Potomac River to the south, the Ohio River in the west, and many other places; (5) The *Heteropygii* have three genera (as understood by Putnam, the only naturalist who has thoroughly studied them) confined to the western and southern states; (6) The genus *Paralabrax* is an entirely marine one, very closely related to *Serranus* (*cabrilla*, *scriba*, &c.), and is represented extensively on the western coast of America, as well as elsewhere in the Pacific Ocean; (7) *Huro nigricans* (the only species) is a mere synonym of *Grystes* or *Micropterus nigricans*, which extends to Florida in the south-east, and Mexico toward the south-west; (8) *Pileoma* is a later name for *Percina*; (9) *Bryttus* and (10) *Pomotis* are not *Percidæ* according to most American authors, nor according to Dr. Günther's recently promulgated views (the vertebræ being only *A* 10 + *C* 14), and belong to a quite peculiar family; (11) *Hypodelus* is a misnomer for *Hopladelus*; (12) *Thaleichthys* is as much a marine genus as *Osmerus* (Smelts); there is no such restriction at all as indicated by the remarks on the distribution of (13) *Moxostoma*, (14) *Pimephales*, (15) *Hybognathus*, and (16) *Rhinichthys* on the one hand, and (17) *Ericymba*, (18) *Exoglossum*, (19) *Leucosomus*, (= *Semotilus*), and (20) *Carpiodes* on the other; and the categories might indeed, as to most causes, be almost reversed; (21) *Mylaphorodon* is a misnomer for *Mylopharodon*. The number of genera enumerated as peculiar might, too, be very safely more than doubled, and by reference to Günther's work and subsequent corrections, *Centrarchus*, *Ptyonotus* (= *Triglopsis*), and *Hysterocarpus* could have been added. All these errors might have been prevented if Mr. Wallace had been familiar with ichthyology and its literature. The paragraph cited also quite conceals the remarkable distribution into secondary faunas of the American genera, and is calculated to entirely mislead respecting the contrasts between North America and the Old World. His use of the term "Eastern States" (instead of "Eastern Province," as Baird calls the division meant) is confusing, inasmuch as it is a geographical designation for a particular group of states.

Smithsonian Institution, Washington, September 21 THEO. GILL

The Self-Fertilisation of Plants

UNDER this title there is an article in NATURE, vol. xiv. p. 475, mentioning some observations on flowers, and concluding thus:—"In view of these examples . . . . it can hardly be that colour, fragrance and honeyed secretions in flowers have been developed solely to secure cross-fertilisation." In reply to this article it may be worth showing that of the examples relied upon the first and last are most probably incorrectly observed and erroneously interpreted, whilst the others are of no consequence at all, so far as the good effects of cross-fertilisation are concerned.

First, the flowers of *Browallia elata* have been most accurately described by F. Delpino ("Ulteriori osservazioni sulla dicogamia nel regno vegetale," Parte I. p. 140-143), and this excellent observer has fully convinced himself that it is cross-fertilised whenever it is visited by Lepidoptera or Bombylius.

Claytonia virginica and *Ranunculus bulbosus* simply confirm the well-known fact that many flowers have recourse to self-fertilisation when not visited by insects (see H. Müller's "Befruchtung," p. 443-448, NATURE, vol. viii. p. 433, vol. ix. pp. 44, 64, vol. x, p. 122).

As to the last example, *Ranunculus abortivus*, it is inadmissible to conclude from the fact that one has not observed visitors on a plant, that this plant is wholly neglected by insects.

With regard to the article as a whole, it seems to me somewhat rash to call in question a comprehensive and well-founded theory on the basis of a few superficial observations.

Lippstad, October 20 HERMANN MÜLLER

The Proposed Zoological Stations at Kiel and Heligoland

IN NATURE, vol. xiv. p. 535, there appears amongst the occasional Notes, a short report of a proposal of the Association of German Naturalists to found two new Zoological Stations at Kiel and Heligoland. The establishment of such stations could not fail to be of immense service to biology, but it is much to be regretted that the Association is inclined to put aside the claims of the present Zoological Station at Naples in favour of these two new institutions. To act in this way would be both unwise and ungenerous: unwise, because a station on the shores of the Mediterranean can obtain a great variety of forms which are not to be found in the North Sea and the Baltic; and ungenerous because the Naples Station has been the means of proving both the value and feasibility of such institutions, and without it the present proposals would never have originated. It is indeed surprising to see a body of German naturalists refusing their support to an institution like that at Naples, which has already rendered such signal services to biology, in which so many of themselves have made important discoveries, and which is, moreover, founded almost on the site of the classical investigations of Kölliker, Gegenbaur, and Hæckel.

It is to be hoped that the Commission appointed by the Association to draw up a memorandum will see their way to urging the claims of the existing Zoological Station at Naples without thereby interfering with the prospects of the similar institutions which it is proposed to found. F. M. BALFOUR

Trinity College, Cambridge

The Flame of Chloride of Sodium in a Common Coal Fire

MR. HARDMAN, in NATURE, vol. xiv. p. 506, gives an account of a number of experiments which he considers to bear out the old theory that the blue flame produced by throwing common salt on a coal fire is due to carbonic oxide. His letter induces me to give an account of a series of experiments which I made last winter, in company with Mr. R. A. Lundie, and which led me to an exactly opposite conclusion. Our experiments were all made with the help of a spectroscope, no dependence being put on observations made with the naked eye:—

1. We examined, with the spectroscope (which was a small direct-vision one), a *very distinct* blue flame of CO, burning in a coal fire; this, as far as we could see, gave no bright lines. A little common salt was then put on the fire, when at once a very marked spectrum appeared, the most characteristic part of which was a pair of bright lines in the blue, and another pair in the violet beyond the spectrum of the glowing coals, against which the flame was generally seen. This flame was very persistent, and frequently long after the *flame* had ceased to be distinguishable, the *spectrum* was still quite marked.

2. We did not succeed in getting the spectrum with other salts of soda, such as carbonate, phosphate, and borate; nor yet with microcosmic salt, while on the other hand, with other chlorides and chlorates, such as KCl, $KClO_3$, and NH_4Cl almost exactly the same spectrum was obtained, and with bromide of potassium a very similar, if not an identical, spectrum was also obtained.

3. We were able, but with more difficulty, to get the characteristic spectrum, when a blow-pipe flame was made to play down on chloride of soda, or ammonia, lying on an iron plate; and in this case it was observed that the blue flame *seemed* to be produced only when the flame which had passed over the salt came to a colder part of the plate where there was more salt.

Want of time has prevented me from continuing my experiments, and I do not venture to suggest any theory to account for the phenomenon. It is possible that part of the blue blaze is due to carbonic oxide, but I am convinced that this is not a complete explanation. Neither do I think that Mr. Müller's explanation (NATURE, vol. xiii. p. 448) is sufficient, though a number of our earliest experiments, in which a brass plate took the place of the iron plate (in experiment 3), certainly favour this explanation to a certain extent. The flame thus produced gave the characteristic spectrum very brightly, but at the same time new lines (copper) appeared in the green. I would add that I have as yet been unable to get satisfactory measurements of the positions of the lines, the spectroscope I used for most of my observations having no micrometer nor scale.

C. MICHIE SMITH

Keig, Aberdeenshire, October 13

OUR ASTRONOMICAL COLUMN

THE INTRA-MERCURIAL PLANET QUESTION.—M. Leverrier has made a further communication to the Paris Academy on this subject. With the view to testing the sufficiency of the method employed, to afford a guide for prediction of future transits of such a body over the sun's disk, admitting that the observations in which appreciable motion is recorded really refer to an intra-Mercurial planet, he applies it in the case of Mercury. Tran-

sits of Mercury were observed by La Concha at Monte Video, November 5, 1789; by Keiser at Amsterdam, November 9, 1802; by Fisher at Lisbon, May 5, 1832; and by Houzeau at Brussels, May 8, 1845. Taking for the heliocentric longitudes of the body observed, the tabular longitudes of the earth at the epochs of the observations, the following formula for the heliocentric longitude (ν) at any time, is obtained—

$$\nu = 56°{\cdot}04 + 4°{\cdot}092307\,j - 7°{\cdot}66 \sin \nu - 9°{\cdot}18 \cos \nu,$$

where j is the number of days from November 5, 1789.

Then admitting the place of the node of the orbit to be in 46°, a transit is indicated by the formula for November 9, 1848, which actually took place.

The problem under discussion, as it refers to a possible intra-Mercurial planet, is susceptible of many solutions, which it becomes necessary to determine. They are comprised in the formula

$$\nu = 139°{\cdot}94 + 214°{\cdot}18\,k + (10°{\cdot}901252 - 1°{\cdot}972472\,k)\,j + (-5°{\cdot}3 + 5°{\cdot}5\,k) \cos \nu.$$

j in this case being reckoned in days from 1750·0, and k being an indeterminate, which may receive values either positive or negative, but necessarily whole numbers.

If $k = 0$, the solution, very precise, is the one already given where the duration of a revolution is 33·02 days, and the semi-axis 0·201.

If $k = -1$, the solution is as exact as the preceding one. The revolution is 27·96 days, and the semi-axis major 0·180.

If $k = -2$, the solution is less exact; the revolution becomes 24·25 days less than the period of the sun's rotation.

If $k = 1$, a solution of the same degree of precision with the last is obtained, with a revolution of 40·32 days.

And if we put $k = 2$, when the revolution would be 51·75 days, large errors will remain.

In all these hypotheses the calculated epochs of transit in 1859 (Lescarbault) and 1862 (Lummis) are very nearly the same. Under these conditions M. Leverrier assumes that we may venture on the calculation of the times of future conjunctions, which occur in the vicinity of the nodes, situated in 192°·9 and 12°·9, the first point being the ascending node, and with the orbit corresponding to $k = 0$, he determines the times of conjunction in the intervals 1853–1863, 1869–1877, and 1885–1892. The tables show that the epochs of transits will be regulated by a period of about seventeen years, in the middle of which the transits will occur, but after which none would be seen for many years. Lescarbault and Lummis it appears observed at the end of one series of transits, which explains why in searching after them in the same region of the sky observers have not seen anything, and seven or eight years might elapse without more success. M. Leverrier then examines the possibility of a transit of the hypothetical planet in the spring of 1877. The conjunction with the sun would occur on March 22 at a distance of 10°·9 from the node, and if this distance be considered certain, as well as the assumed inclination of 12°, there would not be a transit, but in view of very probable modifications of these numbers, a transit may be possible; and he then urges observers to a close watch upon the sun's disk on the 22nd of March next, seeing that there would be no other transit at the spring node before 1885; and a similar examination of the conjunctions at the opposite node (September and October) shows that for the present they do not occur under more favourable conditions. The conjunction in 1876 would take place on September 21, when a transit, though not altogether impossible, is very doubtful. For a transit at this node it is necessary, under the assumed conditions as to the position of the orbit, to wait until about 1881.

For the present, then, there remains no other resource than a direct search off the sun's disk, and M. Leverrier remarks that Dr. Janssen "ne désespère pas d'y parvenir, grâce aux perfectionnements de l'optique céleste, auxquels il a si puissamment contribué." The remaining part of the communication to the Academy is occupied with ephemerides of differences of right ascension and declination of planet and sun for the last half of October.

Mr. De la Rue has instituted a very close examination of the Kew heliographs, with some interesting results.

THE VARIABLE STARS S CANCRI AND U GEMINORUM.—The following are times of visible geocentric minima of S Cancri, calculated from the elements of Prof. Schönfeld's latest catalogue, where the period is 9d. 11h. 37·75m.:—

	d.	h.	m.		d.	h.	m.
1876, Oct.	30	15	9	1877, Jan.	14	12	3
Nov.	18	14	22	Feb.	2	11	18
Dec.	7	13	35	,,	21	10	35
,,	26	12	48	March	12	9	52
				,,	31	9	9

While the irregularity of intervals between the observed maxima of U Geminorum of late years appears to forbid the hope of making a reliable prediction of these epochs at present, it may assist observation of the right object if it is noted that the variable precedes the principal component of Σ 1158, 1m. 26·5s., and is north of it 7′ 31″. The writer is informed by M. Otto Struve that this star does not quite disappear in the Pulkowa refractor, but with instruments of more ordinary dimensions it is invisible during the greater part of the period of 9½ days. There is a star 12·13m. very near its position.

BIOLOGICAL NOTES

CEPHALISATION.—Such is the name given by Prof. Dana to what he terms a fundamental principle in the development of the system of animal life. Its meaning can be best explained by the employment of the instances used by its author. The lobster and the crab are closely allied decapod crustaceans. In the lobster the tail is large, the cephalo-thorax elongate, and the antennæ of considerable size. In the crab the tail is minute, packed under the cephalo-thorax, which is short, as are the antennæ; and from this we may infer that passing upwards from the Macrural to the Brachyural forms there is an abbreviation and a compacting of structure before and behind the head. "In the whale the tail is the propelling organ and is of enormous power and magnitude, and the brain is very small and is situated far from the head extremity in a great mass of flesh and bone furnished with poor organs of sense." The principle is therefore that in low types "there is, usually, large size and strength behind, an elongation of the whole structure, and a low degree of compactness in the parts before and behind; in the high, there is a relatively shorter and more compacted structure, a more forward distribution of the muscular forces or arrangements, and a better head." The analogy is ingenious, but we can see nothing of value in the argument more than a repetition of the well-known principle that height in the scale of creation and amount of cerebral development are correlated phenomena. Are we to place the koala, which, by the way, is wonderfully like some of the much higher Lemurs in its proportions, at the top of the Marsupial phyllum and the kangaroos at the bottom, because the former wants the tail and has a blunt nose, whilst the latter have an enormous caudal appendage and a slender snout? Is the sun-fish so much higher than the eel, and the ostrich than the lyre bird? We fear that cephalisation is not a true law of nature.

RHINOCEROSES.—Anyone visiting the Zoological Gardens in Regent's Park at the present time can obtain ocular proof of the existence of two species of single-horned rhinoceros, differing in size, texture of integument, and skin-folding. On a former occasion (NATURE, vol. ix. p. 466) we were able to demonstrate to our readers the distinguishing points in the last-mentioned of these features, and in the