

Letters to the Editor.

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Gravitation and Light-Pressure in Spiral Nebulæ.

PROF. LINDEMANN'S idea that the spiral nebulae may be clouds of particles small enough to be repelled by light is of considerable interest. But we must remember that light carries with it another potential influence which it exerts when it encounters matter, namely, the power of ejecting an electron with an energy of the same order as that of the electron responsible for the light. Star-light, therefore, should be able to eject electrons with enormous energy; and this kind of induced radio-activity may have several partly unforeseen results. A stellar variety of spectrum is one of them, if a continuous spectrum can be composed of a multitude of fine lines, with gaps only where the specific exciting radiation was absent.

Unpolarised self-luminosity is surely more likely than mere reflection of incident light. The reddened light from the centre, observed by Mr. Reynolds, might well be a sunset effect, due to vision through a number of small blue-reflecting particles; the phenomenon does not harmonise so well with the idea of borrowed light.

I suppose that Dr. Jeans's spiral polar arms might occur in a Lindemann cloud as in any other enormous quasi-gaseous mass.

The fact that some few of these nebulae are approaching the galaxy, instead of rapidly receding, may be accounted for by the suggestion that in these few the particles have aggregated into larger groups (as they may under some kinds of electrification), so that gravitation once more predominates over light-pressure.

The excessive transparency of space seems limited to our own extensive neighbourhood, for in remote regions opacity will set in sooner or later, and all stray radiation—however enfeebled by distance—will sooner or later be re-absorbed, with perhaps exciting and generative material consequences. The birth as well as the death of matter seems not hopelessly beyond our scope. OLIVER J. LODGE.

Breeding Experiments on the Inheritance of Acquired Characters.

ALTHOUGH I agree with Dr. Kammerer in holding the opinion that somatic modifications do, sooner or later, affect the gametes or reproductive cells in such a way as to produce an inherited development of a corresponding change of structure, I regret that the evidence presented in his lecture printed in NATURE of May 12 is in some respects open to the objection that it is not in accordance with the present state of biological knowledge. Another objection, which may be partly due to the fact that the lecture is only a brief summary, is that the evidence does not include sufficient detail, or precise comparison with controls.

For example, Dr. Kammerer states that "Thanks to its enclosing membrane, the ovary of the Salamandra can be removed from the surrounding tissue as a whole," which, according to the context, is not the case with the ovary of birds. I have never heard hitherto of the existence of an enclosed ovary in any amphibian. Unless I am altogether mis-

taken, the germinal surface of the ovary is exposed to the coelom in Salamandra as in other Amphibia, and the ova escape through this surface as they do in birds, and not into an internal cavity of the ovary as, e.g., in teleostean fishes. If the latter were the case it would be very difficult to understand how ovarian transplantation could be carried out as in Kammerer's experiments.

The fact that Mendelian segregation occurs when naturally spotted Salamandra is crossed with naturally striped Salamandra, but not when it is crossed with experimentally striped specimens, suggests that in the former case the striped character is gametic, while in the latter it is not. But if the experimentally striped character is not gametic, what becomes of the heredity? Kammerer says doubtless both are inherited; but the long-established character obeys the Mendelian law, the new character does not. But T. H. Morgan has shown that new gametic characters in *Drosophila* obey Mendelian laws from their first appearance. Newness or oldness has nothing to do with Mendelism. A slight degree of heredity is possible, then the experimental striping causes only a slight change in the gametes. Then I presume the natural striping has been caused by exposure to yellow surroundings (soil) for thousands of generations and has become completely gametic, or almost so.

The difference in Mendelian behaviour then would be due to the fact that the natural striping is almost entirely gametic, the experimental striping almost entirely somatic. Such a result would agree with the result of my own experiments on the production of pigment on the lower sides of flat-fishes, supposing it to be true that spotted salamanders occur in Nature on dark ground, striped (yellow) specimens on light or yellow ground.

On the other hand, in the experiments on the results of ovarian transplantation Kammerer puts forth the extraordinary conclusion that the soma of the naturally striped female has no influence on the ova derived from a spotted female, but the artificially striped soma makes the ova derived from a naturally spotted female behave as though they came from a striped female. Here we have a complete gametic change due to somatic influence, while according to the Mendelian experiments there was little or no evidence of gametic change. Such contradictory results may be true, but it would require a great deal of corroboration to prove them.

Kammerer states that the case of *Ciona intestinalis* affords an *experimentum crucis*. He certainly exhibited photographs of living *Ciona* in the parents of which the siphons had been several times amputated. In these young specimens the siphons were "monstrously long," and had been so "from birth." Putting aside the fact that *Ciona* is not, I believe, viviparous, where were the controls? I have a very strong suspicion that all young *Ciona* when extended under favourable conditions (e.g. supply of oxygen and food) have "monstrously long" siphons. The evidence required is a large number of exact measurements, under the same conditions, of the siphons in the young of parents which were subjected to amputation, and in those of uninjured parents. J. T. CUNNINGHAM.

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May 12.

Vertical Change of Wind and Tropical Cyclones.

THE first step towards forming an opinion about the physical processes which operate in the formation and maintenance of tropical cyclones is a clear