from a least-square adjustment of all the observations between 1580 and 1890 : —

$$D = +6.24^\circ + 17.75^\circ \sin [0.7^\circ (t - 1850) + 112.7^\circ] \text{ - - - - (1).}$$
$$\pm .10 \qquad \pm .2$$

Where D stands for the declination at any time t, positive when west, and $0.7^\circ = \frac{360^\circ}{514}$ = angular motion in one year. The differences between the observed and computed values would seem to indicate a fluctuating smaller period of about 80 years, having a variable parameter of about $\frac{1}{4}^\circ$. Somewhat similar results were found at Paris by Mr. Schott, and at Christiania by Professor Weyer. No attempt was made at present to establish this second term, it lying within the probable error, which for a computed result is $\pm$ 20′.

The inclinations date with Norman's in 1576. The observations between this date and 1891 can be represented by the following formula : —

$$I = 70.40^\circ - 3.98^\circ \sin [0.7^\circ (t - 1850) + 23.0^\circ] \text{ - - - - (2).}$$
$$\pm .065 \quad \pm .09 \qquad \pm .1$$

Where I = inclination at any time t and the period involved, 514 years — apparently the same as for (1).

The probable error of a computed value is $\pm$ 10′, which, considering the material, is satisfactory. Both (1) and (2) apply to latitude 51° 30′ and longitude 0° 07′ west of Greenwich. The mean of the Greenwich and Kew observations was taken to apply to this station. Comparing (1) and (2), a remarkable result peculiar to this station will be noticed — that the epochs are practically complementary, hence the following approximate relation between the declination and dip can be found : —

$$\frac{\delta^2}{(17.75)^2} + \frac{\iota^2}{(5.98)^2} = 1 \text{ - - - - (3).}$$

Where $\delta = D - 6.24$ and $\iota = I - 70\ 40$.

From (1) and (2) the following results are obtained : —

	Declination.		Inclination.	
	Amount.	Time.	Amount.	Time.
Maximum	24.0° W.	1817.5	74.4°	1688.5
Minimum	11.5° E.	1560.4	66.4°	1945.7
Range	35.5°	— —	8.0°	— —
Mean	6.2° W.	1689 } 1946 }	70.4°	1560 } 1817 }
Zero	0.0°	1660 } 1976 }		

From which it appears that for London the mean declination takes place about the epoch of maximum and minimum inclination, and *vice versa*.

With the aid of formula (1) and (2) the curve described in space by the north end of a free magnetic needle was now accurately constructed and graphically exhibited. It was shown that the first approximation of the curve could be taken as a spherical ellipse, the period being about 500 years. For Paris, a similar result was obtained and exhibited, using provisional formulæ. For both stations the curve lay to the greater part west of the true meridian, and the direction of the motion (standing at the centre of the needle and looking towards the north end) was that of the hands of a watch, or opposite to that of the earth. A rough survey was then made of the globe in an easterly direction approximately in the latitude of London, and elucidated by a diagram. It was found that the needle was farther along in its secular orbit at every station. The curves for some stations in the southern hemisphere were also exhibited.

The following conclusions were reached : —

1. The direction of the secular motion of the north end of a free magnetic needle in both hemispheres is that of the hands of a watch.

2. That if the secular orbit is a *single closed curve*, then are the periods *different* for different stations.

3. That if the period is a *common* one, then must the orbit be a closed curve of two or more loops lying within each other. We are getting then, at present, a small loop in America and a larger one in Europe.

4. That our present feeling is rather that, strictly speaking, we have no such thing as a *period* of secular variation, but that the needle partakes of a sort of spiral motion, returning approximately to a former position, but never exactly so.

Future study may possibly modify some of these conclusions. The possibilities opened up by such a study as outlined were next briefly alluded to, and reference made to a possible extension of the well-known Gaussian analysis by the introduction of the variable t — time.

In conclusion, can we not say with Sabine : "Viewed in itself and its various relations the magnetism of the earth cannot be counted less than one of the most important branches of the physical history of the planet we inhabit."

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.

On Biological Nomenclature.

I HAVE read with interested attention the discussions by botanists in *Science* on this subject. It would appear that they are fully alive to the need of some canons of nomenclature in their branch of biology — a need which has been felt, and supplied of late years, by the zoölogists.

The nomenclature of botany has always seemed to me to be more stable and uniform than that of zoölogy, for the reason, as I supposed, that the naming of new genera and species has, for the most part, been reserved to a comparatively few leaders in the science; and the same cause has contributed to the fixity of botanical classification, in comparison with the incessant taxonomic fluctuations which zoölogy has suffered.

With the late great increase in the number of working botanists, the distinction of a small select "caste" of authoritative namers and describers in botany seems to be breaking down, with the various good and evil results attending this transfer of power from a privileged oligarchy to more democratic rule.

I think that not improbably the botanists who are now exercised over names may examine with much confidence the canons of nomenclature lately formulated and rigidly enforced by the American Ornithologists' Union. These rules have been found to work admirably in practice. They may not be the best possible, but on the whole they are the best extant. A number of leaders in other departments of zoölogy, besides ornithology, as in mammalogy, herpetolcgy, ichthyology, malacology, entomology, etc., have found them entirely available. If some two or three of the rules are not so acceptable as the rest, yet it seems to be generally conceded that it is better to abide by them all, than to dissent from the code as a whole on account of a few comparatively unimportant points that may not be liked so well as the rest are.

Referring to the excellent article of C. H. Tyler Townsend in *Science* of Sept. 16, it seems to me that the moot points he raises have all been considered carefully by the ornithologists, who have settled each of these questions to very general satisfaction; and that the considerations upon which their conclusions have been reached are entirely applicable to the botanical questions involved.

I wish to say a word respecting the somewhat epigrammatic rule, "once a synonym, always a synonym," for the form of which I am measurably responsible, if I remember rightly. Like any other curt sententious saying, the rule is, as I perceive by Mr. Townsend's remarks, liable to be misunderstood. There is no question that, as he correctly says, "If a form which had been described and then thought to be the same as some other species, is later proven to be a valid species, the name originally proposed should stand." Certainly it should. That is not the application at all of the phrase "once a synonym, always a synonym;" and I never heard before of its application to the case Mr. Townsend adduces. What the aphorism really means is best illustrated by a concrete example:

Let there be a genus *Smithia* in botany. Let a genus *Jonesia*

then be named. Let *Jonesia* then be found to be the same genus as *Smithia*. Then the name *Jonesia* "lapses into synonymy" and cannot be thereafter applied *to any other genus* in botany. That is all that is meant by the saying "once a synonym, always a synonym." In other words, if *Jonesia* is not good for what it originally meant, it is good for nothing; it is to be deleted absolutely, and cannot come into re-existence by transfer to any other genus.

Exactly the same principle holds for all specific names within their respective genera. Example: Let there be a *Rosa Smithi*. Let some one then name a *Rosa Jonesi*. Let *R. Jonesi* be considered to be the same species as *R. Smithi*. Then there can never be a *R. Jonesi;* that is to say, *no other species* of *Rosa* can be specified as *Jonesi*. But, of course, if anyone discovers, after this reduction of *Jonesi* to a synonym of *Smithi*, that what had been called *R. Jonesi* is a good species, then *Jonesi* revives as the name of *that* species; and the fact that it had been (erroneously) regarded as a synonym of *Smithi* is no bar to its use in its original sense.

So the expression, "once a synonym, always a synonym," is seen to hold perfectly good in its proper acceptation The fact that a certain name has ever been wrongly regarded as a synonym does not make it a synonym; for it ceases to be such the moment the mistake is detected and corrected, and therefore is not amenable to the rule at all.

I think that, on this reconsideration of the subject, Mr. Townsend may be himself the first to affirm the validity of the now famous maxim, and I am sure that, if he does so, he will find it works well. ELLIOTT COUES.

Smithsonian Institution, Washington, Oct. 10.

Crayfish Attacked by Leeches.

WHILE walking on the beach at Lake Chautauqua one day, recently, I observed a crayfish about four inches in length lying just at the edge of the water, where it had apparently been thrown up by the waves.

On picking it up, I found that it had moulted but a short time previously, and that its new shell was still quite soft. As I lifted the animal, I was surprised to see five large leeches, the smallest of which in its semi-contracted position extended nearly three inches, hanging from the body, and upon a closer investigation observed that all five were attached to a single portion — the left chela. The part which had been attacked by the leeches was the area of attachment of the adductor muscle; and, if the work had not been interrupted by my examination, it would have resulted in the complete crippling of the pinching apparatus of that side. Other and seemingly less protected portions of the body were uninjured.

It would be interesting to ascertain whether the point of attack in this case was accidental or determined by intelligence, but the appearance was that the leeches, appreciating that their prey was just at this time incapable of protecting itself, had deliberately attacked the animal in such a way as to prevent it from protecting itself in case its shell should sufficiently harden before they had succeeded in killing it. The right chela had one slight perforation in it, in the same location, and it is possible that a leech had begun there also, but dropped off unnoticed when the crayfish was raised from the ground. I should be glad to learn of any other observations on the way in which leeches attack their prey.

H. T. FERNALD.

State College, Centre County, Pa., Sept. 27.

A Wasp Study.

NEAR my summer home we have large numbers of the small solitary wasp (*Eumenes smithii*). The mother-wasp digs a passage and cell, usually in the open sandy pathway: our pathways show hundreds of these wasp-holes, about one-half an inch in diameter, while the work is going on and before the cells are closed. The egg having been laid, the mother-wasp provides a caterpillar or two, which she leaves in the nest in a state of coma or paralysis; this coma lasts until the young wasp is hatched, when it finds fresh living food ready for it.

About the time when the Eumenidæ are busy with home-building and egg-laying, we usually have on our wild cherry-trees and young poplars large numbers of the nests of small caterpillars. This year I noticed a remarkable absence of caterpillars; scarcely a web-nest was to be seen. It did not occur to me to wonder what food Madame Eumenes would provide for her babes in this famine of caterpillars, until one day I was treated to a curious spectacle. I saw a slender blue-black wasp about an inch long, carrying off a large gray grasshopper. The grasshopper was fully two inches long, large and heavy in proportion to its length, a handsome insect of a greenish-gray, with some pale yellow touches and markings.

The wasp lay upon the caterpillar, its thorax upon the thorax of the caterpillar, and its sharp-pointed black head resting exactly between the large, full eyes of the captive. The small fore-legs of the wasp clasped the upper part of the caterpillar's thorax; the wasp's third pair of legs lay along the thighs of the large hind-legs of the grasshopper. Clasping its prize firmly with its first and second pairs of legs the Eumenes flew, carrying the grasshopper. Each flight was short, not over from five to ten yards; then the wasp settled, and before flying again made some little progress between walking and flying, dragging the grasshopper beneath it in the position just described. The course of the wasp was in a direct line. It followed the path for a time, but where the path curved or deflected the wasp moved directly over bushes, stubble, and long grass.

Meanwhile, the grasshopper was absolutely quiescent, and had I not known the wasp's penchant for living prey, I should have believed it to be dead. I followed captor and prisoner for some fifty yards, and then seized them both. The wasp clung to her prey, and seemed so absorbed in that as to be heedless of capture. I took the two home in my hand, holding by the body of the grasshopper, put them on a plate under a goblet, and proceeded to examine the case.

The wasp was not biting or stinging the grasshopper, but merely held it firmly clasped, the rigidity of the heads of both insects being very noticeable. The extremity of the abdomen of the wasp trembled slightly; the eyes of the grasshopper had a very peculiar, dull, unseeing expression, like those of a person in a state of coma; occasionally the grasshopper's large thighs quivered, and constantly there was a slow expansion and contraction of the abdominal rings. Finally I forcibly removed the wasp from the back of the grasshopper, and placed the latter on the floor in a draught of air. In a moment or two it seemed to recover itself slightly, stretched all its legs, and gave a feeble hop. I then set the wasp free within a foot of the grasshopper, and seeming to recognize its booty, it dashed upon it, and took the same position as before. There was no biting of the head of the grasshopper. I watched both closely. After this second capture the grasshopper rapidly succumbed: its first pair of legs curled up closely; the second pair folded together into a kneeling posture; the hind-legs were extended, quivered no longer, and the abdominal expansion and contraction were feebler and slower.

At the end of twenty minutes I removed the wasp, carried her to the other side of the house and set her free. She departed as if reluctantly "enforced to go and seeming still unready," evidently all her mental powers, whatever they might be, were concentrated upon that grasshopper. I returned to the grasshopper, and found it giving no signs of life except the abdominal motion. I then sprinkled it thoroughly with ice-water. It recovered a little, moved its thighs several times, but the contraction of the first and second pairs of legs, and the motionless, stiffened state of the antennæ, were very marked. In whatever position I put the insect there it remained, on back or side, or propped up on its bent, "kneeling" fore-legs. The slight reviving produced by air, water, and freedom from its enemy did not last. The grasshopper grew more rigid and the ring expansion less and less marked. I desired to keep the creature to discover how long it remained uncorrupted in this state, but a sharp gust of wind blew it from my hand into grasses where I could not recover it.

The manner of the carrying of the grasshopper by the wasp, the strength exhibited by the wasp, its absorption in its prize, the peculiar resting of its head motionless upon the upper portion of