

ductivity of the water should be nearly the same in either case, and therefore should not preclude the operation of the process in the case of melting. If, however, in accordance with their mechanism, the positive charges on the growing ice surface were furnished by the water, there would seem to be no reason for presuming such a reverse potential. In the case of melting, this charge would merely be returned to the water as opposed to being incorporated in the ice during freezing.

Gill and Alfrey state that a potential of sign opposite to that observed during freezing was observed during melting and that this opposite potential "was due to positive charges in the ice which had not time to leak completely away before melting commenced, and which entered the water as the ice melted. (There had been a slight leakage over the insulators of the negative charge in the water.)". The situation described would result in a reversal of current at the ice-water interface but would not, of course, result in the described reversal of sign of the potential difference. As we have stated², the potential difference is often reversed during the solidification of doubly distilled water without added impurities (the principal contaminant in the water probably becomes different as freezing progresses because the ice incorporates or rejects different impurities to different extents). Since it would be difficult, in the experiment described, to determine whether freezing or melting was going on at the interface, we believe it is likely that the reversed potentials thought to accompany melting actually occurred while freezing was still going on.

Our discussion² of the applications to meteorology of the electrical phenomena accompanying freezing is quite parallel to that of Gill and Alfrey, except that we were able to point out that the usually observed contaminants in rain water collected aloft cause a 'freezing potential' which would result in a thunderstorm of the usually observed polarity.

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¹ *Nature*, 169, 208 (1952).

² *Phys. Rev.*, 78, 254.

THE main point at issue occurs in the first paragraph. It is whether or not there is an abrupt potential change at the surface, whatever name it be called—contact potential or potential barrier. The original statement by Workman and Reynolds that the potential barrier is at the water-ice interface means in the ordinary use of language that the potential change, which may be more than 100 volts, is a complete discontinuity there, or at best is spread over a distance of a few molecular diameters, involving an electric force of millions of volts a centimetre. The expanded statement seems to imply much the same.

We, on the other hand, think this voltage is produced by a volume density of charged ions frozen into the ice. The voltage change is spread over several millimetres and the electric force is reasonable. In addition, the fresh ions do not have to cross this large potential barrier (and it is difficult to see how they could) but increase it by being frozen on the outside.

We entirely agree that the phenomenon is not due to the water, but to what is dissolved in it.

Experiments done with a variety of solutions did prove that charge was frozen into the outer layers because, if the outside of the ice, which was formed around a central metal rod, was gradually melted off by immersion in warm water, a positive charge could be measured continually arriving in the water until a few millimetres had thawed off, when it ceased.

The suggestion of Workman and Reynolds that the results of a somewhat similar experiment with distilled water described in our original communication were due to a mixture of impurities, and that freezing was continuing when we imagined melting was taking place, cannot possibly explain these latest results.

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A Sensitive Method for the Detection of Traces of Water in Oil

CORRESPONDENTS have pointed out that the derivation of the expression for the wetness of an oil in terms of the manometer reading, as given in the communication under this title in *Nature* of March 8, p. 412, is probably incorrect.

A close examination has been made of all the test results obtained. An incorrect value was used for the density of acetylene used in calculating wetness by the carbide test. The values for the water content of the oils quoted have therefore been recalculated¹ and are as follows:

OM 13	(a)	0.0057 per cent
	(b)	0.0076 " "
OM 35		0.012 " "
Castor oil BP	(a)	0.679 " "
	(b)	0.086 " "

These values are of the same order of magnitude as those commonly accepted for 'clear and bright' oils containing water in solution. The humidiscopes readings are in wide disagreement with these values, but until further theoretical investigation is completed (in collaboration with correspondents) the reason for this must be left in abeyance.

Examination of the humidiscopes readings obtained for emulsions appears to show that a relation holds of the form

$$p^2 \propto w,$$

where p is manometer reading, w is water percentage added to dry oil. Suggestions as to possible implications would be welcome.

My apologies are due to readers who may have endeavoured to use the equation given in my original communication; at the moment, the use of the instrument can be recommended only where facilities for making emulsions of known water content for calibration purposes are available; continuous re-circulation through a high-speed gear-pump for 20 min. is satisfactory.

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¹ Based on the value $d_{0,760} = 1.173$ mgm./ml., from Esbach, O. W., "Handbook of Engineering Fundamentals" (1946).