sandstone; this lava was partially denuded, and buried under a conglomerate composed of its *débris*, mingled with rhyolitic, trachytic, and granitic material. The detritus was also buried under another lava-flow; and this alternating action went on, first with increasing and then with diminishing eruptive activity, until the western sandstones and conglomerates were reached, which were laid down on the last lava-flow. It is probable the lava came from fissure eruptions. Wherever the detritus was deposited on the lava, whether within the trappean belt or on its western side, denudation has taken place, and fragments of the trap (melaphyr and diabase) have been enclosed in the overlying detritus. Unconformability would, of course, thus exist, and the writer has figured such a case; but it is the unconformability that always exists when lava flows on a shore, and is subjected to the denuding action of the waves, and proves nothing regarding the geological age.

The evidence which Irving claims has been ignored, and which he says is "proof absolute that the Keweenawan [copper-bearing rocks] series belongs below the base [Potsdam] of the paleozoic column of the Mississippi" (*Geol. Wisc.*, iii. 23), is principally the finding of a trappean rock at Taylor's Falls, against which rest sandstone and shales holding fragments of the trap and primordial fossils. Excepting the fossils, these are exactly the conditions which are found, and which ought to be found, within the copper-bearing belt, and on its western side; and it proves nothing regarding geological age, but only sequence of time. If such evidence as this is 'proof absolute' of distinct geological age, then there is proof absolute that there are as many different geological formations in the copper-bearing rocks as there are detrital beds enclosed in the traps, and proof that the last lava-flow of any active volcano, reaching the sea, is separated by a distinct age and 'immense unconformity' from the detritus deposited upon it before it is hardly cold. Unconformity of itself proves nothing, unless both formations are sedimentary; for an eruptive rock cannot, from the very nature of the case, be conformable, in the true sense, with any thing. The relations that the old basaltic lavas have, according to Irving, to the western sandstone, are exactly what they ought to have from their origin, as shown thirty-three years ago.

Again: according to the Wisconsin geologists, the Taylor's-falls trap is fifteen miles from any other so-called copper-bearing rocks, and may as well be an azoic rock; for similar ones have been collected by the writer in the granite of the Marquette azoic district. If it is referred to the copper rocks on lithological grounds, the same argument could be used to unite with this series a large part of the basaltic traps the world over. The resemblance between them is, in the writer's opinion, that which any two basaltic lava-flows or dikes have wherever they may have been extruded.

The writer has shown that the first trap on the east overflowed and indurated the eastern sandstone; and he collected specimens showing the induration, the trap, and the trappean detritus in the overlying conglomerate. Therefore Irving's statements, that the eastern sandstone unconformably overlies the trap, and that no trappean detritus occurs in the fragmental rocks, are incorrect; and the published evidence was in his hands several years ago. Irving is mistaken when he says that all the geologists who approached the question from the east felt baffled, as the writings of Foster and Whitney, Selwyn, or myself, give no indications of the kind. It may be mentioned, that in 1850 Foster and Whitney showed that a fault existed along part, at least, of the eastern side of the traps, and that the Bohemian range was a later protrusion. This evidence will explain the apparent unconformity of the traps with the eastern sandstone observed in some places.

For a fuller discussion of the copper-bearing rocks and allied formations, together with the literature down to 1880, the writer would refer to the bulletin of this museum, vol. vii. pp. 1–157.

M. E. WADSWORTH.

Museum of comp. zoöl., Cambridge, Mass., March 15, 1883.

Domestic ducks that fly abroad like pigeons.

In response to Mr. Storer's note under the above heading (SCIENCE, No. 3), I would state that in my boyhood I lived on a plantation in Liberty County, Ga., on which there were a great many domesticated ducks, both mallards and musk-ducks. Many of these latter belonged to the negroes, and were tended with but little care. Near by the negro village there was a swamp full of large trees, and often covered with water. A considerable portion of the swamp was cleared, and annually planted in rice; but many dead cypress (Taxodium) trees still remained standing. This swamp was a favorite resort for wild ducks of all kinds, especially mallards, teal, and summer ducks (wood-ducks). Many domesticated musk-ducks, especially those belonging to the negroes, flew abroad every morning, remained in the swamp (one to two miles distant) all day, and returned at night. Some of them built their nests and reared their young in the swamp, though they never became thoroughly wild.

I never observed this habit, except in the musk-duck. The reason, I think, is plain. In shape, in gait, in flight, and in habits, the musk-duck is very similar to the wood-duck (sponsa). Like the latter, it walks with freer step, it rises, flies, and alights with greater ease and grace, than other species, because the wings are broader and rounder. Like the wood-duck, also, it alights on trees. The dead cypress-trees were a favorite resting-place for the musk-ducks. Like the wood-duck, too, it builds its nest on trees or stumps, and takes down the young when hatched. I have never known the musk-duck to build on the tops of tall cypresses, like the wood-duck, but often on the tops of hollow stumps fifteen to twenty feet high.

JOSEPH LECONTE.

Berkeley, Cal., March 15.

Apparent attractions and repulsions of small floating bodies.

To obviate possible misunderstandings, it may be proper for me to make a few remarks in relation to 'E. H. H.'s' *critique* (SCIENCE, i., p. 43) on my article (*Amer. journ. sc.*, Dec., 1882) on the above phenomena.

I am to blame for whatever ambiguity attaches to the use of the term 'tension' as applied to the explanation of these phenomena. In one instance (that cited) I inadvertently used the expression 'superior *tension*' instead of 'superior *force*.' But inasmuch as in the formal announcement of the capillary principle — which is applied to the case in question, and also in the preceding as well as the succeeding context — it is very clearly indicated that the effective capillary forces (and not the *surface-tension*) are regarded as inversely proportional to the radii of curvature of the meniscuses, few physicists will, I trust, be misled by the expression.

He does not admit "that a liquid film tends to draw a solid, to which it is attached, toward the centre

of concavity of the film." The most simple and satisfactory proofs of the relative efficiency, as well as the *direction*, of the resultant of these capillary forces, are to be found in the well-known contrary movements of small columns of water and of mercury, when introduced into conical capillary glass tubes placed horizontally. In these cases it is evident, that the effective forces are inversely as the radii of curvature of the terminal meniscuses, and are directed toward their respective centres of concavity.

He maintains, that, if the capillary forces were directed toward the centre of concavity of the film, "the tendency of a column of water raised between two floating bodies by surface-tension would be to lift those bodies: similarly, a column of liquid sustained in a fine tube would tend to lift the tube." Simple mechanical considerations are sufficient to show that he is mistaken in supposing that such a result would follow. Indeed, it is obvious that the elastic reaction of the common meniscus, formed when two such floating bodies are brought near to one another, *does not tend to lift them:* for the *vertical* component of the capillary forces, directed toward the centre of concavity, is exactly counterbalanced by the weight of the adhering liquid elevated between them, while the *horizontal* component is free to draw them together.

So, likewise, the column of liquid sustained in a capillary tube can have no tendency to 'lift the tube;' for it is evident that the weight of the liquid elevated must exactly balance the vertical component of the capillary forces acting at the crowning meniscus within the tube: the horizontal component tends to draw the sides of the tube together.

It is freely admitted that my explanation of this class of phenomena may be imperfect, and may be more or less unsatisfactory; but it seems to me that its shortcomings are not to be found in the directions indicated by the objections put on record by the critic. Such elementary facts as have been elicited above could not appropriately find a place in my paper.

After all, however, the simplest method of reducing this class of phenomena to the reaction of elastic films of liquids is the application (as has been done near the close of my paper) of the principle of Gauss; viz., that this reaction "always tends to reduce the surface to the smallest area which can be enclosed by its actual boundary." JOHN LECONTE.

Berkeley, Cal., March 16, 1883.

A new lecture experiment.

It has long been known, that an iron bar may be permanently magnetized by holding it in the direction of the dipping-needle, and striking it a blow with a hammer. The novelty of this experiment, so far as I am aware, consists in indicating the magnetization of the bar at the instant the blow is delivered. I use for the purpose a reflecting galvanometer (Kohlrausch's pattern), a lantern with detached lens for focusing the reflected beam (or, in the day-time, a *porte lumière*), a piece of gas-pipe 80 cm. long and 45 mm. diameter, and a coil of fine wire large enough to slip freely over the gas-pipe. After carefully demagnetizing the gas-pipe, the coil of wire is connected with the galvanometer, and slipped down against the hand, holding the pipe about 30 cm. from the upper end. With the pipe pointing in the direction of the dipping-needle, a ringing blow is struck on its upper end, and the spot of light on the screen moves promptly from two to four feet, according to the distance of the screen from the galvanometer. A second blow produces only a very small movement compared with the first one. Reversing the gas-pipe, and again striking it, the change of magnetism is indicated by another induced current about equal to the first. The direction of the current is the same as is obtained by moving the coil from the end struck toward the middle of the pipe. By moving the coil along the pipe, before the blow and after it, the induced currents indicate that the temporary magnetism of the pipe produced by terrestrial induction is much weaker than the permanent magnetism produced by the blow. H. S. CARHART.

North-western university,
March 20, 1883.

HOUGHTON FARM EXPERIMENTS.

Houghton Farm. Experiments with Indian corn, 1880–81, with a summary of the experiments with wheat for forty years, at Rothamsted. Cambridge, *Riverside pr.*, 1882. 75 p. l. 8°.

——— *Agricultural physics. Series i. Nos. 1, 2. Meteorology and soil-temperatures.* By D. P. PENHALLOW, B.S. Newburgh, *Ritchie & Hull, pr.* [1883.] 57 p., 5 pl. l. 8°.

BESIDES the intrinsic value which these publications have as reports of carefully conducted experiments, they possess additional interest to all who have at heart the advancement of scientific agriculture in this country, because they are the first public reports of what is here a novel undertaking. The proprietor of Houghton Farm, Mr. Lawson Valentine of New York, has, in effect, established upon it an experiment-station devoted to the scientific investigation of agricultural questions. So far as we are aware, this is the first institution of the kind in the country supported by private munificence, and hence untrammelled by the demand for results of immediate practical utility, and by the mass of miscellaneous chemical work which seriously circumscribes the scientific activity of public experiment-stations. The outcome of this form of the 'endowment of research' will therefore be awaited with much interest.

The first of these reports gives an account of the field-experiments with Indian corn, executed by Dr. Manly Miles in 1880 and 1881. These experiments are, in the main, modelled after the famous Rothamsted experiments of Lawes and Gilbert, and are to be continued through a series of years, with the design of doing for Indian corn what the English experiments have done for wheat and barley. The experimental plots having been laid out and drained in the previous year, a crop of corn was grown in 1880 *without manure*, in order to test the uniformity of the soil and establish a basis for subsequent comparisons. This was followed in 1881 by a crop to which various kinds and quantities of manures were applied on the several plots, certain plots being left unmanured for comparison.