DISCUSSION ON

"THE SPONTANEOUS BACKGROUND NOISE IN AMPLIFIERS DUE TO THERMAL AGITATION AND SHOT EFFECTS."*

Dr. F. B. Llewellyn (U.S.A.) (*communicated*): The paper contains a comprehensive theoretical and experimental study of the various sources of noise in vacuum tubes and their associated circuits. The point of view throughout is consistently independent, bearing little resemblance to those adopted by earlier writers in the field. It need hardly be said that such an independent study is stimulating and represents a welcome addition to the literature on the subject.

The American reader will be most gratified by those parts of the paper which lead to the same formulæ as had been derived in the United States. This applies in particular to the formulæ for shot-effect noise in the absence of space charge and for the noise caused by the thermal agitation of electricity in conductors. In both of these cases the results of the authors for the specific case treated by them agree with the more general results of T. C. Fry† and H. Nyquist.‡ On the other hand, the portions which disagree with earlier studies, though they do not represent as pleasant reading, may actually prove to be more valuable in the end because of the opportunity they provide for reconsideration and clarification of the questions at issue.

Such an instance arises in the treatment of noise under actual operating conditions, where the results differ widely from those which I obtained several years ago.§ Since the authors specifically state that they do not understand my argument, I shall attempt in this communication to clarify some of the details they appear to have found obscure.

Before doing this, however, it may be well to point out that the ultimate test of any such theory must be furnished by experiment, since the final purpose of the study is to search out the true state of affairs as regards noise and not to build up a mathematical argument for its own sake. Unfortunately, in the case in question, the experimental findings are in general very discordant, different tubes and different circuits yielding widely diverse results. Hence it is not possible to say of either formula, "It must be right, because it agrees with all the facts." In spite of this, however, the experimental results do seem to offer conclusive evidence that the formula (17) obtained by the authors is wrong.

To see how this comes about it is only necessary to observe that the formula under discussion purports to give the amount of shot-effect noise under conditions of complete space-charge limitation. In an actual tube this may be *augmented* by the noise from a variety of other causes, such as the thermal agitation of electrons and the flicker effect, and it also may be increased by incomplete temperature saturation. There appears, however, to be no way of *diminishing it*. In other words, the formula should represent an absolute minimum below which tube noise cannot go. Now when we compare the observed noise with that predicted by my formula, we do find in fact that in general the observed noise with complete space charge is always the greater; but it is less than that predicted by the authors.† This, as I see it, leaves the burden of defence decidedly upon their side.

Returning now to the aspects of my theory which the authors found obscure, I shall first attempt to clarify the problem by setting down the fundamental characteristics and properties of the flow of electrons; first without any space charge, and secondly with complete space charge. This I shall do in the form of the following brief, where I have attempted to arrange the items in accord with the steps in a logical reasoning process; beginning with the hypothesis on which the argument rests, next introducing experimental evidence, then deduction, and finally reaching a conclusion in each case:—

(1) *With no space charge.*

Electrons leave the filament independently of one another.

The random variations in the rate of emission determine the variations in current.

This is not affected by initial velocities, and hence depends on filament temperature only to the extent that the temperature determines the average rate of emission.

The internal tube resistance is infinite.

Variations in current are determined solely by variations in the rate of emission.

(2) *With complete space charge.*

The actual emission takes place in the same manner as above.

Space charge returns most of the emitted electrons to the filament.

Changes in the rate of emission do not affect the number of electrons reaching the plate.

* Paper by Messrs. E. B. Moullin and H. D. M. Ellis (see page 81). Reprinted from *Journal I.E.E.*, 1934, vol. 75, p. 395.

† "The Theory of the Schroteffekt," *Journal of the Franklin Institute*, 1926, vol. 199, p. 203.

‡ "Thermal Agitation of Electricity in Conductors," *Physical Review*, 1928, vol. 32, p. 110.

§ "A Study of Noise in Vacuum Tubes and Attached Circuits," *Proceedings of the Institute of Radio Engineers*, 1930, vol. 18, p. 243.

† With their own tubes, illustrated in Figs. 22 and 23, which seem never to have saturated well, the excess of calculated over observed noise is less than was found to be the case with my tubes when I tested the formula in 1929 which Moullin and Ellis now propose. The experiments of other writers lead to the same results as did mine. In particular, Figs. 11 and 12 in a paper on "The Schottky Effect in Low-Frequency Circuits," by J. B. Johnson (*Physical Review*, 1925, vol. 26, p. 71) show a comparison between measured noise and calculations made in 1925 by the same formula as that now given by the present authors and illustrate the fact that the measured noise is far less than that predicted by the formula when adequate space charge is secured. Reference may also be made to the work of Hull and Williams (*Physical Review*, 1925, vol. 25, p. 147, Section 17 and Table 4), Thatcher and Williams (*Physical Review*, 1932, vol. 39, p. 472), and Thatcher (*Physical Review*, vol. 40, p. 114).

The chance of an electron getting past the space-charge barrier and reaching the plate depends on the initial velocity with which it left the filament and on the amount of space charge.

Variations in plate current are therefore determined by variations in the initial velocity and by variation in the space charge.

The first of these is related to the filament temperature by the Maxwellian distribution law. This introduces the factor kT.

The second is determined by the internal a.c. resistance of the tube.

Variations in plate current are determined solely by the filament temperature, and the a.c. internal resistance.

With the aid of this brief the form of the noise equations in the two cases can readily be inferred. Without space charge, the variational voltage set up by the fluctuating current is proportional to the external impedance $|Z|$. The mean square value of the variations resulting from perfectly random emission is proportional to the average value of the emission current I. Hence the mean square noise voltage is proportional to $I|Z|^2$, which is in accord with Fry's formula.

With complete space charge we have to deal with an impressed e.m.f. rather than with an impressed current, as was the case without space charge. The energy of this impressed e.m.f. is proportional only to the average energy of emission of the electrons, and hence to kT, where T is the filament temperature. The power expended by the impressed e.m.f. is proportional to the square of the e.m.f. divided by the resistive component of the impedance through which the resulting current flows. If, then, a short-circuit for alternating currents were placed across the vacuum tube, it follows by Thevenin's theorem that the mean square value of the effective internal e.m.f. would be proportional to the product of the impressed energy, kT, and the internal a.c. resistance of the vacuum tube. Hence the mean square value of the internal e.m.f. acting in the plate circuit and resulting from the initial velocities of the electrons is proportional to kTr, which is in accord with my formula, and agrees with the formula of Nyquist for a resistance r at the filament temperature.

When an external resistance R is connected to the vacuum tube, having, as the present authors agree, an effective internal e.m.f. proportional to kT_0R, the total e.m.f. around the circuit is the sum of the two impressed e.m.f.'s. In finding the mean square value of this, the authors question the validity of placing product terms of the form E_1E_2 equal to zero in any frequency interval df. They offer a possible explanation, based on the random character of the phase of E_2 with respect to E_1, which is correct as far as it goes. In a more general sense the mean square value of the sum of two time functions which are individually independent of one another and which are of the "random" character indicated by the form of the equations is given, within a frequency interval df, by the simple sum of the mean square values of the two components taken separately. A commonly used example of this property occurs in textbooks on the theory of white light.

Coming now to the case of noise in the presence of partial space charge, and the factor $\partial I_{av.}/\partial I$ which the authors seem to have misunderstood, I first wish to express my appreciation of the remarks of Mr. C. L. Hirshman in the discussion.

The authors state that my view appears to be that "the pelting of the anode circuit by electrons produces *ipso facto* no effect. . . ." This is nearly, but not quite, a correct statement of my position. To show the slight difference, consider a stream of electrons proceeding across the vacuum tube at an exactly uniform rate, so that the interval between the arrival of any two successive electrons is the same for all. Under these conditions the current is the sum of the separate currents from each individual electron. The total current from a single electron is a pulse of some shape which starts at the time of the starting of the electron but which flows in all parts of a series circuit simultaneously, and not locally where the electron happens to be. The current from all of the electrons can be obtained by superposing the pulses from each individual electron. Each pulse has the same shape with respect to time as all the other pulses, however, and the time between the starting of any two successive pulses is the same. Hence, even though many pulses may have started before the first one has been damped out, the configuration will ultimately repeat itself every time a new electron starts. It therefore has a periodicity equal to the time between successive electrons. Moreover, this is the period of the lowest-frequency component present in the resultant current.

As an example, a current of 1 microampere represents the flow of $6{\cdot}28 \times 10^{12}$ electrons per second, and this would be the lowest of the fluctuation frequencies if the flow were exactly uniform.

It is consequently appropriate in noise studies to investigate the effects of variations from this precisely uniform flow. It is the variations from the uniform which produce the noise we are seeking, and not the pelting of the anode circuit by electrons.

The shot formula developed by Fry gives the mean square variation of the electrons emitted from the cathode. As remarked above, and pointed out by Hull and Williams* in 1925, the space charge tends to smooth out these variations and my factor $\partial I_{av.}/\partial I$ is a measure of its success.

In Fig. 17 the present authors show curves of anode current and noise plotted as a function of anode potential, and state that for these curves "there is no change in filament temperature and hence no change of I or $dI/dI_{av.}$." Now, the factor $\partial I_{av.}/\partial I$ which I have used is the slope of the curve of plate current versus total filament emission taken at the value of filament emission and plate potential actually employed. In mathematical form the factor is obtained by finding the variation in the functional equation

$$I_{av.} = I_{av.}(I,\ V_a)$$

so that

$$\delta I_{av.} = \frac{\partial I_{av.}}{\partial I}\delta I + \frac{\partial I_{av.}}{\partial V_a}\delta V_a$$

This process is exactly analogous to the method of finding the variation in plate current resulting from

* *Physical Review*, 1925, vol. 25, p. 147.

variation in grid and plate potential. In this case we should have

$$I_{av.} = I_{av.}(V_g, V_a)$$

so that
$$\delta I_{av.} = \frac{\partial I_{av.}}{\partial V_g}\delta V_g + \frac{\partial I_{av.}}{\partial V_a}\delta V_a$$

Let us paraphrase the authors' statement from the standpoint of this latter relation. In place of Fig. 17 there would be a curve of some function of $I_{av.}$ (analogous to their noise curve) plotted against V_a. Their statement would now read " In this there is no change of grid potential and hence no change of V_g or $dV_g/dI_{av.}$."

This statement may or may not be true, depending on how one defines $dV_g/dI_{av.}$. It is evident, however, that $dV_g/dI_{av.}$ is not the reciprocal of $\partial I_{av.}/\partial V_g$ as defined in the variational equation above. It happens that $\partial I_{av.}/\partial V_g$ has been given the symbolism μ/r_a, while $\partial I_{av.}/\partial V_a$ is $1/r_a$, and the variational equation now reads

$$\delta I_{av.} = \frac{1}{r_a}(\mu V_g + V_a)$$

and no one would say here that because V_g happened to be held constant while V_a was varied in taking a set of data, it would follow that μ had the same value irrespective of V_a. In fact one of the important recent developments in vacuum tubes has been just such variable-μ tubes in which the variation in μ partly accounts for the characteristics of the tubes.

In an exactly analogous way, in the noise equation, the fact that I was held constant while V_a was varied does not mean that $\partial I_{av.}/\partial I$ was constant.

From a physical standpoint it is easy to see that the factor would not be constant, for $\partial I_{av.}/\partial I$ is a measure of the amount of space charge present, and this in turn is related to the anode potential whenever its value is sufficiently high to overcome the condition known as " complete " space charge, yet not so high as to obliterate the space charge entirely. The only safe way to determine the behaviour of the factor is actually to vary the filament temperature a slight amount and observe the behaviour of the plate current.

Several of the authors' figures contain curves of plate current plotted against filament current. In particular, Figs. 15, 16, and 20, may be cited. In none of these does the slope of the space-current curve approach zero. This being the case, it follows that the characteristics of the noise will qualitatively obey the kind of formula used by the authors, since $(\partial I_{av.}/\partial I)^2 I$ will vary in a rough way as does $I_{av.}$ when the space charge is incomplete as was the case in their tubes.

In conclusion, I hope that this communication has clarified any points in my paper which may have been obscure, and I anticipate that the present authors will find results more in accord with my views when they secure tubes capable of operating with more complete space-charge saturation of the filament.

Messrs. E. B. Moullin and **H. D. M. Ellis** (*in reply*): We are very grateful to Dr. Llewellyn for sending a contribution to the discussion of our paper. When our views have differed from those of Dr. Llewellyn we have commented in a spirit of friendly inquiry and not of dogmatic criticism: it is clear that Dr. Llewellyn has accepted these comments in the spirit in which they were made, and we are very glad he has given a more detailed explanation of his views of this intricate problem. On the whole, we do not feel we are able either to accept or to rebut Dr. Llewellyn's views: the whole problem is intricate and obscure, and we do not feel that the experimental evidence is sufficient to clear the whole matter. We are not convinced that release from the space-charge barrier is perfectly regular, and we do not consider Dr. Llewellyn's exposition of this proposition to be conclusive. At the top of the second column of page 100 we suggest that the difference of view is due to a different method of dividing the total effect into components, and we think that this is borne out by Dr. Llewellyn's tabular arrangement of effects with and without space charge. We have regarded the random pattering as being due to the variation of initial velocities, but we still do not see that this necessarily introduces an explicit factor kT. We have not succeeded in forming a mechanical picture of thermal agitation from Nyquist's derivation of the formula, whereas we have succeeded in forming such a picture from our own derivation from equipartition. Our derivation makes it hard for us to associate thermal agitation with an electron stream. We know no derivation of the expression for shot voltage which seems to us entirely satisfactory and for which the physical premises are not open to criticism. For example, what is there in our method of developing the expression which would be invalidated if the electrons arrived at the anode by conduction along a wire? Yet in such circumstances experience shows that the shot voltage is zero. We feel there should be some way of deriving the expression from the principle of equipartition of energy, if account is taken of the fact that the agitation velocity of the electrons is not a Maxwell distribution, because there is a specified average rate of arrival of electrons having super-normal energy. If this view should lead to the established expressions, we think that our difficulties, and, if we may say so, Dr. Llewellyn's difficulties, would disappear and both interpretations would be comprehended in a more general interpretation of the effect.

Since our paper was published, one of us has made further measurements to see whether V_s^2 is proportional to the joint impedance of the valve and circuit, in the condition of a constant average anode current and potential of the anode. There seems to be no possible doubt that in such circumstances V_s^2 varies as $[R\rho/(R + \rho)]^2$, and in this respect our formula (17) and the formula of Dr. Llewellyn are in complete agreement. Further careful tests, however, similar to those described by Fig. 22 and using a valve of the same type, showed that V_s^2 was not truly proportional to $I_{av.}$. As in Fig. 22, V_s^2 was proportional to $I_{av.}$ for currents between 3 and 12 mA, and in this range the value deduced for e was sensibly half the true value. For values of $I_{av.}$ less than 3 mA, however, it seemed that the departure from proportionality was greater than could be accounted for by uncertainty in the value of ρ. When $I_{av.}$ was 1 mA, the apparent value of e was 20 per cent in excess of the true value. We are obliged to Dr. Llewellyn for pointing out our misconception of the factor $\partial I_{av.}/\partial I$, used by him.

In conclusion, we wish once more to thank Dr. Llewellyn

for accepting our implied invitation to furnish more details of his picture of the physical mechanisms involved. We do not feel that the present state of knowledge is sufficient to prove that one picture is wrong and the other right, but we think that Dr. Llewellyn's contribution to the discussion will help readers of our paper to form their own mental picture of these obscure effects. We hope before long to make more experiments which may help to clear up the difficulties, and we assure him that we shall approach such experiments with a perfectly open mind in respect to points where his view and ours have an apparent or real difference.

DISCUSSION ON

"MEASUREMENT OF THE ANGLE OF INCIDENCE AT THE GROUND OF DOWN-COMING SHORT WAVES FROM THE IONOSPHERE."*

Prof. J. Hollingworth (*communicated*): I think there can now be little doubt, as a result of the investigations described in this paper and the recent one by Dr. Walmsley, that the short-wave transatlantic transmissions in general show an angle of incidence of 76° to 80°, a fact which is of great practical importance.

I am inclined, however, to think, apparently with Dr. Walmsley, that the mechanism causing the waves to follow this path is still very imperfectly understood. The usual idea of multiple hops, while generally giving figures of the right order, does not seem to stand up to critical analysis.

In particular, the present author shows an angle of incidence varying from 73° to 90° and ascribes this to an increase of ionization lowering the effective height of the layer. Assuming a "double hop" and a distance of 5 400 km, this involves a change in layer height from about 740 to 150 km, which seems excessive. If, on the other hand, the change is caused by changes in the number of hops or shifts of the point of reflection from the F layer to the E layer, two difficulties arise. In the first case, which goes to the root of the problem, no explanation is provided of why the great part of the energy is concentrated in a ray making a certain number of "hops" with almost total exclusion of the others. That this can be so is, I think, unquestionable; I have had very definite proof of it in my own experiments; but a ray at 74° with a layer height of about 200 km requires 4 or 5 hops, whereas both 3 and 2 are still geometrically possible, and at present we seem to be still without definite information of the reason for this selective property. If, however, the ray shifts from one layer to the other we should expect discontinuities in the curve, of the type found by Appleton and Naismith.

Instrumentally I favour the author's method of depending on phase rather than intensity measurements, as it has practical advantages for the experimenter who is not equipped with two similar measuring sets, and its calibration and lining-up are somewhat easier. I hesitate, rather, however, at its use on stations off the designed direction, since in these positions it will receive a component of the horizontal electric force in the direction of propagation. This component is not strictly absent unless the earth is treated as a perfect conductor. It may, of course, be negligible; but as the constants of the earth at frequencies of this order are still not quite certain it may produce errors, since in a signal consisting of two circularly polarized components with opposite directions of rotation the fades are not simultaneous on the horizontal and vertical components of the electric force. Personally I always try to avoid such difficulties by only taking observations when the ellipses on the cathode-ray tube are near their maximum values, as it so often happens that when a large steady ellipse shrinks owing to a fade it becomes unsteady and fluctuating owing to the presence of such residuals.

The statement on page 158 as to the reversibility of the ray track is strictly only true if the effect of the earth's magnetic field is neglected; though the effect of this in general is probably small.

Mr. A. F. Wilkins (*in reply*): I agree with Prof. Hollingworth that the ascription of the total summer diurnal change of angle of incidence to variations in equivalent height of the layer necessitates excessively large variations of that height. The real cause of the variation of angle has been made clearer as a result of frequent 20-metre pulse emissions which have been made from Lawrenceville during the past year. These tests have usually taken place between 1 400 and 1 700 G.M.T., so that it has not been possible to study the variations in number and angle of incidence of the received rays throughout the whole period of normal working of these 20-metre stations. The trend of angle of incidence outside the period of the pulse tests may be deduced, however, from the seasonal trend. The outcome of these tests has been that, during a summer afternoon, the received energy is concentrated in a single group of rays of small time-separation, and it is thought that the whole group is associated with one order of multiple reflection from the F region.

* Paper by Mr. A. F. WILKINS (see page 154). Reprinted from *Journal I.E.E.*, vol. 75, p. 863.