Limnæa. Again, Mr. W. Doherty, writing from Cincinnati, records a remarkable dentate variety of *Conulus fulvus;* he further remarks that dentate species of *Helix* are the forms there prevalent, and points out that this formation is useful in obstructing the entrance of a grub which lives in beds of leaves and preys on small snails.

An American malacologist, Professor Wetherby, adduces evicence which goes far to prove that even malformations resulting from individual injuries may, under certain circumstances, be transmitted to the offspring.

In investigating these phenomena and their causes, I would suggest, first, that the manner of variation should be investigated and described, and, second, the exact nature of the surroundings as regards possible causes, always bearing in mind the conditions under which the species lives in its original home, and especially noting all deviations from these which may be supposed to induce the varietal character.

Among the species common to North America and Britain are the following: *Vertigo alpestris, V. edentula, Conulus fulvus, Helix aspersa, H. hortensis Limnæa peregra, L. auricularia, L. stagnalis, L. palustris, L. truncatula, Physa fontinalis, Bullinus hypnorum, Planorbis albus* (= *P. hirsutus,* Gould), *P. glaber* (= *P. parvus,* Say).

W. A. GAIN.

Tuxford, Newark, England.

Books for Children.

IN answer to Mr. Waldo's request printed under the above heading in your issue of *Science* for June 16, let me suggest that such books as he desires are a desideratum not only for children, but for adults who, while not scientifically inclined, are yet interested in the wonders and beauties of nature. Unfortunately our attention has been too exclusively absorbed with the struggles and the problems incident to a new country for us to have time to educate the men who could study and name all our plants and animals, much less those who could translate scientific monographs into popular language. Especially in the insect world a good collector could bring in from any summer-day's excursion dozens of specimens which have never yet been christened.

But while we cannot hope for books which will enable us to attach names to everything we may find in a ramble through Nature's museum, most of the more conspicuous animals and plants have been studied, at least enough for this purpose, though the results have been put forth in scientific works. But on the stores of knowledge thus accumulated popular writers are beginning to draw to meet the demand created by our growing out-of-door life, our increased out-of-door interests. As was to be expected, plants have received the greater amount of attention. Mrs. William Starr Dana's "How to Know the Wild Flowers," just published by Charles Scribner's Sons, at $1.50, is intended to teach one to identify the commoner flowers by color, size and shape of leaf, size of plant and so forth. Ten-year-old children would seem to me rather young to use such a book, but it is admirable for those of twelve or thirteen. Newhall's "Trees of Northeastern United States," published by G. P. Putnam's Sons, at $2, teaches one to identify trees by the leaves, bark, and so forth. This I know from experience to be admirable for children. The same author is at work on a similar book upon shrubs, but I believe it is not yet out. I know of no such book on birds as the ones I have just suggested on plants. The best thing for children I believe to be Florence Merriam's "Birds through an Opera Glass," published by Houghton, Mifflin & Co., at 75 cents. The appendix to this little book contains lists giving form, color, size, habits, song, flight, nest, and so forth of our common birds. A fuller and altogether admirable book on birds is Minot's "Song and Game Birds of New England," published, I believe, by Casino, at $2.50 or $3. The best book on insects is one which Professor Comstock, of Cornell University, has in hand. It will probably be out now in the course of a very few months. Prepared especially for the school children of California, it is written in a manner attractive to children and will contain tables by which any insect may be traced to its proper family. Farther than this it would be hardly possible for a child to go, as the characteristics on which genera and species are founded are often so difficult of observation that the best tables which could be prepared would be only a source of perplexity and worry.

After all the best method of teaching children is that which Mr. Waldo quotes as employed by his former teacher. And there are many books which occur at once to the mind of any teacher as valuable aids to the parent who wishes to work with his child. I have not named these because I understood the request to be for books which the child could use alone. But I should be happy at some future time to extend my list if it is not done by some other person better qualified for the task.

M. A. WILLCOX,
Professor of Zoölogy, Wellesley College.

Two Queries.

AN incident of a recent personal experience may interest those of your readers who are studying the subject of mimicry. On the 21st of May last, I was botanizing with two companions in the thinly populated sand-dune region at the south end of Lake Michigan, and about forty miles east of Chicago, when the event I am about to relate occurred. I was walking rather in advance of my companions across a level area that separated two series of high dunes, when I accidently stepped upon two large snakes which were lying close together, doubtless enjoying the warm sunshine. It was a case of mutual surprise, and as the snakes, or one of them, suddenly sprang upward into unpleasant proximity to my face, I only a little less suddenly sprang backward, believing for the instant that I had encountered a rattlesnake. I soon discovered, or thought I did, that the reptiles were only fine specimens of the kind of black snake, popularly called the blue racer. One of the two had been considerably hurt by my heavy tread, and with violent contortions of his body made what haste he could to a hole about six feet distant, and disappeared in it. The other was uninjured and crawled rather leisurely away in another direction to a distance of twenty feet or more, and then lay quiet, watching our movements. Irritated by the violent start I had received, and cherishing no great love for snakes in general, I seized a club, and, while his snakeship lay broadside to me, I aimed a vigorous blow at him. I was again surprised, even more so than before, though in a different way, for with lightning rapidity the lithe reptile dodged the blow which otherwise would have struck him near the middle of the body, and instantly threw himself into a coil precisely resembling that of a rattlesnake when about to strike, and shook his erected tail with such vigor and rapidity that it was scarcely more distinctly visible than the spokes of a bicycle wheel when propelled by a fast rider. At the same time a sound was emitted, less shrill perhaps, but continuous and distinctly similar to that produced by the rattlesnake. Whether the sound was produced by the very rapid vibration of the tail, assisted perhaps by its scaly covering, or whether it was a hiss produced in the ordinary manner, I am of course unable to say. So close was the mimicry that I was for the moment almost deceived into the belief that I had mistaken a rattlesnake for a racer. The illusion was soon dispelled however, for a stick which I threw at him hit him on the head and stunned him, and I then had the opportunity to scrutinize him closely and verify my first conclusion.

I have frequently heard of other constrictor snakes mimicking venomous ones, in fact have occasionally observed such mimicry myself, but never before in this species and never in such perfection. It would be interesting to know if others have observed the habit in this species.

On the same trip another fact of interest came under our observation. The region visited contains many ponds and lagoons, and in these turtles (mainly *Chrysemys picta,* Ag. and *Nanemys guttatus,* Ag.) abound. About these ponds, often many rods from the water, were the remains of hundreds of turtles that had evidently all been killed since the opening of the spring, and some of them within a few hours. The dead turtles varied in size from those with carapaces two inches long to those fully six inches in

length. It was clear from an inspection of those most recently killed, that they had been killed by some animal for food. The flesh of all had at least been partly devoured, but it was observed that not a carapace nor a plastron was broken. The reptiles had been killed, apparently, by some sharp-beaked bird, by thrusting its beak between the joints of the reptile's armor, so to speak. The loon is clearly competent to do this, but loons are seldom seen in this locality. Moreover these birds would hardly drag their prey so far inland to devour it, as was observed to be the case with many of the turtles. The blue heron is more abundant here than the loon, but still not abundant enough to be credited with so much destructive work on animals so large. I have never suspected him, either, of being a turtle-eater. The only other birds competent to do the work and sufficiently numerous and intelligent to be suspected, are crows. Several flocks of these were hovering about the locality, and though we were not able to approach the wary birds close enough to observe them feeding, our suspicions fell upon them. Has any reader of *Science* observed crows killing turtles? If so, is this a well established habit of the bird or is it one which has been recently acquired?

EDSON S. BASTIN.

Chicago, Ill., 2421 Dearborn Street, June 14.

The Aurora.

DR. VEEDER'S reply of June 2nd, is so objectionable on account of the positive way in which he closes his part of the argument (believing, as I do, that his facts are in fault) leaving it to be believed that at "no point throughout the research has there appeared to be even the slightest 'chance' for an alternative hypothesis," that I am once more tempted to reply. Let me, before passing on, emphasize the fact that we are not discussing the question of "magnetic storms" and sun-spots. I believe there is only one astronomer and physicist of any eminence who disbelieves in this association, so that as far as discussion of the question is concerned, we may consider it as practically closed; but, even if I held the contrary opinion with the majority, so long as an opponent of such eminence held out, I should consider it inadvisable to be as positive as Dr. Veeder in his last letter, on the subject of the aurora, where, I believe, I am not alone in supposing there is reason to doubt a connection between this display and areas of disturbance on the eastern limb of the sun. I have raised some well-known objections to this theory, and, as a rule, have been met by Dr. Veeder with generalities (*Science*, April 7, 28, May 19 and June 2); it is unnecessary to mention them again here, so that I shall content myself with discussing this last contribution, which leaves me in such an uncomfortable position, apparently.

The whole base and superstructure of this theory is erected upon a solar period of rotation of "27¼ days," and to quote from a letter which I have received from Dr. Veeder, dated March 16, 1892, the addition of "a few hours difference in the length of the period introduces a drift into the tables that becomes everywhere apparent." Surely this is a suspicious degree of perfection in the theory, as no one knows what the solar period of rotation is: such periods as have been determined from sun-spots (the only possible method so far) give values between 25 and 27½ days, depending on the solar latitude of the spot; yet, the addition of a "few hours" can introduce a "drift which becomes everywhere apparent," when 2½ days is left out of the tabulating without apparent effect, for, it is evident, that in considering the effects of the return to the eastern limb of a sun-spot or area of disturbance, that it is not a fixed rotational period that should be used, but the one belonging to the latitude of the spot under discussion.

This year auroras were visible here on the following days of the year: the 5th, 6th, 8th, 21st, 35th, 36th, 44th, 45th, 46th, 47th, 104th, 109th, 127th, 128th, 130th, 144th, 145th, 160th, 164th, 165th and 166th. If auroras are caused by a disturbed solar area at the eastern limb, we should find, by adding the interval adopted by Dr. Veeder of 27¼ days to any of the above days, the probable date of the returning display. What do we find in fact? That, of the 52 periods obtained by adding this interval in succession to the above days, up to the present date, there were only 10 of the days so determined on which displays took place; that is, 20 per cent of successes as against 80 per cent of failures. In illustration of the above, the aurora of the 5th day should have reappeared on the 32¼, 59½, 86¾, 114 and 141¼; from the days of auroras given above, it will be seen it appeared on none of the required dates; nor did that of the 6th; that of the 8th reappeared twice out of five solar periods; the 21st, once out of five; the 35th, once out of four, and so on.

One more objection, previously overlooked, before passing on. I am of opinion (no one can be certain, failing the necessary observations), that there is practically no instance in which aurora displays are not taking place in one hemisphere or other of the earth; a large proportion should be observed co-incident with any other class of recurrent phenomena, and think it possible that "chance," which Dr. Veeder avoids the discussion of, is really an important element in our discussion, as I shall now endeavor to prove this by his own admissions.

In a letter to me, dated May 4, 1892, he says: "The year 1879, selected for printing as an illustration of the results seen throughout the entire table, is one of profound minimum at which times solar disturbances are well separated from each other and the relation *comes out distinctly* although for the construction of such a table one year is just as good as another." (*italics* are mine.) This is a perfectly sound conclusion, and by it alone might this theory stand or fall if "chance" is not, or is, as important as I maintain. On May 13th, Dr. Veeder writes: (This table of comparison between the phenomena being now printed) "It [1879] being a year of minimum the relation *does not come out so strongly as when disturbances were more numerous.* In the next year (1880) *the numbers would be much larger and the relation in every way more distinct.*

So far, then, Dr. Veeder has been about equally positive on *both* sides of this question, both of which opinions are apparently obtained from the observations he is in possession of, leaving the possibility open (it is his suggestion) that we are very far from "a realizing sense, that it is *facts* and not a personality against which" we "are contending."

Might I again suggest the advisability of setting a limit on the term "eastern limb," adhering *rigidly* to it throughout the investigation, not admitting too much of the suppositional when sun-spots fail at the required period by the substitution of "faculae," and seeing how far the element of "chance" enters into this question by showing a continuous series of comparisons through a semi-period, at least, of solar activity.

W. A. ASHE.

Quebec, May 17.

Scientific Words in the Century Dictionary.

ALTHOUGH one of the most useful books published, the Century Dictionary is, of course, not faultless. The mention of a mistake in a recent issue of *The Critic* reminded me also of the following:—

According to the latest edition of Foster's "Physiology," saliva "in a healthy subject is *alkaline*, especially when the secretion is abundant. When the saliva is scanty, or when the subject suffers from dyspepsia, the reaction of the mouth may be *acid.*" According to the Century Dictionary, the saliva "is a colorless ropy liquid which normally has an acid reaction."

The word "griffe," which is commonly used in Louisiana, is defined by the Century Dictionary as a "a mulatto—especially a mulatto woman." I have copied in a note-book from a lecture delivered in New Orleans by Hon. Charles Gayarré, the historian of Louisiana and authority on such matters, the following: —

"In Creole America there is a very mixed population. Even in very early times there were these distinctions: European, or fresh white immigrant; Creole, or pure white American of European parentage; the aboriginal Indian; the *griffe, or cross between Indian and negro;* the mestizo, or mixed white and Indian; the mulatto, etc., etc." These may not be the exact words of the speaker, since I may have misunderstood or copied it wrongly, but I think the same statement may be found in one of his works. Griffe, no doubt, is from the Spanish *grifos*, meaning frizzled