is, there would result a quantitative division of the material representing the character in question. This brings us back, at least so far as certain characters are concerned, squarely to the position taken by Morgan last year in his paper: "Chromosomes and Heredity" (*Amer. Nat.*, 44: 449–496).

While the hypothesis there presented, including the proposition that the plane of division of homologous chromosomes may be at any angle to the plane of union and the assumption that a certain quantity of the material representing a character must be present in order that the character develop, will doubtless account for the results (ratios) obtained in F_2 of a cross, *it certainly will not account for the purity of extracted recessives and dominants as exhibited by their behavior in* F_3 *and later generations. To overlook this is to neglect the fundamental part of Mendelism.*

A hypothesis that does not explain how extracted recessives can breed true generation after generation without the production of so much as a single individual having the dominant character will hardly be accepted by present-day students of genetics.

R. A. EMERSON

HIBERNATION

TO THE EDITOR OF SCIENCE: The *Popular Science Monthly* of October, 1910, published an article of mine entitled "The Natural History and Physiology of Hibernation," pp. 356–364. Since this article appeared some additional facts, in natural history, have been brought to my notice as well as some errors bearing on this subject. It is the purpose of this letter to note the former and to correct the latter.

On page 359 will be found the sentence: "Many butterflies and moths hibernate in the perfect state as well as in the form of imagos, but not in the larval state (?)." The correction here is that "*pupa*" should be read instead of "*imagos*." In the latter part of the same sentence, "but not in the larval state (?)" the statement is wrong, for several butterflies and a great many moths hibernate in the larval state, notably the Noctuidæ and Arctiidæ, consequently the query mark should be abolished.

Again the statement is made, "Insects which hibernate do not pair until spring and bees do not hibernate at all." This sentence is not quite full enough and demands more detailed information or additional light on the subject. All our wild bees, wasps and some others pair in the autumn and the fertile females hibernate. Hive bees, on the other hand, pair in the spring and do not hibernate.

The statement will be found on page 360: "but curiously enough no case [hibernation] is known among birds." I must still hold to this notwithstanding the following account, which may interest your readers, furnished to me in a private communication, by Mr. C. W. Nash, biologist to the Ontario government. I quote in full: "I have found evidence (of a sort) which leads me to believe that the Purple Martin and Chimney Swift may at times become partially dormant and I have recently received from an eye-witness an account of the cutting down of a hollow tree near Peterboro, in the month of January, many years ago; this tree is said to have contained hundreds of swallows in a dormant state, some of which were revived. I have the names of other witnesses of this curious incident and am looking them up."

In support of this suggestive phenomenon one can say that we do not know what part of the world the Chimney Swift does migrate to for the winter but it would be well to remember also that we are equally ignorant of the path the Arctic Tern takes to and from its winter and summer homes, 11,000 miles apart! We are still lamentably ignorant of a great many things about birds.

Mr. Nash supports the statement on page 360 that a low temperature was not the only cause of hibernation. He experimented with Black Bass and found that when the fish were "kept in a warm room they ceased to feed at the end of October and resumed again in March, though they never became dormant—in fact were just about as active during the winter as at any other time, though in nature I do not think they are so." I trust that these

corrections and additions will prove of value to the student of natural history as well as increase the interest in the phenomenon of hibernation.

ALLEN CLEGHORN

DRAFTS AND COLDS

TO THE EDITOR OF SCIENCE: Does it often happen that a writer opposes his own claims so neatly and conclusively as in "Drafts and Colds" in your issue of the 22d?

Those who read about the 16-inch fan will think of the generous third that rests upon a feather or other mattress and is correspondingly warmed while two thirds is fan-swept.

How could a more complete "disturbance of the thermo-neural equilibrium of the surface of the body" be secured and maintained?

(Other inventors prefer the cool blast from an exactly opposite direction.)

H. F. DUNHAM

September 26

"WASHINGTON SCIENCE"

THE communication in SCIENCE of September 29, with the above title, signed "Washingtonian," has doubtless been read with interest by many scientific men in this city and elsewhere. However, the article contains no mention of one feature in government work which in the mind of the present writer constitutes an obstacle to scientific work and a serious defect in the plans of more than one branch of the service. This is the placing and assumption of too much executive responsibility in scientific bureaus in the hands of unscientific subordinates to the chief.

The justification for "red tape" given by "Washingtonian" should have been applied to "system." In this sense the arguments can not be successfully controverted nor is there much more necessity for system in government work than exists in many universities or business houses. The head of a scientific bureau or department, however, has no desire to give routine matters his personal attention and wisely delegates the keeping of records and accounts to a chief clerk or executive assistant known by some other title. It is, however, too often true that these subordinates overstep their original authority and gradually assume more. They may and often do become autocrats in their respective departments or bureaus. "System" then rapidly becomes "red tape." We then have the spectacle of a man without scientific training or experience dictating to the scientific corps how they shall conduct themselves.

Scientific work in an atmosphere such as this is aggravating and, while many of the annoying circumstances are too small to carry to the bureau or department head, as a whole they constitute an objection to scientific work in government service sufficient to have driven more than one good man away. A university may employ a purchasing agent or bursar, but it would, I trust, be difficult to find cases in which a department head receives practically all of his orders from such employees. In Washington it is not so difficult to find such cases. The fault has its origin in the executive's distaste for the details of system, but too often involves the whole corps in the maze of regulations made by men who have nothing else to do and who hunger and thirst for authority. In view of these actually existing conditions the writer may perhaps be excused from openly currying disfavor with the real powers by signing himself merely

ANOTHER WASHINGTONIAN

CHANGES IN THE PERSONNEL OF THE INTERNATIONAL COMMISSION ON ZOOLOGICAL NOMENCLATURE

THE members of the International Commission on Zoological Nomenclature have unanimously invited Professor K. Kraeplin, Direktor des Naturhistorischen Museums, Steinthorwall Hamburg, Germany, to serve on the commission until the next International Congress, in the place of Professor Maehrenthal, deceased; also Dr. P. Chalmers Mitchell, secretary Zoological Society of London, Regent's Park, London, N. W., in the place of Dr. Boulenger, resigned.

CH. WARDELL STILES,
Secretary

October 10, 1911