After this extraordinary preface, Prof. Hæckel enters on the more serious part of the book by a history of the doctrine of development. Passing rapidly from Aristotle and the founders of biology in the sixteenth and seventeenth centuries, he describes at some length the discoveries of Wolff (published in 1759), which were so long and so unjustly neglected; the scarcely less splendid researches of the now venerable Von Baer (1827), and those of Mr. Darwin, from the appearance of the "Origin of Species" in 1859 to the present time. Among the ontogenists, beside Wolff and Von Baer, whom he justly places in the first rank, due mention is made of Pander, Rathké, Bischoff, Johannes Müller, Kölliker, Remak, Fritz Müller, and Kowalevsky. But while most English embryologists (and histologists too) will probably agree in substance with our author's judgment on the doctrines of Reichert and of His, they would scarcely speak of a distinguished living anatomist as "dieser ausserordentlich unklare und wüste Kopf." Among the philogenists who preceded Darwin, particular attention is paid to the speculations of Lamarck, in his "Philosophie Zoologique," which were published in 1809, and thus exactly divided the century which elapsed between the first great work on the subject, Wolff's "Theoria generationis," and the last, Darwin's "Origin of Species;" and also to those of Goethe, extracts from whose writings, both prose and verse, are scattered up and down the volume, not only in the text, but on the fly-leaves and other blank spaces. We venture to think that both here and elsewhere Prof. Hæckel has put too high a value on these pre-Darwinian speculations. He discovers who proves: and neither Lamarck nor Goethe could justify their guesses by facts. They happened to be right, just as among all the random guesses of the ancient Greek cosmologists Thales happened to have hit on the true relation of the sun to the earth, probably from his being less and not more philosophical than his fellows. If some of the assertions of modern spiritualists or phrenologists should hereafter turn out to be true, they would no less deserve the condemnation of a future generation for believing what, on the facts within their knowledge, they had no business to believe.

The chapters which succeed are devoted to a clear and tolerably full account of the development of the human embryo from the ovum-cell to the stratification of the blastoderm. The only fault to find with this part of the book (and its merits need no praise for those who are acquainted with our author's skill in exposition of a difficult subject) is the exaggeration of such phrases as this: "The process of fecundation is very simple, and involves nothing at all peculiarly mysterious." In one sense, of course, this is true; the ultimate mystery of every function, organic or inorganic, is equal: but fecundation, like other organic functions, has the peculiar mystery that we cannot yet rank it with other mysteries. Most of us believe that one day each movement of each particle of the ovum will receive its appropriate physical explanation, but till then we must be content to call them vital, just as we call other movements chemical: and even a popular lecture should not anticipate the advance of science.

The most important position maintained in this part of the book is that in Vertebrata the two primitive blasto-

dermic layers (epiblast and hypoblast of Huxley, exoderm and entoderm) differentiate each into two, as in Vermes, and that the mesoblast (motorgerminal layer of Remak) subsequently arises by coalescence of Von Baer's Fleischschicht or Hautfaserblatt and Gefässschicht or Darmfaserblatt. The various opinions which have been put forth on this difficult subject are discussed, and the author's view illustrated by some coloured figures. In the number of the *Quarterly Microscopical Journal* for last April there is an article by Prof. Hæckel (very ill-translated) on the "Gastræa" theory which was put forth in his valuable work on "Calcareous Sponges;" and there he discusses the homologies of the secondary germ-layers. To it we may refer the English reader as an exposition of this part of the subject, and unfortunately as another instance in justification of what has been said of the dogmatic confidence and undignified personalities which disfigure the present volume.

The description of the further development of the human embryo, including a short account of the origin of the various organs, is an excellent example of how a very complicated subject may be explained and illustrated. The figures from Bischoff, Kölliker, Gegenbaur, and other anatomists are somewhat coarsely reproduced, but are supplemented by some new drawings on stone. These chapters, however, on human ontogeny and organogeny are unexceptionable and somewhat commonplace. They seem to be chiefly introduced for the sake of the philogeny which occupies the third series of lectures. It is the close connection between the known development of the individual and the hypothetical development of the race which it is the merit or demerit of the book to expound to a popular audience, and to this subject we hope to refer in a future article.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

Migration of Birds

I HAVE to thank Mr. Wallace and Mr. Romanes for their remarks (*NATURE*, vol. x. pp. 459 and 520) on the article in which I drew attention to this subject. The former especially has laid all ornithologists under an obligation for the characteristic skill with which he has illustrated the way whereby migratory habits have most likely been brought about. I think it is very possible, as he suggests, "that every gradation still exists in various parts of the world, from a complete coincidence to a complete separation of the breeding and subsistence areas," and that "we may find every link between species which never leave a restricted area in which they breed and live the whole year round, to those other cases in which the areas are absolutely separated." Still, I cannot point out any species which I believe to be, as a species, strictly non-migratory. No doubt many persons would at first be inclined to name half a dozen or more which are unquestionably resident with us during the whole year, and even inhabit the same very limited spot. But I think that more careful observation of the birds which are about us, to say nothing of an examination of the writings of foreign observers, will show that none of them are entirely free from the migratory impulse. Perhaps the nearest approach, among British birds, to an absolutely non-migrant may be found in our familiar Hedge Sparrow. Personally, I have never been able to detect any movement in this bird, but one has only to turn to works on the ornithology of the extreme north and south of Europe to see that it is affected like the rest, and even in the Orkneys it is described as an occasional autumnal visitant. However, in most of the

British Islands and the more temperate parts of Europe it is very possibly only the young of this species which migrate, and the adults, having once fixed on a place of residence, may stick to it; so that here we have a case which will almost bear out Mr. Wallace's supposition. With this, however, he stops, and I am sorry to say offers no suggestion as to the way in which migration is effected.

The question which Mr. Romanes puts would be more appropriately answered by Mr. Tegetmeier, and I hope he will be induced to do so. I can only say that that gentleman has repeatedly urged his views on me in conversation and upon the public in his books (see "Pigeons, their Structure," &c., pp. 84, 85, and "The Homing or Carrier Pigeon," pp. 37-42, 105-118) which, being ready of access, I need not here quote. To limit myself to what I am alone answerable for, I would say that when declaring that sight alone cannot be much aid to birds while migrating, I had especially in mind the almost peculiar case of the Scandinavian form of Bluethroat (*Ruticilla surcica*), which winters in Egypt and the Nile Valley, and summers in the northern or mountainous parts of Sweden, Norway, Finland, and Russia; while, though no doubt passing regularly twice a year over the intervening countries of Europe, it is there so singularly scarce as to have been, until of late years, almost unknown to the best of German ornithologists. For the benefit of such of my readers as are unacquainted with the bird, I may add that the cock has a conspicuous and beautiful plumage, a fine song, and habits which, in the spring of the year, cannot be called unobtrusive. If, therefore, it did commonly occur in Germany—where I should state that a kindred form (*Ruticilla leucocyana*) is very well known—it could not escape observation. Wonderful as the feat looks, it would therefore seem as though this Scandinavian Bluethroat passed over Europe at a stretch, and if so, I cannot conceive its flight being guided by any landmarks.

Furthermore, there is ground for believing that some of the migrations of many species, particularly of water-birds, are performed at night, when sight, one would think, can be of little use to them. But, to be honest, I must confess that dark, cloudy nights seem to disconcert the travellers. On such nights the attention of others besides myself has often been directed to the cries of a mixed multitude of birds hovering over this and other towns, apparently at a loss whither to proceed, and attracted by the light of the street-lamps.

One other point only need I now mention; this is Mr. Romanes's assertion that "in the case of all migratory birds, the younger generations fly in company with the older ones," which is at variance with a statement (hitherto, I believe, uncontroverted) of Temminck's:—"On peut pour un fait que les jeunes et les vieux voyagent toujours séparément, le plus souvent par les routes différentes." (Man. d'Orn. ed. 2, iii. Introduction, p. xliii. note.)

ALFRED NEWTON

Magdalene College, Cambridge, Nov. 2

Insects and the Colours of Flowers

THERE is one point connected with Mr. Darwin's explanation of the bright colours of flowers which I have never seen referred to. The assumed attractiveness of bright colours to insects would appear to involve the supposition that the colour-vision of insects is approximately the same as our own. Surely this is a good deal to take for granted, when it is known that even among ourselves colour-vision varies greatly, and that no inconsiderable number of persons exist to whom, for example, the red of the scarlet geranium is no bright colour at all, but almost a match with the leaves.

RAYLEIGH

Whittinghame, Preston Kirk

Sounding and Sensitive Flames

A SEVERE indisposition, which disabled me from correspondence during nearly the whole of last month, prevented me from acknowledging as soon as it appeared in NATURE (vol. x. p. 244) Prof. Barrett's excellent communication on Sounding and Sensitive Flames, replying to my letter on the same subject at page 233 of this volume. Prof. Barrett supplied me with many useful references, and with one at least the want of which led me to misrepresent his connection with the discovery of sensitive properties in suitably adjusted wire-gauze flames, for which I had sought in magazines and journals for some months previously in vain. A note of the original description of Mr. Barry's experiment in NATURE, vol. v. p. 30, had in the meantime been pointed out to

me in another record of very similar experiments, which is itself also, I have no doubt, the same account of "further experiments with the same kind of flame," that Prof. Barrett cites as appearing in the *Journal of the Franklin Institute* for April 1872, to which I have not been able to obtain access. The nearer channel to which I was referred for its perusal is the *Philosophical Magazine* for June 1872, where a paper is briefly extracted from the *American Journal of Science* of the preceding month, describing new experiments with Barry's sensitive flame, by Mr. W. E. Geyer, of the Stevens Institute of Technology, in the United States. By placing a wide tube over the flame at a proper height it became sounding, or, if silent, might be made sensitive in such a way as to sound at the slightest hiss or rustle, and on producing any jingling or tinkling sounds in its neighbourhood. Thus the flame sounded twice on pronouncing to it the word "sensitive," showing its instantaneous affection even by momentary sibilant sounds. By varying the experiment, an opposite condition of the flame was obtained, in which it continued sounding until checked by a hiss or rustle from without. It is observed by the editor of the *American Journal of Science*, in a note to Mr. Geyer's paper, that in the number for September 1871, of the *Moniteur Scientifique*, a form of apparatus and experiment apparently identical with Mr. Barry's is noticed as having been made by Prof. Govi at Turin, and this was a few months prior to the letter in which the account of his experiments is given by Mr. Barry to Prof. Tyndall. Thus the sensitive properties of certain wire-gauze flames, like the property of such flames to excite very readily musical vibrations, may have had many independent discoverers; the value of such discoveries is now, as it must have ever been, the new light which one is capable of throwing upon another. The rapid publication of results urgently requires their frequent collection and comparison together; and this process, pressing and urgent as it is, seldom fails in experienced hands to prove a connection, to bind together a chain of consequences, and to leave a subject in general better explored and embellished with new-found illustrations than it was before. Such was the successful treatment, a few years ago, by Prof. Tyndall, of the question of sounding and sensitive flames, when it was shown by beautiful illustrations of Savart's sensitive water-jets, and by equally ingenious and new experiments with smoke-jets as substitutes for flames, that sensitiveness is a residing property of liquid veins and gas-jets, independently, in the latter case, of their being lighted. The laws of fluid pressure and motion, and apparently foremost of all those of capillary attraction in liquids and of mutual friction and diffusion in gases, and not the energies of heat and combustion of a flame, preside principally over the observed phenomena. The bifurcated head, or low ruffled brush to which the tall wand-like sensitive jet is suddenly reduced, is but the glowing representation of the form which, if it were visible to the eye, the unlighted jet would, under the same circumstances, be observed to take. This is at least in general terms, and perhaps also in plain and fairly accurate statement of the real facts, the simple result which the collection and elucidation of the most brilliant then known experiments illustrating sensitive flames, led a philosopher of Prof. Tyndall's enlightened sagacity and skill in physical investigations to adopt. There can be no doubt of its substantial correctness in the increasing array of cases to which it may be successfully applied. The flame is but an illuminated effigy of some of the lowest parts of the issuing gas column, whether tranquil or disturbed, whose upper parts it removes and replaces by products of combustion. The lower parts are also marred in their form by heat, but not so much as to obliterate the original character, shape, and dimensions of the part of the gas column that it represents. The flame terminates upwards, and ceases to represent the unlighted column further when it has found surface of contact enough with the outer air to effect the complete combustion of the gas. The up-draught of violently heated products of combustion near the base impedes the access of fresh air to parts near the summit of the flame, and it must, besides, deform them otherwise, sometimes even rhythmically, as in the unsteady throbbing flame of an ill-trimmed lamp or of a candle burning in its socket. The noisy roar with which flaring of gas-flames is attended tells us also of the uneven mixture of the gas and air supplies with each other in the flame, and reminds us of the rapid fire of small explosions that must probably introduce new sources of confusion in its form. If these explosions, however, are regularly timed, they can be made to maintain the simple musical note of harmonic flames; and these flames again, wholly dependent as they appear to be