the electromagnetic field turns out to be independent of the time,[1] but according to the above rules the lines of magnetic force can be regarded as moving with velocity c^2/v parallel to the axis of x, and the lines of electric force with velocity v in the same direction. If our function ψ can be regarded as a constant multiple of Schrödinger's ψ and τ as its imaginary conjugate, then, whenever we have an expression such as

$$\psi = \Sigma A_n e^{2\pi i \nu_n \left(t - \frac{x}{c}\right)},$$

the only frequencies which appear in the electromagnetic field are the differences $\nu_n - \nu_m$ of the fundamental frequencies.[2] We are not bound, however, to make ψ and τ conjugate complex quantities. If, for example, we put

$$\psi = \frac{hec}{Er} e^{\frac{iEt}{h}}, \quad \tau = ie^{-\frac{iEt}{h}},$$

where r is the distance from the origin, both ψ and τ satisfy de Broglie's equation and we have

$$E_x = -\frac{ex}{r^3}, \quad E_y = -\frac{ey}{r^3}, \quad E_z = -\frac{ez}{r^3},$$

$$H_x = 0, \quad H_y = 0, \quad H_z = 0.$$

The field is thus that of a simple electric pole.

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Patent Law and Unemployment.

THE writer of the article on the above topic has provided, in NATURE of November 13, p. 695, a very valuable graph of the number of British patents kept in force for the fourteen years' term, and has supplied useful figures of the number of foreign patents granted in Great Britain from 1900 to 1909; but I dissent from his conclusions. The former shows an almost continuous rise in the number of the fourteen-year enduring patents from 1897 onwards. This cannot be due to the retrospective action of the 1902 Act. It supports my contention that the rise is due to external industrial conditions. If the writer of the article compares his graph with the list of the numbers of foreign inventions patented here he will see that the rise or a fall in the latter is reflected in a corresponding rise and fall in the graph.

Hence I again urge that an official inquiry should be made to ascertain whether these foreign patents were taken out for the purpose of fostering or obstructing British industries. This is more material, as it is known that since the year 1900 or thereabouts British capital has gone abroad in increasing quantity (cf. Hobson, "Export of Capital"), and this fact, while it makes for national wealth, accentuates the problem of unemployment. I agree with the writer that foreign inventions patented here represent the cream of foreign inventive talent. I was wholly opposed to the foolish Act of 1919 which for a time acted as a deterrent to the foreign inventor. My contention is that if it is wished to induce the foreigner to introduce his industries here, we must stop threatening him and must offer him such better terms as will induce him to come over and help us.

E. WYNDHAM HULME.

Littlehampton, November 14.

MR. HULME'S contention that there is "an almost continuous rise in the number of fourteen-year enduring patents from 1897 onwards" can be tested numerically. For the period 1885 to 1896 the average number of such survivors was 498 per annum, the mean deviation being 4·9 per cent. and the maximum deviation 9·4 per cent. For 1899 and 1900 the figures are 509 and 508 respectively, and the deviation from the previous average, namely 2 per cent., was thus so well within the previous mean deviation that the rate of survival may be regarded as steady down to 1900 at least. Nor is the rise really great for the next two or three years. Practically the whole transition took place over the period 1904–1906, and by 1907 the figure had become so steady again that for the period 1907–1912 the mean deviation from the new average of 1210 was only 2·3 per cent., and the maximum deviation only 5·4 per cent. It is difficult to think of any cause for such a change other than the enhanced prestige conferred upon patents by the Act of 1902, as a result of increased confidence in their validity.

The rate of survival is no doubt affected by the number of patents granted to foreigners. There are other factors, however, which swamp the correlation in question, and it does not seem to be very close.

Mr. Hulme more than any one else can claim credit for having directed attention in recent years to a principle which is in real danger of being forgotten, namely, the principle that the value of a patent system must be measured by its success in fostering the establishment of new manufactures within the realm. But surely if a capitalist is to risk his capital on a manufacture which has been patented, he wishes to feel some assurance that his patent is a valid one. In proportion as his confidence is increased in this respect will he be willing to risk the necessary outlay in experimental work, plant, organisation and publicity. He has to create a new demand; and he needs to be assured that when he has done so he will not be robbed of his reward by the competition which invalidation of his patent would make possible.

THE WRITER OF THE ARTICLE.

The Oogenesis of Lumbricus.

THE letters by Mr. L. A. Harvey and Prof. V. Nath in NATURE of November 27 and December 4 require comment. With regard to Parat's 'Vacuome Theory,' I have little of value to say. Some of my associates have a leaning towards the vacuome theory, and Prof. Nath does not mention Nassonov's Protozoa work, which is certainly in its favour. One of my most valued pupils, Dr. Bhattacharya, of Allahabad, after studying in Paris, embraced Parat's views, and naturally this has had some influence on me. In the oogenesis of Patella, a form investigated by Ludford, Woodger, Rodgers Brambell, and myself, it does seem that it is the sphere-substance and not a vacuole, which forms the fat globules. Reinvestigation of this form, in view of Parat's claims, might yield interesting results. If Mr. Harvey really wants to see Golgi bodies forming yolk and fat, I commend him to Patella, where the phenomenon is very clear.

I think that the evidence for Parat's views is getting stronger, but I do not care to commit myself to any more definite expression of opinion at the present moment. I consider that Prof. Nath's very interesting attempt to co-ordinate Parat's results, and his own valuable investigations on yolk formation, should be weighed carefully by future workers on the cytoplasmic inclusions.

Mr. Harvey is ill advised to reopen a controversy on his paper. I have no intention of repeating here the substance of our reply to Mr. Harvey's paper, which Dr. Nath and I have published in the recent issue of the *Q.J.M.S.* The reader of this letter should refer to that journal.

[1] This may be avoided by using the type of solution employed by L. de Broglie, NATURE, Sept. 25, 1926.

[2] This was suggested by the remark at the end of Schrödinger's paper, *Ann. d. Phys.*, Bd. 79 (1926), p. 734.