It seems that Russell's idea of a correlation between frost action and some one isotherm has merit. Frost action is doubtless of great importance in the asymmetry of valleys in humid lands, though perhaps of less importance in arid regions.

In the North America Continent nearly all the area north of the isotherm of 32° is glaciated and only post-glacial valleys can show asymmetry. Many of these are, however, too youthful. It may be that all the humid area north of the isotherm where frost action is prevalent would in turn develop asymmetry. Obviously the nearly unglaciated northern portion of Asia is the region for testing this question. Asymmetry in the valleys of the northward flowing rivers of Siberia has already been reported and attributed to right-hand deflection. The characteristic of smaller valleys, so far inadequately described, would appear to be critical.

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METEORIC DUST

SCIENCE NEWS, in SCIENCE for January 22, contains an article on "Meteoric Dust," to one paragraph of which I feel bound to take exception.

It is not a fact that I have been collecting meteoric dust "over a period of thirty years." Of course, meteoric dust, like rain, falls alike on the just and the unjust; but that is not scientific collecting.

It is true that certain samples of dust, from roofs, towers, flues and locomotive smoke boxes, have been examined by me, using quite simple, even crude apparatus. In the outdoor dusts there were found both magnetic globules and glassy globules. Flues of anthracite furnaces show occasional magnetic globules, and the dust from locomotive smoke boxes contains them in large proportion, whence I take it that locomotives are efficient, if not sufficient, producers of the magnetic globules in atmospheric dust.

The glassy globules appear in dusts from house roofs and towers; *e. g.,* in deposits on the flat roof of Building C, Harvard College Observatory, where they are in the winnowings of thirty years; in dust from a house roof in Chippewa Falls, Wisconsin; and in the dust on the upper platforms of the Pilgrim Memorial Tower, Provincetown, Massachusetts.

Such globules were reported by Thoulet in 1908 as existing in the dust from towers of the cathedral in Nancy, France. I do not find them in anthracite flues or in locomotive dusts. But I have not examined dusts from glass works or from mineral wool factories; and the samples from locomotives have been too few for generalization. Some well-equipped mineralogist might pursue the subject to advantage.

Until some one discovers a criterion for the identification of meteoric dust, the only course is the exclusion of alternatives. This exclusion seems to be satisfactory in three cases: the sample from the ship *Joshua Bates,* studied by Ehrenberg, the magnetic globules of Murray and Renard, found in the "red clay" deposits of the deepest seas, and the sample collected on November 16 and 17, 1897, in Dublin, Ireland, and analyzed spectroscopically by Hartley and Ramage.

Thoulet was probably hasty in assigning a cosmic origin to the Nancy globules; and a considerable search for alternatives is necessary before calling the glassy globules which I have found meteoric or cosmic.

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THE OESTRUS-PRODUCING HORMONES

THE recent note by Marrian and Butenandt[1] contains several statements which can hardly be substantiated if one refers to our original papers. Our first paper[2] on theelol appeared in the October issue of the 1930–31 *Proceedings* of the Society of Experimental Biology and Medicine and Marrian's[3] paper was received by us on October 28, 1930. We characterized the triol as an unsaturated trihydroxy compound having a formula $C_{18}H_{24}O_3$ and a melting-point of 273° C. The tri-acetyl derivative had a melting-point of 126° C. In a later paper,[4] the one about which Marrian and Butenandt complain, Marrian's data are compared with ours in adjacent columns of Table I and some discussion is given in the text. The table contains Marrian's carbon and hydrogen analysis, the molecular weight, melting-point, formula and the fact that Marrian found 3 hydroxyls per molecule. In the text we expressed the belief that Marrian might have an isomeric triol or an impure triol and that, if the latter were true, the contaminating substance might be theelin.

It is also stated that we have ignored the evidence of Marrian's analytical data. We doubt whether carbon and hydrogen analyses would detect the presence of amounts of theelin ($C_{18}H_{22}O_2$) up to 10 per cent in otherwise pure theelol ($C_{18}H_{24}O_3$), whereas the melting-point would certainly reveal the presence of the impurity. In our preparations we have frequently obtained about ten times as much theelol as theelin.

The complaint that Butenandt's conversion of $C_{18}H_{24}O_3$ to $C_{18}H_{22}O_2$ has not been properly recog-

[1] G. F. Marrian and A. Butenandt, SCIENCE, 74, 547, 1931.

[2] E. A. Doisy, *et al., Proc. Soc. Expt. Biol. and Med.* 28, 88, 1930.

[3] G. F. Marrian, *Biochem. J.,* 24, 1021, 1930.

[4] S. A. Thayer, L. Levin and E. A. Doisy, *J. Biol. Chem.,* 91, 655, 1931; E. A. Doisy and S. A. Thayer, *J. Biol. Chem.,* 91, 641, 1931.