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 intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Metallic Coloration of Chrysalids.

I HAVE read Mr. Mallock's letter (NATURE, November 3, p. 302) on iridescent colours with the greatest interest, but I cannot help thinking that in some cases his statements are slightly misleading. Mr. Mallock seems to imply that all the iridescent colours of the animal world are due to some form of interference. It is true that he restricts his statement to the cases he has examined, but it seems safe to assume that if many colours were due to other causes, Mr. Mallock would have met with some of these among the many hundreds of cases investigated during the twenty years in which he has prosecuted his researches.

I cannot lay claim to having carried out such extensive investigations, nor have I the training in physics which gives so much weight to Mr. Mallock's words, nevertheless I venture to urge that the phenomena of insect iridescence (not to speak of the rest of the animal world) include many cases which cannot be brought into the category of interference. These exceptions cannot be described now, but some have appeared in NATURE (September 30, October 7, and October 14, 1920), and a fuller account is to be found in Phil. Trans. Roy. Soc., B, vol. 211, pp. 1–74.

It would be interesting to know how Mr. Mallock explains the colours of the wings of those beetles, bees, and dragon-flies which the late Lord Rayleigh, Prof. Poulton, and myself have found would change colour neither on pressure nor when immersed in fluids under reduced pressure. Such beetles include the rose beetle (*Cetonia aurata*), many Buprestids, and other common insects, some of which Mr. Mallock is sure to have examined. Then there are the golden elytra of *P. resplendens*, which resist pressure and change to magenta on being polished; also the numerous iridescent tortoise beetles, the colour of which not only resists pressure, but even the removal by polishing of the thick surface-layer of chitin without altering its appearance. Among iridescent birds Mandoul has found that peacock's feathers resist pressure, and even hammering on an anvil; the bright-coloured feathers of Cotinga, which Haecker and Meyer attribute to "blue due to the scattering of small particles" by fine canals (*Porencanälen*) in the keratin, would, I presume, not be considered by Mr. Mallock as true iridescent objects.

Finally, there are the beetles to which Biedermann first directed attention (*e.g. H. africana*). Whether or not the cause of their colour is the same as in the last case, sections of the elytra in the plane of the wing are of nearly the same colour as the original beetle. Sections at right angles to the elytra are the same yellowish colour as the chitin.

Mr. Mallock also asserts that the colours of birds and insects are not, as has so often been said, due to diffraction. I admit that I know of no butterfly in which the principal colours are caused in this way, but what of the pale Morphos and other insects which, when the wing is partly turned, exhibit all the colours of the spectrum superimposed on the ground colour? These colours correspond exactly, in appearance and angle, to those of the replica diffraction gratings made in collodion from the wings.

Personally, I am prepared to agree that the wings of almost all iridescent Lepidoptera owe their colours to interference, but it would be interesting to learn Mr. Mallock's reasons for disagreeing with so eminent an authority as Prof. Michelson, who is of the opinion, on purely physical grounds, that all the colours of insects are due to selective metallic reflection, with the exception of the iridescent scale-bearing weevils (*e.g.* the diamond beetle), the colours of which he attributes to diffraction—a cause ruled out by Mr. Mallock.

I have referred only to the colours of insects and birds, but it would be most interesting to know to what forms of structure Mr. Mallock attributes such striking examples of iridescence as are to be found among the hairs of some mammals, the setæ of many marine worms, certain ferns and seeds, and many brilliantly coloured crustacea, some of which Mr Mallock must surely have examined.

H. ONSLOW.

3 Selwyn Gardens, Cambridge, November 9.

The Softening of Secondary X-rays.

A NUMBER of experimenters have noticed that when a beam of X-rays or γ-rays traverses any substance, the secondary rays excited are less penetrating than the primary rays. Prof. J. A. Gray (Franklin Institute Journal, November, 1920) and the present writer (*Phil. Mag.*, May, 1921, and *Phys. Rev.*, August, 1921) have shown that the greater part of this softening is not due, as was at first supposed, to a greater scattering of the softer components of the primary beam, but rather to a real change in the character of the radiation. My conclusion was that this transformation consisted in the excitation of some fluorescent rays of wave-length slightly greater than that of the primary rays. Prof. Gray, on the other hand, showed that if the primary rays came in thin pulses, as suggested by Stokes's theory of X-rays, and if these rays are scattered by atoms or electrons of dimensions comparable with the thickness of the pulse, the thickness of the scattered pulse will be greater than that of the incident pulse. He accordingly suggests that the observed softening of the secondary rays may be due to the process of scattering.

It is clear that if the X-rays are made to come in long trains, as by reflection from a crystal, the scattering process can effect no change in wave-length. On Gray's view, therefore, if X-rays reflected from a crystal are allowed to traverse a radiator, the incident and the excited rays should both have the same wave-length and the same absorption coefficient. If, on the other hand, the softening is due to the excitation of fluorescent rays, as I had suggested, reflected X-rays should presumably be softened by scattering in the same manner as unreflected rays. An examination of the absorption coefficient of reflected X-rays before and after they have been scattered should therefore afford a crucial test of the two hypotheses.

The double reduction in intensity which occurs when the X-ray beam is first reflected by a crystal and then scattered by the radiator made Gray's preliminary attempts to perform this experiment unsuccessful. In the September (1921) issue of the *Philosophical Magazine*, however, Mr. S. J. Plimpton describes a successful attempt to measure the absorption of the K lines from rhodium and molybdenum after being scattered by paraffin and water. He observed no change in the absorption coefficient of the rays after being scattered by the paraffin. Apparently his measurements were made on the secondary rays at comparatively small angles, and this, together with the relatively long wave-lengths employed, form the conditions under which the least change in hardness occurs when unreflected X-rays are used. I accordingly repeated Mr. Plimpton's