and fresh ideas. It does not then seem to be "merely a matter of a choice of points of view", as Prof. Fortescue suggests.

Sir Ambrose states that we do not have to alter the tuning of our condensers to receive high notes. Now it is an experimental fact that if we have a receiver of several stages—say three tuned circuits, each lightly damped—the high notes will be cut down very effectively, but we can restore them by tuning one circuit to the carrier and the other two slightly above and below respectively. This at first sight suggests that the side bands exist and tuning to them restores the high notes. However, it admits of as good an explanation on the other theory, for it is observed that the effect of 'detuning' thus is to reduce the overall damping of the circuits—as evidenced by a reduction in intensity and a drop in the maximum amplitude—while the resonance curve becomes a steeper-sided one with more flattened top. Thus more selectivity is achieved without loss of damping by three detuned circuits.

A. A. NEWBOLD.

"Meadway", Cheltenham Road,
Evesham, Feb. 13.

ALTHOUGH the letters recently published in NATURE on the above theory have indicated that some of the contentions in my article of Jan. 18 last are not generally supported by scientific opinion, yet some service has perhaps been done by it if only in eliciting the interesting letters from Sir Oliver Lodge, Sir Richard Glazebrook, Prof. Fortescue, Mr. Bedford, and others.

In addition to noting the importance of the remarks by Sir Oliver Lodge, I find the letter of Sir Richard Glazebrook very valuable, because he gives the proof that a receiver tuned to frequencies of $n+m$ or $n-m$ can be set in oscillation by a carrier wave of frequency n modulated by an acoustic frequency m. Now here we touch the very pith of the discussion. When a carrier wave modulated as above is sent out from a transmitter, can we say it travels through the space to the receiver as two distinct waves of frequencies $n+m$ and $n-m$ respectively? Or is it simply a single modulated wave which can actuate a receiver tuned to the two or more frequencies?

Since we can only detect any wave by a receiver, we have the same difficulty that we have in deciding the nature of a ray of white light and how it is the prism resolves it into an infinity of rays of various wave-length in the spectrum. That the prism itself has a good deal to do with the effect is indicated by the phenomenon of anomalous dispersion.

So it is also with the wireless receiver. We have difficulty in disentangling the pure space phenomena from those produced by the receiver itself. I am unable to see that those who object to my views on the wave band theory have given proof that the side waves exist in space and are not an effect due to the nature and operation of the receiver.

Apart, however, from philosophical questions on which differences of opinion may exist, there is the very practical question: What kind of receiver should anyone buy to obtain the best results in receiving broadcast music? An eminent scientific friend tells me in a letter that a wireless dealer told him he ought not to have a very selective receiver to get the best results. Prof. Fortescue seems to agree to some extent with this statement. On the other hand, my experience is that the most selective receiver gives the best results, and many would agree. It is, then, very important to ascertain whether good musicians with normal hearing using highly selective receivers and listening to music of a wide range of pitch detect any enfeeblement of high notes relatively to low notes and if this effect is absent in not very selective receivers. I hope some evidence on this point may be gathered in.

In the present state of jam in the ether with wave-lengths between 200 metres and 600 metres the wireless receiver makers require some guidance from scientific opinion as to the type of receiver they should make and advise their customers to buy. The reception from 5GB, 2LO, and the Brookman's Park short wave, of broadcast music in anything like satisfactory tone is becoming very difficult and demands some remedy. Is that remedy to be found in the use of hyperselective receivers or not? That is the question, and the answer to it given by experiment bears closely on the validity of the wave-band theory.

AMBROSE FLEMING.

Manor Road, Sidmouth,
Feb. 18.

Crossed Connexion of the Cerebral Hemispheres with the Muscles and Sense Organs.

PROF. ROAF'S interesting speculation published under the above title in NATURE of Feb. 8, (p. 203) is based on the assumption that the two eyes possessed by most vertebrate animals have arisen in the course of evolution from a single median eye such as is found in the free-swimming larva of an Ascidian. He argues (if I understand him rightly) that when the image of an object falls on the left half of the retina of an animal of this type, the appropriate response is a contraction of the muscles of the right side of the creature's body and tail, and that the efferent nerve paths from the brain will therefore be simplified if the afferent fibres involved end in the right half of the central nervous system. Such a view may be held to account for the central projection of the retina of each of the two eyes of a mammal in such a way that fibres from its upper half are connected with the superior lip of the calcarine fissure; and that fibres from its right margin are connected with cerebral points situated to the left of those with which areas of retina lying farther to the left are connected. (This may legitimately be inferred from the work of Gordon Holmes and others on cortical projection in man.) But Prof. Roaf goes further and suggests that it may also account for the fact that in most vertebrates the right eye is directly connected only with the left side of the brain, and the paths from the two eyes undergo complete decussation. At this point the argument seems to me to become less convincing.

Even if comparative anatomists were to assure us that a single median eye was indeed the direct ancestor of our two eyes (and, so far as I am aware, such an ancestry has not previously been suggested), we should need also to be told that the evolutionary development took the form of a bisection of this eye so that the right half of its retina became the retina of the resulting right eye. Alternatively we should have to assume that, in the most primitive vertebrates possessing two eyes, the left eye received images of objects lying to the right of those seen by the right eye.

Now it is usual in all vertebrates, other than a few birds and higher mammals, to find the two eyes placed laterally in the head, with the right eye forming images only of objects situated on the animal's right, and with little overlap between the two visual fields. Impulses from the right eye are carried to the left side of the brain, and are then relayed back to the right side in order (presumably) that contraction of muscles on this side of the animal's body may direct its movements towards the seen object. If Prof. Roaf accepts the rather improbable suggestions contained in the last paragraph as an explanation of the