for all energies and $W \simeq e^{10} \sim 10^4$. It thus follows that it will take only about $W \cdot t \sim 10^{-9}$ second to bring an electron up to energies $E' \sim I$. This shows that in strong fields (but below the breakdown strength where a stationary state is still possible) the equilibrium distribution of electrons is such that a considerable fraction of electrons have energies of the order $E \sim I$.

It should be emphasized that by a direct acceleration of an electron, which moves in the direction of the field, an ionization process may be caused even at field strengths below breakdown. Since, however, the reverse process exists (i.e., as electrons may as well be retarded by the field) the field will, in a first approximation, not change the number of ionization processes since it accelerates as many electrons as it retards. Thus it has to be decided by the second approximation, i.e., by the energy transfer A, whether or not a net increase of ionization processes is caused by the field.

There is still a possibility of obtaining a stationary state if we consider the recombination of an electron. Very little is known about recombination in solids, but it seems to be rather certain that such a process will take much longer* than 10^{-9} sec. Therefore, even if in principal a stationary state may eventually be reached, this state would be entirely different from the state where $E' > I$.

We shall now connect the above model with the actual case with which we have to deal in the breakdown problem. We note the experimental fact that for field strengths near the critical field of breakdown, but below it, there exists a stationary current in the dielectric medium. This means that in fact we deal with an electron gas in a stationary state. We shall not, at present, investigate theoretically how these electrons come into the conductive levels of the dielectric but accept it as an experimental fact. Then, it seems that the above considerations about the possibilities of reaching a stationary state may be applied.

The experimental evidence too seems to justify this condition. Hippel's condition (used by Seeger and Teller) would yield theoretical values for the breakdown field which are too high by a factor ~ 5 in the case of the alkali halides. The fact that Seeger and Teller obtained better values is due to a compensation by the different mistakes, discussed in §§2 and 3.

Finally I should like to mention in this connection that experiments by S. Whitehead, A. E. W. Austen and W. Hackett carried out at the laboratory of the British Electrical and Allied Industries Research Association (kindly communicated to the author before publication) have now confirmed most of the conclusions of the author's theory.*

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* The cross section for recombination with emission of light is $<10^{-20}$ cm.

* Cf. Austen and Hackett, Nature **143**, 637 (1939).

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Remarks on the Dielectric Breakdown

R. J. Seeger and E. Teller
The George Washington University, Washington, D. C.

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In the foregoing paper Fröhlich touches upon a number of questions that are of importance in the theory of breakdown of insulators. Since we do not find ourselves in agreement with his conclusions, we should like to clarify our point of view.

In the first place Fröhlich correctly remarks that in our treatment[1] of the interaction between an electron and a lattice the variation of direction in the motion of the electron has been neglected. Such variation of direction may, in fact, become very important either for electrons of high speed

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[1] R. J. Seeger and E. Teller, Phys. Rev. **54**, 515 (1938).

or for crystals at high temperatures. In these two cases an electron after being scattered by a lattice vibration may still have a considerable velocity, and hence the direction of its motion is of importance. For electrons of low speed, however, and for crystals of sufficiently low temperature (i.e., the actual cases of interest in our paper) an electron will be practically at rest after a collision with a lattice vibration so that its direction of motion does not matter very much.

From a systematic point of view, of course, it would be desirable to have a treatment which includes the changes of electronic momentum and does not have to restrict itself to the calculation of energy loss. Such a treatment seems to exist, at present, only for those cases where Born's approximation applies. In the present problem this approximation is not valid owing to the strong interaction of an electron with the vibrations of the ions. It may be hoped that an approximation similar to the one used for molecules can be developed. In such an approximation the electronic wave function would depend on the displacements of the ions, while the force acting on the ions would be obtained by averaging over the electronic wave function. Such an approximation would probably also permit a more detailed discussion as to what extent Bohr's theory of deceleration of α-particles can be used to describe the interaction of a free electron with the lattice vibrations of an insulator.

A second point raised by Fröhlich is the form of the interaction between a free electron and the ions of the lattice. Fröhlich states that in the expression for the force acting on the ions $e^2/\kappa r^2$ the quantity κ must have a value between unity and ϵ_0 (e = charge of the electron, r = distance between electron and ion, ϵ_0 = dielectric constant for a crystal in which the ions may be polarized but may not be moved). This is correct, however, only so long as one assumes that the electric effect of the displacement of an ion can be represented by moving a charge of the magnitude e. The treatment given in our paper avoids such an assumption of a charge rigidly bound to an ion.

Nevertheless Fröhlich is correct in objecting to our final formula since that formula gives $\kappa = (\epsilon_0 \epsilon)^{\frac{1}{2}}$ (ϵ = actual dielectric constant of the crystal) rather than ϵ_0 for the limiting case of a charge of magnitude e rigidly bound to the ions. In this connection Professor Mott has kindly pointed out to us that since the longitudinal rather than the transversal lattice vibrations should appear in our derivation, the frequency of the former should appear in our final formula. Now this longitudinal frequency is considerably higher for a finite wave-length than that of a transversal vibration since electric charges accumulating in the nodes of the wave contribute to the restoring forces. This difference of frequencies has often escaped notice.. Actually as soon as the wave-length becomes long as compared to the dimensions of the crystal the difference between longitudinal and transversal vibrations vanishes and a dependence of the frequency on the shape of the crystal appears instead. If the frequency of the longitudinal vibration is used in our formula then the correct value of ϵ_0 is obtained for the case of rigid ions. Thus it was apparently not correct for us to use the "*reststrahl*" frequency which—as one would assume—corresponds to a transversal vibration.

The difficulty of finding valid values for longitudinal and transversal vibrations becomes, however, apparent from measurements of Fermi and Rasetti[2] on the Raman effect of rocksalt. They find a continuous range of Raman shifts which, in accordance with selection rules, they interpret as first overtones of the lattice vibrations. The maximum shift agrees closely with twice the value of the "*reststrahl*" frequency. Now the latter frequency is supposed to be transversal. The maximum shift in the Raman effect on the other hand should be at least equal to twice the longitudinal frequency. Thus the question which values should be used for the longitudinal and transversal vibrations in crystals remains to be settled. In cases where the frequencies of the relevant vibrations are known it seems to us best to use the formula derived in our paper.

We agree, of course, with Fröhlich that for very fast electrons the screening by the bound electrons of the lattice would not be complete and that in this case κ should be smaller than

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[2] E. Fermi and F. Rasetti, Zeits. f. Physik **71**, 689 (1931).

given by our formula. However, if the time of passage of an electron near an ion is longer than the reciprocal values of the electron-frequencies within the ion, simple arguments would lead us to expect that the electronic polarizability has its full effect as has been assumed in our paper.

The factors discussed so far are of importance only for the details of the theory. The main point of difference, however, remains, namely, whether the condition for the breakdown proposed by von Hippel and adopted by us is the correct one, or whether the condition set forth by Fröhlich is preferable. Our above discussion, however, is closely related to the answer to that very question since for electrons of very high speed Fröhlich's approximations are justified whereas for electrons of low speed (which are the important ones if von Hippel's criterion holds) our treatment seems more adequate.

Fröhlich has shown that, if the capture of free electrons can be neglected, then no stationary state exists for $E' < I$ (E' = electronic energy above which there is a net acceleration of electrons, I = ionization energy of the lattice). However, it is not quite clear that under such conditions even a value of $E' > I$ will make a stationary state possible. In fact, even if $E' > I$ is assumed, some electrons may gain sufficient energy from the field to produce ionization in the lattice. Inasmuch as no corresponding reverse process is considered, no stationary state can exist.

We believe, on the contrary, that the capture of free electrons may be a highly probable process. In particular, the possibility of "trapping"[3] of free electrons may be of importance. Although such removal of free electrons is not strictly the reverse process of ionization, a subsequent diffusion of the trapped electrons in the lattice may reunite the electrons with the halogen atoms. In our paper we have emphasized that in order for an electron avalanche to be built up it is not necessary that the field should be able to accelerate electrons of all energies. In order to find out how much smaller fields suffice it would be necessary to discuss in detail the trapping and the capture of free electrons. Such discussion would also give some information on the temperature dependence of the breakdown.

Unfortunately a direct experimental decision between the two proposed criteria for breakdown is difficult because of the appearance of the effective mass of the electron as an unknown constant in both theories. It would be interesting in this connection to investigate the breakdown of a crystal in which all relevant vibrations are hydrogen-vibrations. By comparing the breakdown of such a crystal with the breakdown of a similar crystal in which hydrogen is replaced by deuterium we could eliminate the effective mass of the electron and get a better insight into the breakdown mechanism. According to our theory the breakdown fields at low temperature should be inversely proportional to the square root of the reduced masses. If for the moment the logarithmic factor in Fröhlich's expression be neglected, the ratio of the breakdown fields at low temperatures should, according to his theory, be the ratio of the reduced masses to the $(-\frac{3}{4})$th power. The logarithmic factor brings the ratios predicted by the two theories more closely together. But even so the ratios should differ by approximately 10 percent so that an experimental decision is possible. The most clear cut example of this kind would be a comparison of lithium hydride and lithium deuteride. It is, however, very difficult to obtain these substances in a form which permits one to measure the breakdown strength.

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[3] L. Landau, Physik. Zeits. Sowjetunion **3**, 664 (1933); A. von Hippel, Zeits. f. Physik **93**, 86 (1934).