was modified so that appropriate transposition behavior was seen after injections of brain extracts from donors that had been given relational training, even when these donors had never experienced the newer (correct) transposition test stimulus. Brain extracts from untrained donors did not alter circle preference in any way, and this preference was remarkably close to the chance, 50 percent, choice of each test stimulus.

The second phase of experiment 1 indicated that extracts significantly modified behavior under nonreinforced test conditions for 72 hours, but under reinforced conditions even at 248 hours after injection; a large increase in the performance of the experimental recipients could be seen with only 2 days of reinforced training on the donors' original discrimination problem. This suggests that extracts may facilitate learning of a task long after any effect of these extracts can be detected with nonreinforced test trials.

Finally, the effects in these experiments were strong and enduring. This could have been due to (i) optimization of behavioral and biochemical parameters; (ii) the high incentive value of the protein and vitamin solution used as the reinforcer; (iii) possible nutritional or metabolic effects of the protein and vitamin solution; (iv) the nature of the task, that is, perhaps relational or conceptual tasks produce especially strong and persistent learning and memory (as they certainly seem to do in studies of human memory processes); or (v) various combinations of the foregoing factors.

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# Evaporation Retardation by Monolayers

Wu (*1*) has conjectured that "the retardation of evaporation due to monolayers in the field under turbulent wind may actually be due to the wave-damping effects of these layers," rather than to their influence on the passage of water through the liquid-air interface. In fact, it has already been noted (*2*) that some reduction in evaporation should follow from the changes in air flow associated with wave damping. In light of Wu's comments, however, it seems desirable to stress that the effect is not "significant enough to constitute the major mechanism responsible for evaporation retardation by monolayers in the field" (*1*).

At Lake Hefner, near Oklahoma City (*3*), it was found that evaporation rates $E'$ (in centimeters per 3 hours) could be well predicted from the relation

$$E' = 1.214 \times 10^{-3}\, U_8\, (e_s - e_8) \quad (1)$$

where $U_8$ (in meters per second) is the wind velocity 8 m above the surface, $e_s$ (in millibars) is the vapor pressure at the surface, and $e_8$ (in millibars) is the vapor pressure at 8 m. This formula has been found to be widely applicable (*4*). During the experiments at Lake Hefner it was found also that

$$(U_*/U_8)^2 \approx 3.4 \times 10^{-3}$$

where $U_*$ is the friction velocity (*3*). Incorporation of this and other conversions gives

$$E = 2.63 \times 10^{-2}\, U_*\, (c_s - c_8) \quad (2)$$

where $E$ has the dimensions of kilograms per square meter per second, and $c_s$ and $c_8$, the vapor concentrations, are in kilograms per cubic meter.

Now Eq. 2 may be written as

$$\Omega_8 = \frac{1}{2.63 \times 10^{-2} U_*} \quad (3)$$

where $\Omega_8$ (in seconds per meter) is the resistance to the transfer of water vapor from the surface to a height of 8 m, where it may be assumed that the water vapor is effectively mixed with the atmosphere.

In Wu's analysis (*1*) the greatest reduction in $U_*$ resulting from wave damping is from about 0.2 to 0.1 m sec$^{-1}$. If this reduction is applied to Eq. 3, this gives an increase in the aerodynamic resistance $\Omega_8$ from about 190 to 380 sec m$^{-1}$. This is distinctly the maximum increase in $\Omega_8$ predictable from Wu's data; all other decreases in $U_*$ due to smoothing are much less than 0.1 m sec$^{-1}$. A film spread from mixtures of 1-hexadecanol and 1-octadecanol has a typical resistance of 300 sec m$^{-1}$ (*5*). Thus in this extreme instance a film increases the original resistance to evaporation from 190 sec m$^{-1}$ to about 680 sec m$^{-1}$. If we ignore thermal compensation (*5*), this indicates a total reduction in evaporation of about 72 percent. Without the change in $\Omega_8$ the total resistance is 490 sec m$^{-1}$, corresponding to a reduction in evaporation of 61 percent. Altogether, in this most favorable instance the aerodynamic effect is not significant.

Apart from this point, it is by no means certain that the transport coefficient of water vapor is lowered linearly with those friction velocity reductions due solely to smoothing. Studies on momentum transfer (*6*) to rough and smoothed surfaces show that the surface traction imparted to a water surface by a given wind is independent of the surface roughness. With this division in momentum transfer it is difficult to generalize about mass transfer.

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2 November 1971

In a recent report (*1*) Wu analyzes in detail the effect of wind upon the retardation of evaporation by monolayers and concludes that this retardation, in the field and under turbulent wind, "may actually be due to the wave-damping effects of these layers." Furthermore, he argues that the wave-damping effect operates through a change in the detailed flow pattern of the air near the surface. This last argument is based on the partially implicit assumption that wind-shear velocity is "the *governing* parameter for evaporation" (italics mine). If correct, this view relegates the well-established barrier-to-evaporation effect of monolayers to the level of a minor contribution.

In reviewing previous work Wu remarks also that "the other transport processes, convective circulation, diffusion, and convection, have not been considered . . . to be directly affected by monolayers." This statement suggests that Wu is not aware of a report (*2*) (anticipating all his references) in which I described an attempt to find the effect discussed by him. My experiments were carried out on a bench-top scale but were interpreted in terms of the resistances to the transport of water vapor in the film and the air phase and the resistances to the coupled flow of heat in air as well as in the liquid phase. The results showed that the resistance of the film itself was significant and was little affected by wind, and also that the film had the effect of increasing the resistance in the liquid phase but not that in the gas phase. In a companion report (*3*) I traced, by direct observation, the increased resistance in the liquid to changes in the convection pattern caused by the presence of the film.

I found the lack of a perceptible effect of wind upon the resistance in air rather surprising and concluded: "Thus, the quieting effect of a monolayer . . . —the calming of troubled waters—which is so prominent in field tests of evaporation control . . . seems not to affect the rate of evaporation under the conditions of these small-scale experiments" (*2*).

Thus there is at least some experimental evidence, based on a less detailed but more complete analysis of the transport phenomena, that the mechanism discussed by Wu, attractive as it is, may not be an important one. It is possible, as originally suggested, that the conclusion from my small-scale experiments is not applicable to large-scale field tests, but then it is not clear why an effect which would be controlling on a larger scale would not even be detectable in the laboratory.

There can be little doubt that the change in the ripple pattern produced by a monolayer does change the air flow pattern close to the surface along the lines described by Wu. What is unknown, however, is whether this is also the region where the important resistance is located. This is a question that Wu has not answered, although he begins by listing many of the other factors involved in the overall process. It is an important question because, if the principal resistance is located above or below this area, large changes in a minor component of the overall resistance would not be significant.

The importance of considering carefully the various resistances involved in evaporation has already been brought out by Langmuir (*4*) and his co-workers in their pioneering papers on retardation by monolayers. It appears, therefore, that a more complete quantitative treatment of the field problem is required before an analysis such as Wu's, which not only provides an additional mechanism of monolayer action but attempts to replace the one originally proposed, can be accepted.

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4 November 1971

I appreciate the comments by Mansfield and Mysels, but I do not agree with the points they raise. Mansfield's quantitative comparison is rather unfair, because, when the structure of the wind boundary layer varies as a result of the change in the surface roughness, the temperature and humidity structures should vary accordingly. In addition, the reduction in wind-shear velocity due to the wave-damping effects, as discussed in my report, has been measured directly and substantiated by observations, whereas the resistance to evaporation obtained from quiescent water has been shown (*1*) to be drastically reduced over the wavy surface. According to Mansfield, the change of resistance from a wind-shear velocity of 20 cm $sec^{-1}$ to a wind-shear velocity of 10 cm $sec^{-1}$ is 240 sec $m^{-1}$. There is a computational error in Mansfield's calculation; the coefficient in his equation 2 should be $2.08 \times 10^{-2}$. On the other hand, the resistance to evaporation is 300 sec $m^{-1}$ for quiescent water and is expected to be drastically reduced for disturbed water. Taken together, these data suggest that the reduction of wind shear may be the major mechanism responsible for evaporation retardation by monolayers in the field.

Mansfield's remarks concerning momentum transfer are incorrect. As a result of recent intensified research, our understanding of wind-wave interaction has greatly improved. By now, it is generally accepted that there is a unique relation between the shear velocity and the surface roughness (*2*). The trouble here seems to be that the laboratory results quoted by Mansfield may be obtained in a short fetch and under low wind velocity. The wind is, therefore, in an aerodynamically smooth regime. The results of some more recent laboratory investigations can be found in (*3*).

I apologize for not being aware of Mysels' report (*4*). However, his study, although interesting in its own right, seems not directly related to my discussion. The wind boundary layer certainly cannot be developed in a dish 15 cm in diameter. Mysels' interpretation is definitely correct when there is no wind, or when the wind serves merely to convect the moistened air from a dish. On the other hand, my discussion pertains to the natural condition in which the wind boundary layer is turbulent and the water surface is hydrodynamically rough.

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