frequency-changer both due to poor screening and through the first I.F. coupling units. Screening, however, can be improved at little expense, but the adjacent-channel selectivity of circuits tuned to 5,000 or 6,000 kc/s cannot be improved without great expense. In order to obtain effective filtering and screening it is necessary to use filters having as low a frequency as possible.

With regard to the frequencies stated by Mr. Kinross to give rise to phantom signals in the receiver quoted these can be dealt with as follows :—

$$2f_2 = 1{,}060 \text{ kc/s} = f_3$$

In my specification I admit this source of phantom signal, which can only be overcome by most elaborate screening.

These phantoms can only occur when f_1 is present in the second frequency-changer.

Furthermore, it should be noted that in the absence of a signal of a corresponding frequency, these phantom signals do not produce any sound from the receiver and when receiving a signal the comparative amplitude of any whistles will be extremely small.

Thus, provided that screening and the selectivity of the first I.F. units are fair, very few whistles and phantom signals need be caused by the second frequency-changer. The first frequency-changer is a different matter. Here the level of the incoming signal is low and any suggestion of harmonic interaction is serious. Furthermore, the input circuits are usually of comparatively poor selectivity and almost impossible to screen, e.g., the aerial and tuning condensers introduce real screening problems. To make matters worse it is necessary to have a large amplitude in the first oscillator to reduce background noises and this accentuates whistles due to harmonics of the oscillator.

If the first I.F. is made very high the second oscillator (f_2) will also be high, thus causing the protection of screening to be defeated by leakage through stray capacities of the coupling units.

Still again, a high I.F. increases misalignment between signal and oscillator circuits in a ganged receiver, and thus further increases the serious nature of any whistles which may occur.

Using a second oscillator frequency a little lower than the lowest frequency to be received in a receiver for the 550 to 1,500 kc/s bank results in only one phantom signal, viz., that specified by Mr. Kinross as when $2f_2 = 1{,}060$ kc/s $= f_5$, i.e., due to twice the second oscillator frequency beating with the first frequency, and this can easily be located so that it lies exactly midway between two channels 10 kc/s apart, thus being practically unnoticeable. All other harmonics of the second oscillator fall outside the tuning range of the preselector stages and the difficulties caused by this single phantom fade into insignificance when compared with the difficulties set forth in Mr. Kinross's article. Thus, to achieve success with the D.S.H. principle, it is necessary to bear in mind all the points enumerated, and the second frequency changer must operate at a high signal level.

A very high I.F. is only justified for covering a large frequency range without switching and then the cost of effective filters may make it a doubtful economy, but the use of a double superheterodyne extends far beyond this object.

My suggestion re the use of the sum of frequencies instead of the difference is based on the difficulty of obtaining sufficient selectivity with a high I.F. to sufficiently attenuate the first oscillator frequency before it reaches the second oscillator. If a high I.F. is used in combination with the sum of the oscillator and signal frequencies, the ratio between I.F. and oscillator frequencies is greater than if the difference is used, e.g., given a signal of 5,000 kc/s and an I.F. of 6,000 we can use an oscillator of either 11,000 or 1,000 kc/s.

A filter tuned to 6,000 kc/s attenuates a frequency of 1,000 more than one of 11,000, while a 1,000 kc/s oscillator is more stable than one of 11,000.

Only a little ingenuity is required to so apply both upper and lower side bands, i.e., both sum and difference frequencies so as to entirely avoid harmonic problems and produce a single-span receiver of satisfactory design.

Mr. Kinross's solution of the aerial filter problem of the single-span receiver is most ingenious, but there is a simpler plan available using stock material to take advantage of the idiosyncrasies of superheterodyne reception which I may be at liberty to communicate at a later date.

E. G. Beard,
Managing Director,
Ace Amplifiers Pty., Limited.
Cremorne, N.S.W.

(On account of the length of Mr. Beard's letter, a considerable portion has been omitted.—Ed.)

Critical Distance Valves and Beam Tetrodes

To the Editor, The Wireless Engineer

Sir,—In the past few months many foreign technical publications describing critical distance tetrodes have appeared. These publications infer or even state that these valves originated abroad. It seems extremely unfair that an impression should be thus given to the world that this type of valve is not, as in fact it is, originally a British product.

I am, of course, not able to speak for other workers in this field, but would like to place the following facts on record.

(1) The " beam tetrode " and the " critical distance tetrode " are one and the same type of valve.

(2) In both cases the stream of electrons forms a well-defined beam from the cathode to the anode and the anode is placed at the critical anode distance.

(3) In 1932 I published in England the first description of the " critical distance " effect (Bib. I). A valve having the anode at the critical distance was first described in this publication. Means were provided to direct a beam of electrons into the desired direction. Two types of electron directing means were employed. In one type of valve the focusing action of the control grid was employed alone. In another, this focusing action was assisted by plates connected to the cathode and suitably disposed between the positive grid and the anode. The desirability of reducing the interception of the space current by the positive grid was pointed out.

(4) Many hundreds of valves of these types were made and tested in my laboratory between 1931 and the date of this publication.

(5) In August, 1935, critical distance power valves were first marketed by The High Vacuum Valve Company, of 111, Farringdon Road, E.C.1 (England). The valves were shown, and one was operated in comparison with a pentode, on the "Hivac" stand at Radiolympia that year.

(6) The now acknowledged advantages of the critical distance tetrode construction over that of the pentode were first pointed out in my articles in *The Wireless World* in 1935 and in *The Wireless Engineer* (Bib. 2).

(7) In the above-mentioned original publication, I also described the principle of what is now termed "reverse feed-back," and suggested its use with critical distance valves. I show circuits for feeding back a portion of the voltage across an anode load to a control grid or accelerating electrode for the purpose of reducing the effective anode impedance.

(8) No other critical distance valve or beam tetrode was, as far as I am aware, marketed until after the announcement of the 6L6 by the Radio Corporation of America in April, 1936 (Bib. 3). This is some years after the research work in this country and over a year after the first commercial use of the Hivac tetrodes. The only feature of the R.C.A. tube that did not previously exist in those already made in England was that of making both control grid and accelerating grid of the same pitch and with the wires aligned. This feature is not, of course, new to the valve art; but unquestionably reduces the screen current appreciably when applied to a critical distance valve.

(9) There is also a curious and persistent emphasis in many quarters (Bib. 4) upon an endeavour to explain the operation of the beam tetrode in terms of the usual space charge effect, such as that first described by Mr. E. W. B. Gill, in 1925 (Bib. 5). Certain properties of secondary radiation are studiously ignored. It may readily be shown that an electron space charge having the properties of those known prior to the date of my original publication in England is quite incapable of explaining the critical distance effect. It is not possible, for reasons of space, to set out this matter more fully in this letter; but it is hoped in the near future to publish a report prepared by the laboratories of this Company on the mechanism of the critical distance.

For the same reason it is not possible to deal in full with the history and origin of critical distance valves. This is a matter which will probably be dealt with elsewhere; but I hope that the above information will be of interest to your readers. This country is entitled to any credit there may be for the production of the now widely used critical distance "beam tetrode." American vision and American publicity have since popularised the valve. It is a strange thing that British industry so often refuses to progress until our cousins across the Atlantic have first gone ahead.

J. H. OWEN HARRIES,
(Director, Harries Thermionics Limited).
London, S.W.19.

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Background Noise

To the Editor, The Wireless Engineer

SIR,—It was, of course, before the publication of my own views on the subject (see "A Theory of Fluctuation Noise," paper read before the Wireless Section of the I.E.E., 5th January, 1938) that Messrs. Percival and Horwood wrote their paper on "Background Noise Produced by Valves and Circuits" (*Wireless Engineer*, March, 1938, p. 128), and I suppose that in fact we must all have been working on the subject at about the same time. I am therefore rather concerned to find that the authors of this paper took for granted certain hypotheses which I consider are at least open to question, and certainly not valid without the most careful qualification.

1. *Space-Charge Limited Diodes.* The authors say that it can be shown that, if the sole effect of space-charge were the formation of a potential barrier, the effective temperature of the diode resistance would be the same as in the absence of space-charge. It would be interesting to know how this result is obtained and what the temperature is, since my own conclusions (*loc. cit.*) were that in the absence of space-charge the temperature is indeterminate, while in the presence of a potential barrier due to space-charge the effective temperature is approximately, but not exactly, equal to half the cathode temperature; and this is without considering the hypothesis of fluctuations of the potential barrier.

2. *Triodes.* The authors say, "However, it has been shown by Williams that the effective temperature of the valve resistance may be thousands of degrees absolute." This isolated quotation is, I think, hardly fair, for it does not mention the fact that Williams on the strength of such results, condemns the whole hypothesis of a "thermal" explanation of the fluctuation noise in both diodes and multi-electrode valves alike. But if the thermal theory is not indisputably established, does it not seem dangerous to build upon it the superstructure of a hypothesis that "in some way an amplifying valve magnifies the effective temperature," without any indication of the mechanism, or other supporting evidence? The present writer's view is that the additional noise in a multi-electrode valve is due to lack of complete space-charge limitation, such as would render possible a thermal interpretation, in the region between anode and the preceding electrode (i.e. control grid in a triode, screen grid in a tetrode).

3. *Screen Grid Valves.* If the current reaching the anode is in any case random, as the present writer believes (*loc. cit.*), can sharing with the screen in a four-electrode valve make it "more random"?

As seen at present, I fear that the method of measurement with anode joined to grid for A.C. will complicate rather than simplify theoretical interpretation, though it probably has advantages in the empirical classification of valves.

D. A. BELL.

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Essex.