Letters

Terms for Temperatures

The Appleman-Braham interchange of viewpoints on nomenclature for temperature reductions that go below a predetermined reference point [*Science* **129**, 1296 (1959)] prompts me to observe that none of the terms suggested are as free of ambiguity as they might be, and that some of them can be misleading. The point of contention was the choice between *sub, super,* and *under* as a prefix to the verb *cool* for description of such temperature reductions.

My first point is that the verb itself can be improved. I have found the verb *chill* to be more descriptive in speaking of temperature drops considerably below ambient levels, admittedly not to the same degree that *heating* rather than *warming* distinguishes a substantial rise in temperature, but still enough to establish a suitable distinction in the mind of the reader. English does not provide a common verb that bears quite the same relationship to *cool* that *heat* does to *warm*.

The use of *under* as a prefix for either verb conveys to me an image exactly opposite to the one intended; a system which is "undercooled" means one that experienced a temperature drop *less* than the magnitude to be expected from the context. To a lesser extent, *sub* as a prefix suffers the same handicap.

On the other hand, the prefix *super* implies a marked deviation from the norm, which is not true necessarily for the downward departures from freezing points or saturation temperatures that are meaningful in the systems usually encountered. *Over* describes such a departure better, because it can connote any deviation, no matter how small. It also avoids Braham's objection—a valid one in my opinion—to mixing words of different derivation.

To say that water or a saturated solution is "overchilled" registers in my mind a sharp impression that the system is at temperatures near a critical point (not just "cool"), and that it is to some degree (not necessarily great), below this reference temperature.

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European Degrees

I should like to support Seiden's opinion [*Science* **129**, 933 (1959)] that it would be useful from the standpoint of international scientific contacts if some professional group would attempt to standardize the anglicization of continental degrees.

May I suggest that it be considered whether the American Association for the Advancement of Science, representing if possible also the American Medical Association, American Institute of Biological Sciences, American Institute of Chemists, American Chemical Society, and other interested U.S. organizations, could undertake such an endeavor, specifically in collaboration or consultation with the analogous British societies.

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Marine Fungi and Limnoria

The recent work by D. L. Ray and D. E. Stuntz [*Science* **129**, 93 (1959)] contains some suggestions and implications concerning our beliefs that need to be corrected.

First, we have not claimed that "marine wood-boring animals do not attack wood or become established in it unless the wood is first invaded and 'conditioned' by marine fungi." However, certain of our studies referred to have indicated that a thorough examination of the role of marine fungi in the destruction of wood certainly is due.

Second, we have not made any statement or implication that "*Limnoria* is unable to attack sterilized wood." There are so many unknown factors in the evidence so far presented that any statement based on this concept would be drawing essentially unsubstantiated conclusions.

Third, we have not expressed a belief that "*Limnoria* will not attack wood until its surface has been 'conditioned'." This "conditioning," if it is of any effect, surely involves physical, chemical, and biological factors, none of which has yet been given adequate study.

A reasonably careful reading of our two articles would show clearly that we were presenting specific observations and results of experimentation concerning marine fungi and were calling attention to the need for careful studies of the interrelationships between the marine organisms which infest submerged wood. We do believe that deterioration is a composite process in which the contribution made by any organism involved in the biologic complex, at any stage in the process, should be given critical evaluation.

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Whether, as Reynolds and Meyers claim, we have erred in our interpretation of the meaning and implications of their work (*1, 2*) can best be judged by reference to what we said, and to their published reports, on which our remarks were based. We wrote (*3*):

"Believing that fungal infection always occurs prior to attack by marine wood borers, especially *Limnoria,* they [Meyers and Reynolds] *suggested* that there *might* be a relationship between fungi and wood-destroying animals." (Italics added).

Both parts of this sentence require comment in the light of Reynolds and Meyers' objections. First, the only *belief* that we credit to these authors is that of holding that fungal infection of submerged wood always precedes marine borer attack. Our statement that this is their belief is based upon the following three quotations. In the concluding paragraph of their article in *Science* (*1*) they said:

"The vigorous fungal infestation of submerged wood prior to borer attack represents a biological phenomenon that investigators of marine wood destruction should not ignore. In northern areas, winter fungal infestation of wood is evident. Hence, in the early spring, when borer activity increases rapidly, the animals have available a wood substrate thoroughly infected by a variety of marine fungi. The interrelationships within this biota are being studied in our laboratory."

And in the same article they wrote:

"Vigorous attack [by fungi] upon submerged wood in boreal and northern temperate areas during winter months is accompanied by no, or very slight, borer damage. A similar situation occurs in subtropical localities, however, with a considerably shorter period of fungal attack prior to borer infestation."

Finally, the following sentences appear in another article (*2*, p. 10, paragraph one):

"The fungal infection that occurs before the borers attack the wood has interesting biological implications. In northern areas, especially, it may facilitate the activities of the borers, not only by making it easier for them to enter the wood, but also by providing them with a source of food."

We still think that it is fair to say that Reynolds and Meyers indicated a belief that fungal infection always precedes attack by marine borers. Concerning the second half of our sentence, we believe that the excerpts quoted above and other comments in the two articles cited do support our statement that Reynolds and Meyers suggest that there may be

a relationship between marine fungi and marine wood borers.

With reference to the three specific objections enumerated in their letter, we will respond to the second one first, for here Reynolds and Meyers have legitimate grounds for complaint. In our article in *Science,* we inadvertently cited the report that "*Limnoria* is unable to attack sterilized wood" as reference number 3 [Reynolds and Meyers (*2*)], but it should have been cited as reference 4 [Schafer and Lane (*4*)]. We apologize for this typographical error in the bibliographic citation. To set the record straight, Schafer and Lane (*4*), reporting work done at the Marine Laboratory of the University of Miami, state in their abstract: "*Limnoria* of both sexes and all ages did not survive when they were allowed to feed only on sterile wood in sterile sea water. No fecal pellets were produced under the conditions of this experiment." Supporting evidence is given in the text of the paper.

Reynolds and Meyers' first listed objection is that we included their work among the cases where we said it has been ". . . stated, suggested, or implied that marine wood-boring animals do not attack wood or become established in it unless the wood is first invaded and 'conditioned' by marine fungi" (*3*). Immediately preceding this comment we cited work from three different groups of investigators. Perhaps it would have been more precise to say that Becker, Kampf and Kohlmeyer (*5*) stated, Schafer and Lane (*4*) suggested, and Reynolds and Meyers (*1, 2*) implied.

It is our opinion that Reynolds and Meyers' work reported in their two papers does indeed convey the implication that marine wood-boring animals do not attack wood or become established in it unless the wood is first invaded and "conditioned" by marine fungi. A careful rereading of these papers has not altered this opinion. Independent evidence that we are not alone in this interpretation may be found in the table of contents of the issue of *ONR Research Reviews* in which one of their papers (*2*) appears. Following the title is this explanatory statement (authorship unknown):

"Investigations now underway show that marine fungi are one of the chief contributors to the deterioration of submerged wood. They not only vigorously degrade wood, but also prepare it for the entrance of other wood destroyers."

Finally, regarding the third point made by Reynolds and Meyers in their letter, we did not claim that they "expressed a belief" that *Limnoria* will not attack wood until its surface has been "conditioned." (We do not believe this either!) Our reference to their work in this connection was only to the observation that a period of time elapses between the exposure of wood to sea water and its invasion by *Limnoria.*

The many questions of keen interest that arise from the results we have submitted, conflicting as they do with those from other investigators, deserve fuller discussion than is appropriate in this letter or was possible in our report. Until such further discussion can be engaged in, we wish only to make our position abundantly clear. We have undertaken a critical evaluation of just one point: whether there is any relationship between marine fungi and *Limnoria* attack. Our results indicate that there is not. We do not believe that this closes the case; it is necessary now to determine why our results differ from those of other workers.

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References

1. S. P. Meyers and E. S. Reynolds, *Science* 126, 969 (1957).
2. E. S. Reynolds and S. P. Meyers, *Office Naval Research, Research Revs.* (Dec. 1957), pp. 6–11.
3. D. L. Ray and D. E. Stuntz, *Science* 129, 93 (1959).
4. R. D. Schafer and C. E. Lane, *Bull. Marine Sci. Gulf and Caribbean* 7, 289 (1957).
5. G. Becker, W.-D. Kampf, J. Kohlmeyer, *Naturwissenschaften* 17, 473 (1957).