, being a component part of biology—the science of life—is the more instructive and therefore the more important." And are, indeed, life-habits and life-history not biology, not, if scientifically studied, science of life, not more important than the mere forms which result from this part of bird biology? Could there be found a worse misconception of where science and popular writing differentiate!

X.

SHORTER ARTICLES.

THE RESULTS ATTENDING THE EXPERIMENTS IN LOBSTER CULTURE MADE BY THE UNITED STATES COMMISSION OF FISH AND FISHERIES.

IN April, 1900, the United States Commission of Fish and Fisheries appropriated several thousand dollars to be used in devising, if possible, a practical method of artificial lobster culture, and the undersigned was appointed to take immediate charge of the experiments.

The breeding period of the lobster, continuing as it does only through a few weeks of the late spring and early summer, is so brief that extended experiments have been impossible, but the experiments that have thus far been made (during the spring of 1900 and of 1901) would indicate that very large numbers of lobsters may be hatched and retained in captivity until they have reached an age when they are well able to take care of themselves. Indeed, it would seem that the enormous mortality among lobster young (which results either from boiling females 'in berry,' or stripping the eggs from the female as the lobsters are taken from the traps) may not only be lessened, but that the young enclosed in these eggs may, with very little expense, be hatched in the more important fishing ports and hamlets and protected until they have passed through the critical stages.

It seems advisable to defer the rendering of the final report until the Commission has profited by the experiments of another season. Inasmuch, however, as the problem is of considerable economic importance, it would seem desirable to make some report at the present time, although only a report of progress.

In the spring of 1900 a number of experiment stations were established along the New England coast, namely, at Orrs Island, Freeport, Annisquam, Gloucester, Woods Holl, Naushon and Wickford. Experiments had been made previously at Woods Holl, but without encour-