

Reply

By JAMES E. McDONALD

*Institute of Atmospheric Physics, University of Arizona,
Tucson 25*

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There appears to be residual disagreement between Mr. Mason and me as to the significance of a merely large temperature variation in nucleation rate in any

argument aimed at demonstrating that the -40°C transition is due to homogeneous nucleation. Since my logical objections to his point of view were stated to my satisfaction in my earlier paper, I need not add further comment here.

That a theoretically predicted transition temperature of -26°C represents a rather poor reward for any efforts to explain the observed -40°C transition is agreed; but this result is, on the other hand, much more encouraging than that which followed from a revision of Mr. Mason's calculations omitting surface distortion "correction." I do not think that my method of obtaining σ can correctly be described as Mr. Mason does in his discussion of my paper. I have, in fact, as pointed out in my paper, followed Mr. Mason's own approach, refining it where this seemed possible, and adding a very approximate correction for surface distortion. It was only in making an estimate of this last effect that I employed the simplifying assumption that the intermolecular forces are roughly proportional to latent heats.

The reference to Rowlinson's work is helpful. It leaves unexplained, however, the substantial discrepancy between my own value of 6200 cal/mole and Mr. Mason's 5100 cal/mole for the bond energy, since only a portion of this discrepancy can be accounted for by allowing for the next-nearest-neighbor and more distant interactions which Mr. Mason chose to omit from consideration.

A recheck reveals to me no basis for the assertion that the value of σ_s' should, if calculated correctly, be 120 erg/cm² and not 122 erg/cm².