the time there was not wind enough to do more than to swing the spider to the same angle from the vertical that he was then making above the horizon. It seemed the more surprising, as the spiders were large, and ought, by all the laws of gravity, to have fallen to the earth at once. And what was their objective point, aiming, as they did, for the clouds and stars? But I content myself with the statement of the facts, leaving to others the how, why, and whither.

E. T. QUIMBY.

Hanover, N.H.

Improvement of western pasture-land.

In his article in SCIENCE, p. 186, Professor Shaler's opening sentence, "that the greater part of the United States west of the meridian of Omaha is unfit for tillage," leaves a somewhat wrong impression. The greater part of Nebraska is west of that meridian; but nearly the whole state, as far as longitude 99°, produces crops of the cereal grains, grasses, corn, fruit, and roots, more surely, even, than the middle states. This area embraces 30,000 square miles. Large sections west of the 99th meridian produce almost equally well, as our statistics show. His suggestions, however, apply to the proper management of the grasses outside of this area, and are of very great importance.

A remarkable peculiarity of our Nebraska flora is its changing character. While not confined to the grasses, it is especially conspicuous among them. When I first crossed this county (Lancaster) in 1865, buffalo-grass (Buchloe dactyloides) covered much of the uplands. By 1871 nearly all of this species had disappeared; and its place was taken by blue-joints (Andropogon furcatus, etc.), interspersed with Boutelouas, Sorghum nutans, Sporobolus, etc. Again, in 1878, the blue-joints disappeared from entire townships, and the Boutelouas usurped their place. Similar phenomena were observed in almost every county in the state, and even in sections where settlements had not penetrated. During the last two years Sorghum nutans has been gaining in eastern Nebraska over all other species. On the whole, the species indigenous to moist regions have been gaining on the buffalo-grasses to such an extent that the latter have almost entirely disappeared east of the 100th meridian, and from large areas farther west. In extreme north-western Nebraska, on tributaries of the Niobrara, I have observed, since 1865, a remarkable exchange of buffalo-grass for Boutelouas and other grasses in different years. This tendency, therefore, is common, though not to the same extent, in the drier as well as the moister portions of the state. When old Fort Calhoun, above Omaha, was occupied by the military, twenty-five years ago, Kentucky blue-grass was brought in baled hay to that post from the south. It spontaneously took root, and spread in every direction, and now it can be found on prairies thirty miles away. Many of our farmers in eastern Nebraska are looking to that species now for a grass to give late fall and early spring pasturage.

Under favorable conditions, the wild native grasses produce a remarkable amount of hay. The blue-joints range in productiveness from one to three tons and more per acre. The latter large yield has been realized even at the 99th meridian on the wide Elkhorn-river bottoms. All the facts noted in the moist as well as dry sections of the state confirm Professor Shaler's theory; namely, that the natural conditions on the plains are most favorable to a changing grass vegetation, and that it is possible, through the agency of man, greatly to improve on the native species.

SAMUEL AUGHEY.

Apparent attractions and repulsions of small floating bodies.

As I thought it worth while, in the interests of clear teaching, to object (SCIENCE, i. p. 43) to certain things in Professor John Leconte's explanation of the 'Apparent attractions and repulsions of small floating bodies,'[1] it seems my duty, now that Professor Leconte has replied (SCIENCE, i. p. 249) to my criticism, to justify that criticism, or, failing in that, to acknowledge my error.

A statement in his explanation of the behavior of two moistened floating bodies, to which I particularly objected, was the following: "But when brought so near that their meniscuses join each other, the radius of curvature of the united, intervening, concave meniscus . . . is less than that of the exterior concave meniscuses, . . . and its superior tension acts upon both bodies toward a common centre of concavity."

The parts omitted from this sentence are merely references to a diagram. Professor Leconte now states that he should have said *superior force* instead of *superior tension*. I, however, objected to the statement on quite other grounds. After quoting it, I said, "We do not think physicists generally will admit that a liquid film tends *to draw a solid, to which it is attached, toward the centre of concavity of the film*. Indeed, if this were so, 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."

I have quoted myself thus at length, — using italics, which I did not use before, — because Professor Leconte appears to understand me as denying that what he calls the 'capillary forces' — such, for instance, as the force exerted upon the enclosed air by the film of a soap-bubble — are directed toward the centre of concavity of the film. I spoke merely of the force exerted upon *the body to which the edge of the film is attached;* and the force exerted by the film upon such a body is certainly not directed toward the centre of concavity of the film. If we coil a rope round a cask, and set a man to pull at each end of the rope, the pressure on the cask will be everywhere directed toward the centre of curvature of the coil: but the pull on the men will not be toward the centre of curvature of the coil; it will be tangential to the coil. In the same way, the action of a meniscus upon the water beneath it, or the air above it, is directed toward the centre of concavity of the meniscus; but the action of the meniscus upon the body to which it is attached is tangential to the liquid surface, and perpendicular to the bounding edge of the meniscus.

Professor Leconte, however, has chosen to make the statement I have quoted above; and to my criticism thereon he replies, "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." He makes a similar statement concerning the action in a capillary tube.

It is, indeed, obvious, that the weight of the water must be sustained; but how and where is this weight applied to the floating bodies or to the tube? If it is applied by means of the surface-film, and at the line where the bounding edge of that film meets the floating bodies, or the wall of the tube, Professor Leconte's

[1] Amer. journ. sc., December, 1882.

final statement of the case of two floating bodies apparently comes to this: that the concave meniscus "acts upon both bodies toward a common centre of concavity," and also exerts upon these bodies a vertical downward force equal to the weight of the water sustained. If this is Professor Leconte's conception of the case, I do not feel to blame for not understanding him at first.

If, on the other hand, he supposes the weight of the water to be applied to the floating bodies, not by means of the surface-film, but in some other manner, it was, I submit, incumbent upon him to explain how and where he supposed it applied.

So much in explanation and support of my criticism of Professor Leconte's original statement. It is now, perhaps, worth while to examine a little further his final statement, as quoted above, beginning, "Indeed, it is obvious." Does not this statement, taken in connection with his first statement, also quoted above, lead directly to the conclusion that he supposes a column of water may be sustained between two bodies by capillary action without exerting any *resultant* downward force upon these bodies? — that, in short, the water is pulled *up* without any resultant tendency to pull the bodies *down?*

I have written thus at great length, and with perhaps unnecessary statement of elementary principles, because I intend this letter to be final upon my part.

Edwin H. Hall.

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THE INDIANA GEOLOGICAL REPORT.

Indiana: department of geology and natural history. Eleventh annual report (1881).. John Collett, state geologist. Indianapolis, *State*, 1882. 401 p., 55 pl. 8°.

This volume contains some interesting scientific and economic matter, partly original, but largely in the form of useful reprints of things not accessible to the people whose needs it is meant to serve.

There is, in the first place, the report of a well-made inquiry into the transverse strength and elasticity of building-stones, principally of the excellent oolite of the St. Louis division of the sub-carboniferous limestones. The point is well made, that the resistance of hammered blocks of stone to compressive strains is very much less than that of sawed masses, owing to the unseen disintegration of the mass produced by the blows of the hammer. There is also the noteworthy suggestion, that the modulus of resistance to compression may be approximately estimated by the 'ring' of the mass when struck.

There are several county reports which have no general value. They contain some venturous discussions of the extremely difficult problems connected with the work of the last glacial period in the Ohio valley. Glacial rivers, glacial lakes, ice-fronts, and all the other machinery of that time, are handled with charming ease and dexterity. We only hope the observers will work past this first transparent stage of the inquiry, and find how beyond imagination hard is this task of explaining the work of the ice-time, and how useless are such vague conjectures unfortified by the amplest delineation of facts.

In the report of Mr. Collett on Shelby county, we find the very interesting statement, that, in several wells sunk in one part of this county, heated waters have been struck within fifty feet of the surface. Nothing is given concerning the amount of flow of these waters or their chemical composition, nor are we told any thing concerning the goodness of the thermometers with which the observations were made, — all very important points. We only have the statement that the water was not potable, and that its temperature was as high as 86° F. As this district is below the level of the carboniferous series, it may not be reasonable to suppose that the temperature is due to the decomposition of iron pyrite, the only considerable known sources of that mineral available in this district being in the coal-measures. It is perhaps more probable that the temperature is due to downward penetration and return of water in a system of faults, which we must suppose to extend to a great depth, though they do not manifest themselves on the surface. If the waters are highly sulphurous, the origin of the heat in the decomposition of pyrite is the most probable; if they are not sulphurous, their source must be sought in faults. The question merits a careful study.

Two hundred pages of the text, and thirty-two of the plates, are reprints of James Hall's Waldron fossils, with some emendations, including four new plates.

Dr. Charles A. White gives a series of plates and descriptions of fossils from the collection of Mr. J. W. Van Cleve. Hall's monograph is well known to but few. It was originally published in the twenty-eighth report of that mysterious body corporate, the regents of the university of New York. This is the first publication of it that could have been of any use to Indianian students.

The species described by Dr. White are chiefly corals, and are not regarded by the author as new species. This part of the work is essentially of local interest. All the species have been better set forth before, but never in a form so accessible for the dweller in the rural parts of Indiana.

Although there is not much that is original in this book, it very likely has a higher measure of utility for the people who pay for it than many a survey report that has better served the purposes of pure science. The old