

### Audibility of Gunfire.

IN an article published in *NATURE* last August the suggestion was made that it might be possible to utilise gunfire for accurate measurement of the time of passage of sound over long distances, and in a postscript to the article I was able to record my success in listening at Grantham for the sound of guns discharged at Shoeburyness. On that occasion the interval between the firing of the gun and the arrival of the sound at Grantham varied between  $10\frac{1}{2}$  and  $11\frac{1}{4}$  minutes.

It is now agreed that 'abnormal' audibility at great distances is due to the refraction of the sound waves in a region at a height of 40 kilometres or more where the temperature of the air is comparatively high. More observations are required to elucidate various problems concerning this region. It is therefore gratifying that the War Office has given approval to the proposal to broadcast the times of firing of one of the guns at Shoeburyness and that the British Broadcasting Corporation is making the necessary arrangements. Full particulars will be announced shortly.

To take full advantage of the opportunity it will be necessary to supplement the aural observations by instrumental records of the aerial disturbance. I am therefore anxious to get into touch with any persons who are provided with apparatus suitable for the purpose. I believe that sets of sound-ranging equipment exist at certain universities and technical colleges. The co-operation of observers who had experience with such apparatus during the War and would be able to utilise it would be especially valuable; it would probably be possible to provide sets for them. It is anticipated that the best distance for successful observations will be at about 120 miles from Shoeburyness, but it will be worth while to attempt observations at rather greater distances, at such places as Nottingham, Birmingham, and Bristol. Records from comparatively near stations, which will be in the region of normal audibility reached by sound waves passing only through the lower atmosphere, are also desirable.

The experimental work on this subject has been done hitherto on the Continent. The trial which we are undertaking offers greater difficulties, as the explosion from which the sound is to originate will be so much smaller. Previous experience shows that as a general rule the zones of audibility are unsymmetrical, so that it is impossible to forecast where the sounds will be perceived. There is, however, reason to believe that in summer the region of abnormal audibility is likely to be to the north and west of the source of sound, so that our trial will be conducted in the most favourable circumstances.

I hope that those who may be able to co-operate in procuring instrumental records will kindly communicate with me at once. Others who are interested and will be able to make aural observations are asked not to write at present. Full details of the trials will be published as soon as possible.

F. J. W. WHIPPLE.

Kew Observatory, Richmond,  
Surrey, May 17.

### Phytophagic or Biological Races in Insects.

I HAD no desire to enter into any controversial discussion with Dr. Heslop Harrison (whose letter in *NATURE* of April 16, p. 562, I regret not to have seen earlier), either in regard to his experiments or the conclusions he draws from them; I sought only to obtain some explanation of an alleged 'new

principle in evolution,' since anything that deserved such a title would probably be serviceable to me in my studies. I was fully acquainted with the previous paper to which Dr. Harrison refers, as I had particular occasion to consider it at its first appearance. This earlier paper related to the fixing of certain qualities of colour, etc., under chemical influences in food, and I could readily accept it; the second described the fixing of hereditary habits by slight changes of food plants in three generations, and was much less easy of digestion. Yet on the strength of this latter case only (for the former does not touch the real point, and is only connected with the latter by an assumption of a common explanation, a conjectural modification of a wholly supposititious germ-plasm) Dr. Harrison proceeded to specify a number of actual instances of pairs of allied species, particularly stated to be 'in Britain,' and well known to me, and to suggest that this case explained their origin. If he did not mean that they originated in Britain, why describe their British association? It is no evidence that they are similarly associated in Siberia.

I do not deny that food produces differences in species; on the contrary, I know it produces differences. I do not know that it produces specific differences, nor, I think, does any one else. But I cannot believe that in any single instance pairs of allied species come into existence as Dr. Harrison describes; he excludes the essential factor of isolation (either in space or time), without which it would be impossible to keep the two stocks distinct until they were fixed. My case is that many of the most obviously close pairs of species feed not on allied but on the very same species of plants in the same way (I will instance *Tischeria complanella* and *T. dodonaea* on oak, *Heliozela sericiella* and *H. stammeella* on oak, *Antispila pfeifferella* and *A. treitschkiella* on Cornus, all exceedingly close pairs), and that the law of averages renders it unlikely that the direct chemical influence of the food plant is often more effective in other categories than in these, where it is nil.

I should like to add, in relation to Mr. W. H. Thorpe's letter (April 23, p. 602), that the genus *Hyponomeuta*, on which he is working, does, in my opinion, offer the most suitable material known to me for investigations on the direct effect of food plants, and I trust he will obtain valuable results.

EDWARD MEYRICK.

Thornhanger, Marlborough,  
May 7.

### The Chemistry of the Adrenal Cortex.

IN a previous paper (*Biochem. Z.*, 181, 433; 1927) it was shown that extracts of the adrenal cortex strongly reduce silver nitrate and iodine. This reduction could not be accounted for either by adrenalin or glutathione, and seemed to be specific to the interrenal tissue. To exclude any anticipation of function and chemical structure, the substance giving this reduction was named by its protocoll-number "C<sub>xii</sub>," being the twelfth substance prepared and examined in my work on tissue oxidation and the function of the adrenal cortex. Having been enabled to resume this work at the Biochemical Laboratory, Cambridge, the substance has been further investigated and finally isolated in crystals, which on recrystallisation showed a constant melting-point.

That C<sub>xii</sub> is definitely confined to the adrenal cortex can be easily demonstrated by the direct application of silver nitrate to the fresh-cut surface of the gland. If a cross-section is made through the (cow's) gland and the pieces immersed into a dilute