found, however, for the *total* radiation averaged over altitudes from 3–10 cm Hg only an east-west asymmetry of +7 percent (at geomagnetic latitude 20°N and for zenith angle 60°). It is to be hoped that this very important result will be checked in further experiments. The author has previously suggested[4] that this small east-west asymmetry of the *total* radiation at high altitudes may perhaps be interpreted as being the result of the combination of a high *positive* east-west asymmetry for the hard component, assumed to be produced mainly from primary positive protons, and a high *negative* east-west asymmetry for the soft component, assumed to be produced mainly from primary *negative* protons. Direct measurements at high altitudes of the east-west asymmetry of the soft and hard components *separately* are, therefore, highly desirable.

Apart from the problem of the east-west asymmetry it is also very difficult to interpret from the proton hypothesis the conspicuous difference between the latitude effect of the soft and that of the hard component, the first increasing strongly with increasing altitude, reaching values about 70–80 percent at the Pfotzer maximum,[5] the latter increasing only slowly with altitudes from about 12 percent at sea level to about 18 percent at an altitude of 21 cm.[6]

From the earlier hypothesis assuming the soft component to be created by cascade multiplication of primary electrons from the top of the atmosphere and down, the latitude effect could be satisfactorily accounted for, as shown by Heitler,[7] assuming a primary energy spectrum of approximately the well-known inverse power type. If, now, as in the proton hypothesis, the soft component is to be produced not as a secondary, but as a *tertiary* radiation, but still from the same primary energy spectrum and still practically at the top of the atmosphere, as in the form of the proton hypothesis given by Heitler *et al.*[2] (i.e., protons being transformed in single processes into long-lived pseudoscalar and short-lived vector mesons, the latter decaying at once into electrons and neutrinos), energy is lost in the transformation of the primary radiation into the soft component. Consequently the primary energy necessary to produce the same primary electron energies at the top of the atmosphere as before is much higher. As the primary energy spectrum decreases with increasing energies and as the field sensitive region is only 2–15 Bev, this energy decrease means that the latitude effect becomes smaller than that calculated from the earlier electron hypothesis which agreed with experiments. (Unfortunately Heitler and Walsh do not give the intensity *vs.* altitude curves of the soft component at equator and at 50°N, which would follow from their theory.)

In the Feynman-Bethe form of the proton hypothesis,[2] the protons produce in multiple processes only the long-lived pseudoscalar mesons ($\tau \sim 2\times 10^{-6}$ sec.) decaying into electrons and neutrinos all the way down through the atmosphere and not only at the top of the atmosphere as in Heitler's form of the theory. In this form the difference between the latitude effect of the soft and that of the hard component would be even more difficult to interpret than in Heitler's form, partly because the energy decrease in the transformation of the primary radiation into the soft component is here much larger owing to the multiple processes, partly because after a few radiation units the soft component will be in equilibrium with the mesons, as is known from the calculations of Euler and Heisenberg.[8] All through the lower part of the atmosphere the latitude effect of both components should consequently increase approximately in the same way, in contrast to the difference observed.[5,6] Another difficulty for this form of the proton hypothesis is to explain the increase of the ratio of soft to hard intensity in the lower part of the atmosphere. A further consequence is that the electrons of the soft component produced in this way would be expected to be less energetic than those produced in the Heitler theory. Not only the intensity of the soft component but also the electron spectra at different altitudes would, therefore, be interesting to have worked out and compared with the spectra found experimentally.

We thus conclude from the experimental evidence as it stands at present that it seems necessary to assume at least one more component in the primary radiation, in addition to the protons which seem to be the primaries of most of the hard component. Whether this second component consists of electrons, negative protons[4] or perhaps higher charged nuclei as suggested by Swann[9] can, of course, only be decided by further experiments. *Especially a more systematic investigation of the intensity, latitude effect, and east-west asymmetry* of the soft and hard component *separately* at high altitudes is highly desirable.

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On the Possible Use of Brownian Motion for Low Temperature Thermometry

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IN a recent communication,[1] Lawson and Long proposed two interesting methods for low temperature thermometry using spontaneous electrical fluctuations. In the present note it is desired to point out that a drastic modification of the first method would be required, and that a fundamental principle, apparently overlooked, invalidates the conclusions in favor of the second method proposed.

Their first method consists in measuring the thermal voltage fluctuations generated in a resistance of large value at the grid of an amplifier valve. The arrangement suggested is one of an ohmic resistor $R(\sim 10^9\Omega)$ in parallel with inevitable circuit capacity ($\sim 30\mu\mu$f) resulting in a frequency response 0–5 c/sec. Concomitantly, the shot noise in the amplifier valve is assessed by the familiar expression,[2] $3/g$, for a valve operating in the space charge region. This

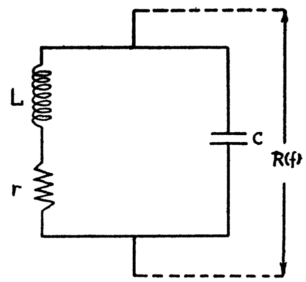

FIG. 1. Equivalent circuit of a crystal.

formula, however, is not valid except for frequencies sufficiently high to exclude the incidence of "flicker effect."[3,4] Indeed, below a few hundred cycles/sec., flicker effect becomes entirely dominant and the fluctuations may rise to the order of 10^2–10^3 times the theoretical value of "true" shot noise as predicted by the formula above. For this reason all modern fluctuations experiments, apart from those designed expressly to examine "flicker" effect, are carried out at frequencies of the order of 10^5 c/sec. or higher to exclude the relatively slow, gross variations characteristic of "flicker" phenomena.

Since the high resistance required must now be *dynamic* in character the question arises whether such a resistance can be provided and utilized. Fortunately, the high conductivity of pure metals near absolute zero favors the provision of a coil of low ohmic resistance and hence a tuned circuit of very high dynamic impedance. Indeed, one might visualize the use of a super-conductor for this purpose.

The next difficulty that arises is the effect of the dynamic input resistance of the valve amplifier itself resulting from reaction. This can be estimated from the expression for the input admittance to the valve

$$Y_i = i\omega\{C_{gc} + C_{ga}(1-m_a)\}, \quad (1)$$

where m_a is the complex amplification of the amplifier stage. It appears very unlikely that it would be possible to maintain the real part of Y_i (i.e., the dynamic input conductance) sufficiently small; (in this connection, however, a cathode follower might prove feasible, the input admittance being very low). In addition, it would of course be essential to ensure that the over-all ohmic leakage between grid and earth was also large compared with the dynamic resistance of the tuned circuit.

A rather more fundamental criticism must be directed against the second part of Lawson and Long's letter. They suggest that effectively increased Brownian voltage might be derived from the piezoelectric fluctuations of a quartz crystal arising primarily from the random energy of mechanical vibration. They make a detailed numerical estimate of the voltage generated, namely

$$\langle V^2\rangle_{Av} = 1.4\times 10^{-12}\cdot T, \quad (2)$$

(reference 1, Eq. (5)) and suggest that the internal resistance of the quartz is only a few thousand ohms, so that an input resistance of 10^5 ohms is adequate.

It must be pointed out that for *any* system in complete thermal equilibrium, *irrespective of the particular electrical or electromechanical mechanisms involved or of the number of causes of the irregular movement,* the Brownian voltage generated between any two terminals is given by Nyquist's formula

$$\langle V^2\rangle_{Av} = 4R(f)kT\Delta f, \quad (3)$$

(reference 1, Eq. (1)) where $R(f)$ is the resistance (at frequency f) measured between the two terminals concerned. The classical example of the galvanometer has been analyzed in detail by Ornstein,[5] and Professor M. H. L. Pryce kindly informs us that similar conclusions can be drawn for the more general case of a linear inter-connected system of n degrees of freedom.

It must, therefore, be possible to calculate Eq. (2) directly from the crystal impedance as viewed from its (electrical) terminals. It is evident, however, that this step cannot be reconciled with Eq. (3), (bearing in mind that the bandwidth ($\Delta f \doteqdot f/Q$) is only of the order of one cycle/sec.) unless the crystal impedance is in fact very high. The paradox is resolved by considering the equivalent electrical circuit of the crystal; to a sufficient approximation this may be represented as in Fig. 1.

It is true that r is only of the order of $10^3\Omega$, but at resonance $R(f)=Q^2\cdot r$ and Q is in the order of 10^3–10^4, so immediately showing that the relevant impedance is exceedingly high. Thus taking $R(f)\doteqdot 2.5\times 10^{10}\Omega$ (clearly a reasonable value on the quoted figures) and $\Delta f \sim 1$ c/sec., we immediately arrive at (our) Eq. (2) from the fundamental formula (3) without further ado.

It therefore appears that, contrary to Lawson and Long's conclusion, the electrical fluctuations of a crystal, despite its mechanical Brownian movement, do not differ fundamentally from those of any other electrical network and can offer no inherent superiority as a low temperature thermometer. The only possible advantage is of a practical nature, since the crystal may more readily provide a very high electrical dynamic resistance, in the use of which, however, all the difficulties mentioned above would still have to be overcome.

Similar conclusions must of course apply to any other physical system of coupled elements in thermal equilibrium.

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Further Remarks on the Possible Use of Brownian Motion in Low Temperature Thermometry

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In a recent letter, Brown and MacDonald[1] have reviewed our discussion concerning the possibility of using Brownian motion for low temperature thermometry and have raised a number of objections concerning which we would like to make some additional remarks.