

"Silver Top" in grass is caused by the working of leaf-hoppers, and that the *Jassidae* furnish many of the insects as well as those in the families named in this paper. In our Park meadows, some of which are left to develop hay, "silver top" is very common, especially on the earlier grasses, and throughout the season a weakening and deforming of grass stalks are everywhere noticeable, which in most cases, no doubt, is due to the extraction of the juices by these insects. The node of the grass, especially on the upper side and for an inch or more, is very tender and succulent; but as we traverse the internode, we find it becomes more firm and woody, as it were. Every boy knows where to find the succulent portion of a grass-stem, and proceeds to pull it out, when it breaks just above the node at the tenderest place. This succulent feeding ground is soon discovered by the "hoppers" in their tours of prospecting up and down the stem; veritable "sappers, if not miners," they are often seen in numbers in such situations, and the punctures can also be noticed. The exhausted stems of the dead part of the grass culm show every sign of having been pumped dry by these creatures, as at that point nothing seems to be left but a bundle of woody fibres, and the internode for some distance diminishing in size from the loss of sap, and that loss occurring before the cells had been sufficiently developed to stand without collapsing. My success in finding Thrips, or *Mero-myza*, in the stems or under the leaf-sheaths has been no better than Professor Osborn's.

As I am working on a list with food-plants and habits of Hemiptera for New York State, I should be glad to receive from collectors information in regard to those found here and their distribution elsewhere, so that the list can be made as complete as possible; for all "local lists" are of the greatest value, not only to local students but to students of North American entomology also. EDMUND B. SOUTHWICK.

LETTERS TO THE EDITOR.

*** Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.

On request in advance, one hundred copies of the number containing his communication will be furnished free to any correspondent.

The editor will be glad to publish any queries consonant with the character of the journal.

Four-Fold Space and Two-Fold Time.

ANY attempt at expounding popularly the recent developments of the old idea of space should be prefaced by the explanation that their tremendous value to mathematics is utterly independent both of their external reality and of the possibility of their realization. For example, had either M'Clelland or Preston ever glanced through Bolyai's "Science Absolute of Space," we would not to-day read in their excellent "Treatise on Spherical Trigonometry," p. 10, "The student must be careful, however, not to regard a solid angle as an *area*, but as a mere *number*, like the circular measure of a plane angle. . . . and the solid angle subtended at O by the $\frac{1}{n^{\text{th}}}$ part of the surface of the sphere is $\frac{4\pi}{n^{\text{th}}}$, which is thus a mere number." A solid angle is a magnitude as different from a mere number as is the current of electricity which kills a man. Though its scientific unit, the *steradian*, is American, yet they could have found it in the "Encyclopædia Britannica, in William Thomson's article "Measurement."

Because these magnitudes, solid angles, have a natural unit, the *steregon*, and a scientific subsidiary unit, the *steradian*, therefore mathematicians, unused to the idea of a natural unit, blunder about them. To Bolyai belongs the honor of showing that each geometric magnitude has its natural unit, which never could have been discovered in Euclidean space, since

homaloidal, parabolic space appears as a limit in which the natural unit for length becomes indefinitely great, so calling, in practice, for an artificial unit for length, a finite sect, as the centimeter. The fundamental importance of the pseudospheric hyperbolic space of Bolyai and Lobatschewsky in no wise depends on whether C. S. Peirce is right in maintaining that such is the real space in which we live. It has already enriched us eternally by the gift of the Science of Comparative Geometry, and so of pure spherics.

Now, in his beautiful paper in Crelle, on "Single Elliptic Geometry," Professor Newcomb has used, unnecessarily we think, space of four dimensions. Elliptic space, though finite, is unbounded. But there is a sense in which hyperbolic space, though infinite, is bounded, and so its realization is naturally connected with that of four-fold space. For this the most fruitful idea has ever been Professor Sylvester's, of working up from two-dimensional beings. And here let me say that thinkers must not confine themselves as in the past to "an imaginary *plane* being," but must likewise draw from two dimensional spherics and pseudospherics. Not only must we think of a *flexible* closed shell turned inside out, as we turn a glove; we must try if we can realize that as the flexibility of the "thin hoop" mentioned by Dr. Hall is only needed because the hoop has as many dimensions as the space in which we wish to turn it, therefore can we not turn an inflexible closed shell, an unbroken eggshell, inside out, *without flexure*?

The corresponding generalizations for *time* are harder, because in time's domain we are one-dimensional beings; therefore our best space-method fails us. Cannot genius give us a next-best almost as good?

GEORGE BRUCE HALSTED.

University of Texas, Austin, Tex., May 22.

Family Traits.

IN your issue of May 20, "Veritas" again combats the proposition that family traits are a reality. The statement was made in my original communication that questions of descent were questions of heredity and environment, and that heredity, consisting as it does of questions relating to the reproduction of the race as an animal, must be referred to biology. The results of all departments of research for the last fifty years refer man to his place in nature as an animal, and as an animal a fit subject for biological investigation.

Will Veritas please explain how, if it be admitted that a man may resemble his father and grandfather, that with the great-grandfather the resemblance must cease? Is not every man the son of his father, and is his father not also the son of his grandfather, and so on from generation to generation back in an infinite series? If a man may or does resemble his father, that is the limit of the question, and further argument is unnecessary.

I freely admit that the Does of the present know, of their own knowledge, nothing of 100 per cent of the traits of John Doe the first. I was not referring to any positive knowledge held in this generation of the ninth generation from the present. Nevertheless John Doe of the seventeenth century had personal traits, and if the oldest Doe now living has seen and known 100 Does in perhaps five generations, and affirms that, out of the almost infinite diversity of traits that constitute human character, a few have been observed in all these generations common to a large majority of the 100 Does, I assert that there are "Doe" traits or "family traits," and in my former article I gave a scientific explanation of the occurrence of such traits, with a number of illustrations that were neither a figment of the imagination nor a delusion. If these are not facts, what are facts?

Moreover, in your issue of April 15, Ed. H. Williams Jr. gave a number of other reasons why family traits should descend on the male side strongest, that are either "facts" or delusions. Facts are established by observation of the repeated recurrence of identical phenomena under like conditions.

What fact is developed by speculation concerning a man's ancestor of the seventeenth century relative to the occurrence of traits common to forty people now living, who all bear his surname, and who are likewise his descendants? Suppose these

forty people are cousins to the entire population of Boston; does that relation either prove or disprove their possession of traits in common that do not appear among 40 other cousins of 40 different names?

ENQUIRER.

The Systematic Position of the Diptera.

In reply to the article, by Professor J. M. Aldrich, in *Science* of April 26, I will say that the Diptera are undoubtedly the most highly specialized order of insects. Professor Hyatt deserves credit for apparently being the first one to call attention to this idea and offer proofs to sustain it. His idea of what constitutes specialization is correct, from my standpoint. I realize the force of Professor Aldrich's remark, when he says that this line of reasoning will apparently lead us to the conclusion that the Pupipara are the highest insects; but I am inclined to believe that the higher families of Cyclorrhapha will be found to exhibit greater specialization. Everything points to their being of the most recent origin. If anything has been published on the the embryology of the Pupipara, I am not aware of it. Embryological research alone will clear up this point, and there is no reason in the world why it should not show that the Pupipara have long ago passed through stages now occupied by apparently more highly developed Diptera, but it is improbable, to say the least. If it can be demonstrated, I shall be glad to accept the Pupipara as the highest insects.

C. H. TYLER TOWNSEND.

Las Cruces, N. M., May 26.

A Botanical Congress and Botanical Nomenclature.

I HAVE read your note of May 20, with reference to the action of the Botanical Club of Washington upon the questions of a botanical congress and botanical nomenclature. It has always seemed to me wise for American botanists first to agree among themselves in reference to this matter, and then they will be in a better position to come to an understanding with the rest of the world. There is no more representative body of American botanists than the Botanical Club of the American Association. It is perfectly democratic; every botanist in the country can join it and take part in its discussions. It may be questioned whether its boundaries are not too ill-defined to make it a fit body to decide such important questions; but it is surely a capital place for their discussion.

JOHN M. COULTER.

Bloomington, Ind., May 25.

AMONG THE PUBLISHERS.

THE first number of the *Yale Review*, a quarterly journal of history and political science, has just been issued for the month of May. It is edited by Professor George P. Fisher and others of Yale University, but will welcome contributions from every quarter and from every school. The papers in this opening number are on "German Tariff Policy, Past and Present," by Henry Villard and Henry W. Farnam; on "The Demarcation Line of Pope Alexander VI," by E. G. Bourne; on "Legal

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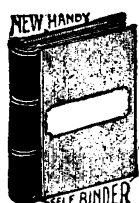
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