forces which act on the moving charges during the period of establishment of the field, whereas Larmor's theorem confines itself to the so-called Coriolis forces which, as Prof. Hicks points out, act transversally on the moving charges, and hence cannot alter their energies.

Prof. Hicks proves in a simple case that the application of the Wilson-Sommerfeld quantum conditions to the Bohr hydrogen atom with reference to fixed axes (instead of the special rotating axes employed in the usually accepted theory) leads to no Zeeman effect at all as a first approximation. A more general proof of this was given in a paper of mine about two and a half years ago (Roy. Soc. Proc., A, vol. 102, 1923, p. 529) in which I also put forward an alternative theory of the simple Zeeman effect which seems to me to answer Prof. Hicks's purpose. The theory is based on a slightly extended form of the quantum conditions which was first suggested by Prof. William Wilson (Roy. Soc. Proc., A, 102, 1923, p. 478), namely,

$$\int_0 \pi_i dq_i = n_i h, \qquad (i = 1, 2, \ldots)$$

where $\pi_i = p_i + eA_i$,

p and q being the usual Hamiltonian co-ordinates, e the charge on the particle in question, and A the generalised magnetic vector potential. These conditions are applied both in the absence and in the presence of the field, thus defining the orbits and their energies in both cases, and the frequencies are then obtained from the energy relation $\Delta W = h\nu$. It is also shown that the relation between corresponding orbits defined by the extended conditions (*i.e.* orbits for which the quantum numbers are the same) is in complete accord with Schott's theorem; in fact the latter is derived as a necessary consequence of the quantum conditions themselves.

A. M. MOSHARRAFA.

The Manor House,
Alphington, near Exeter, June 28.

THE objection of Prof. Hicks to the use of Larmor's principle (NATURE, June 27, p. 978) is well founded, but the Zeeman triplet effect can be made to fit into the quantum theory by keeping strictly to dynamical principles. The phase-integral $\int pdq$, for a variety of reasons, is, for the case of a magnetic field, to be replaced by $\int(\delta L/\delta\dot{q})dq$, where L is the Lagrangian function. For the hydrogen atom

$$L = \tfrac{1}{2}m(\dot{r}^2 + r^2\omega^2) - \tfrac{1}{2}Her^2\omega/c + e^2/r.$$

Hence, on quantising,

$$mr^2\omega - \tfrac{1}{2}Her^2 = nh/2\pi.$$

From this, for radial quantisation,

$$m^2\dot{r}^2 + n^2h^2/4\pi^2r^2 = 2e^2m/r - 2mC',$$

where $C' = C + nhe/4\pi mc$, $-C$ being the energy and H^2 being neglected. Hence the "permitted" value of the energy is

$$-(2\pi^2me^4/h^2)/(n+n')^2 + nhe/4\pi mc,$$

where n, n' are the azimuthal and radial quantum numbers. ARTHUR W. CONWAY.

Abbeyview, Dalkey, Co. Dublin,
June 27.

The Oogenesis of Lumbricus

IN a letter to NATURE (June 27, p. 979) Prof. J. B. Gatenby objects to certain comments upon his work made recently by Mr. L. A. Harvey in a paper on yolk-formation in the earthworm (*Q.J.M.S.* 69, p. 291). Mr Harvey is a student working in this department and it is on his behalf that I wish to protest against the tenor of Prof. Gatenby's letter. It is quite evident that Prof. Gatenby has not comprehended clearly the contents of Mr. Harvey's paper; for his letter contains misstatements, and these may do a considerable amount of harm unless speedily contradicted.

Prof. Gatenby accuses Mr. Harvey of having been discourteous in saying that a glance at a paper of his (Prof. Gatenby's) summarising what is known about the formation of yolk shows that "really very little is known" on the subject. Mr. Harvey was perfectly justified in making this statement—it is simply a statement of his opinion—and on this point I am in complete agreement with him. The fact that Prof. Gatenby disagrees with the statement does not make it discourteous. The paper referred to, Prof. Gatenby complains, is an "old one." Its actual date is 1920, and if the advance since then is represented in Dr. Brambell's paper (1924) on "Yolk," to which Prof. Gatenby refers, it can safely be said that any advance made has been extremely small.

The remarkable objection is then made that Mr. Harvey, in studying yolk-formation in Lumbricus, is not justified in inferring any conclusions as to the similar process in Limnæa—a form studied by Prof. Gatenby. He gives no reason in making this statement. However, he previously refers to a paper by a student of his as containing an account of *Molluscan* oogenesis. Actually it deals with two forms and those both gastropods, and hence any general conclusions drawn must have been inferred from the study of those two forms.

Prof. Gatenby suggests that before criticising his work Mr. Harvey should have repeated it. While I admit that repetition might be desirable, it is obvious that Prof. Gatenby has failed to grasp Mr. Harvey's criticism, which is, not that his observations are at fault, but that his deductions are. This is made perfectly clear on p. 292.

Prof. Gatenby's next point is that it was unfortunate that the egg of Lumbricus was chosen for the study of yolk-formation, as it contains no "real yolk." This is incorrect. Yolk *is* present in the egg, and the criteria used for the recognition of that yolk were those advocated by Prof. Gatenby himself. This is fully explained on p. 299. Further, Prof. Gatenby objects that Lumbricus is a "special atypic annelid" and yet refers to Saccocirrus (apparently) as a typical annelid.

It is the static conception of the cell to which Mr. Harvey objects. He regards it essentially as a dynamic concern—an equilibrium system in which the constitution of each constituent is a function of its surroundings—and because of this he considers that the technical methods and the reasoning adopted in modern cytological investigations into the question of yolk-formation are wrong. If Prof. Gatenby had read more carefully the introduction to Mr. Harvey's paper he would have grasped this, and, in that event, it is to be hoped, would not have written his letter.

H. GRAHAM CANNON.

Zoology Department,
Imperial College of Science,
South Kensington, July 2.

Transmission of a Rosette Disease of the Ground Nut.

THE important part played by insects in the dissemination of the virus diseases of plants is now recognised, and experimental proof of transmission by particular insects exists in a number of cases. As a result of investigations during the past season, we are able to add one more to the list of those diseases of which the insect vectors are known.