present in the ATCC master culture, nor, as Fennell reports, was it present in the NRRL master culture. Why should the aflatoxin-producing variant show up in New Brunswick in three separate transfers (two from NRRL and one from ATCC) and not in Washington or Peoria? We suggest the New Brunswick laboratory should have paid more attention to Fennell's admonition that the subculture sent to her from New Brunswick was "heavily infested with culture mites."

Mites are notorious for cross-contaminating mold cultures when infestations become heavy, and their populations spread from either culture or commodity habitats.

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20 December 1976

I would not wish to enter into the fray of the El-Hag and Morse (*1*) versus Fennell (*2*) debate over the identity of the supposed "variant of *Aspergillus oryzae* NRRL 1988" were it not that the implications are frightening. Morse (*3*) chooses to quote paragraphs from a text by Raper and Fennell (*4*) that do nothing for his case. As every mycological taxonomist knows, diversity and variability is one thing, delimitation of species is another. Morse is essentially questioning our taxonomic expertise and our success at applying the concept of species. Raper and Fennell do not remotely suggest that a given specific entity can vary and mutate to become another recognizable species. It is hard to believe that anyone could convince himself that *A. oryzae* could become *Aspergillus parasiticus*, which Morse would have to do in order to make his case watertight. Morse expresses a wish to have the matter closed but yet maintains that the question remains open. There is only one way to close the matter—El-Hag and Morse should realize that from the published evidence the cultures they received became contaminated in their laboratory.

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13 January 1977

## Estimating Evaporation: Difficulties of Applicability in Different Environments

Idso *et al.* (*1*) have presented a method for calculating daily totals of evaporation from wet and drying bare soils. They have shown that their technique, which requires only a knowledge of daily solar radiation, surface albedo, and maximum and minimum temperatures in air (screen) and at soil surface, is consistent with bare soil data at Phoenix, Arizona. However, we are unable to accept the rationale behind these empirical relationships which, we suggest, are site-specific. Equation 1 of Idso *et al.* (*1*) assumes (i) that potential evaporation can be subdivided into two parts: that induced by solar radiation and that induced by thermal radiation; and (ii) that the solar component is quantitatively equivalent to net shortwave radiation ($S_N$). In our opinion, neither assumption can be justified.

Figure 1 of Idso *et al.* (*1*) shows clearly that net thermal (longwave) radiation ($L_N$) and evaporation ($LE$) are both energy sinks, and $L_N$ cannot therefore induce part of $LE$. The two sources of energy for positive nocturnal $LE$ are downward sensible heat transfer from air to the surface and a soil heat flux toward the surface. The combination equation is generally accepted as a valid approximation for potential evaporation $LE$ (*2*):

$$LE = \frac{s}{s+\gamma}(S_N + L_N + G_N) + \frac{\gamma}{s+\gamma} f(u)\,(e_0 - e_a) \qquad (1)$$

or alternatively

$$LE = \frac{s}{s+\gamma}(S_N + L_N + G_N) + f'(u)\,(T_{db} - T_{wb}) \qquad (2)$$

where $s$ is the slope of the saturation vapor pressure curve with respect to temperature at a characteristic air temperature $T_{db}$, $\gamma$ is the psychrometric constant, $G_N$ is the net soil heat flux, $f(u)$ and $f'(u)$ are transfer functions dependent on wind speed, $e_0$ is the saturated vapor pressure at air temperature, $e_a$ is the actual vapor pressure, and $T_{db}$ and $T_{wb}$ are the dry bulb and wet bulb temperatures of the air. In the case of daily values, $G_N$ will approach zero, and so Eq. 2 may be written:

$$LE = S_N + \frac{s}{s+\gamma}L_N - \frac{\gamma}{s+\gamma}S_N + f'(u)\,(T_{db} - T_{wb}) \qquad (3)$$

Thus in estimating 24-hour totals of $LE$, the quantity represented by the net longwave radiation function $f(L_N)$ of Idso *et al.* may be equated to the last three terms of Eq. 3. Thus $f(L_N)$ is dependent not only on $L_N$ but also on $S_N$, temperature, humidity, and wind speed.

In arid areas such as central Arizona, there is an annual cycle in the clear-day values of air temperature, wet bulb depression, $S_N$, and $L_N$. The first three of these parameters are high in summer and low in winter (with slight differences in phase), and the last parameter reaches more negative values in winter than in summer. It is therefore to be expected that in Arizona, $L_N$ could act as surrogate throughout the year for all elements with the exception of $f(u)$. Although an increase in wind speed will reduce the surface temperature ($T_s$), it will also make $L_N$ less negative, thus effectively increasing $LE$ through equation 4 of (*1*). We therefore believe that the local success of the relationship of figure 1 and equation 4 of (*1*) is chiefly related to the annual cycles in nocturnal $L_N$, $S_N$, $T_{db}$, and $(T_{db} - T_{wb})$. We agree that $L_N$ is validly approximated for clear nights by equations 3 and 4 of (*1*) and that figure 1 and equation 4 of (*1*) indicate a noncausal relationship between nocturnal values of $L_N$ and $LE$.

However, the correction of the overestimate of energy contribution from $S_N$ by extrapolation of the nocturnal evaporation expression given in equation 4 of (*1*) has no physical basis that we are aware of. Figure 2 of (*1*) indicates that it happens to work with the data of Idso *et al.* in the short term for days which cover a range of advective conditions, but this does not justify the inference with respect to mechanism.

We also question the concept of figure 3 of (*1*). If the solution of the combination equation is extended to provide an estimate of actual surface temperature, the relationship between the maximums in surface and air temperatures is a function of all the factors in Eq. 3 (*3*). At low radiation levels $T_s$ approaches $T_{wb}$, and $T_s$ rises significantly above $T_{db}$ only for a combination of high radiation levels and low evapotranspiration rates. It is very likely that the relationship shown in figure 3 of (*1*) exists for the combination of radiation, temperature, wind speed, and humidity in central Arizona. That combination is probably not atypical of various temperate and semiarid regions but is rather different in monsoonal regions characterized by strong seasonal differences in the relationships between those elements.

We therefore conclude that the techniques outlined by Idso *et al.*, although not inconsistent with the Phoenix, Arizona, data, do not have the generality inferred. Local experiments should precede the application of their relationships in other environments. In particular, in monsoonal climates the relationships are different and change seasonally.

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24 May 1976; revised 27 July 1976

Idso *et al.* (*1*) have given some theoretical justification for a method of estimating potential evaporation from bare soil which requires the knowledge only of incoming solar radiation, wet surface albedo, and temperature of the soil surface and the air. Our purposes in this comment are: (i) to show that this method does not have the theoretical basis that Idso *et al.* claim for it; (ii) to illustrate significant errors which can arise if their method of estimating mean temperatures is applied in different environments, and how these errors might be avoided; (iii) to investigate limitations in the method of estimating potential evaporation, when the rate of actual evaporation is less than the potential rate; and (iv) to comment on whether the empirical relationships employed are likely to apply in a quite different environment.

Idso *et al.* assume that, when evaporation proceeds at a potential rate, the energy equivalent of evaporation over a 24-hour period (written $\int^{24}LE$) can be partitioned into two components, the first related to net solar radiation ($S_N$) and the second to net longwave or thermal radiation ($L_N$). The solar component of evaporation ($\int^{24}LE_S$) was assumed to be equal to the daily total of net solar radiation ($\int^{day}S_N$). The thermal component of evaporation ($\int^{24}LE_T$) was based on nighttime measurements of evaporation ($\int^{night}LE \equiv \int^{night}LE_T$) and calculated values of $\int^{night}L_N$. They obtained the following correlation:

$$\int^{night}LE_T = 1.56\int^{night}L_N + 156 \qquad (1)$$

where evaporation has the units calories per square centimeter.

Idso *et al.* then assumed Eq. 1 to be valid if the duration of the energy integrals is extended from nighttime to 24 hours (that is, day and night), so that:

$$\int^{24}LE_T = 1.56\int^{24}L_N + 156 \qquad (2)$$

and hence potential evaporation:

$$E_p \equiv \int^{24}LE = \int^{day}S_N + 1.56\int^{24}L_N + 156 \qquad (3)$$

The assumption that Eq. 1 can be extended from nighttime to 24-hour period is formally incorrect. The numerical term (156 cal $cm^{-2}$) in Eq. 1 is totally dependent on the time period of integration used and should be doubled (to 312 cal $cm^{-2}$) if the time period is extended from (approximately) 12 to 24 hours. The apparent agreement between Eq. 3 and experimental data obtained by Idso *et al.*, despite this algebraically incorrect extrapolation, emphasizes the empirical character of the relationships.

Although estimates by Idso *et al.* of the thermal component of evaporation by night were positive (that is, a water loss, Eq. 1), 24-hour estimates based on Eq. 2 were all negative, that is, a water gain, which is nonsense. Thus Eq. 2 should be regarded as an empirical relationship, apparently correcting for an overestimate of potential evaporation based on net solar radiation, but without theoretical justification or physical meaning.

Net longwave radiation ($L_N$) is the difference between outgoing radiation, calculated from soil surface temperature ($T_S$), and incoming radiation, calculated from air temperature ($T_A$) as explained by Idso *et al.* These authors used the average of maximum and minimum values of $T_S$ and $T_A$ as "representative" values in computing 24-hour values of $L_N$. This procedure can lead to quite significant errors in the estimation of mean surface temperature, as illustrated in Table 1 for 2 days after rainfall on a clay-loam soil in a dry monsoonal climate. As expected, the error in the simple procedure of Idso *et al.* increases with the amplitude of the temperature swing.

The data in Table 2 flow from the consequences of the errors shown in Table 1 and confirm that estimates of $\int L_N^{24}$ based on mean temperatures are very close to "true" values (with $L_N$ calculated at 15-minute intervals). Table 2 also shows that the values of potential evaporation ($E_p$) based on more accurate estimates of mean temperature are in better agreement with field evaporation measured by loss of water in the top 12 cm of soil. Thus, in applying the methods of Idso *et al.* to other environments, a more accurate method of estimating mean daily surface temperature may be required.

Later in their report, Idso *et al.* demonstrated that there is an approximate relationship between the ratio of actual to potential evaporation ($E_a/E_p$) and temper-

Table 1. Error arising from the use of "representative" instead of mean temperatures.

| Date (1974) | Soil surface temperature (°C) | | | | | Air temperature (°C) | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | $T_{max}$ | $T_{min}$ | Representative temperature* | Mean temperature† | Error | $T_{max}$ | $T_{min}$ | Representative temperature | Mean temperature† | Error |
| 3 Dec. | 39.0 | 25.0 | 32.0 | 30.6 | 1.4 | 30.6 | 23.3 | 26.9 | 26.9 | 0.0 |
| 28 Dec. | 54.0 | 25.0 | 39.5 | 33.5 | 6.0 | 34.4 | 23.9 | 29.2 | 28.4 | 0.8 |

*Representative temperature = $(T_{max} + T_{min})/2$. †Mean temperature = $(\sum_{1}^{48} T/48)$, that is, the mean of temperatures measured at half-hour intervals.

Table 2. The consequence of the use of "representative" and true mean temperatures on the calculation of $\int^{24}L_N$ and $\int^{24}LE$ (or $E_p$).

| Date (1974) | $S_N$ (cal $cm^{-2}$) | $\int^{24}L_N$ (cal $cm^{-2}$) | | | $\int^{24}LE$ calculated (cal $cm^{-2}$) using | | | $\int^{24}LE$ measured in field† (cal $cm^{-2}$) |
|---|---|---|---|---|---|---|---|---|
| | | Calculated using: Representative temperature | Calculated using: Mean temperature* | True value | Representative temperature | Mean temperature* | True value | |
| 3 Dec. | 243 | −206 | −187 | −188 | 78 | 107 | 106 | 118 ± 24 |
| 28 Dec. | 514 | −273 | −202 | −207 | 244 | 355 | 347 | 313 ± 90 |

*Calculated as shown in Table 1. †Mean and range of water loss from the top 12 cm of soil (three replicates).

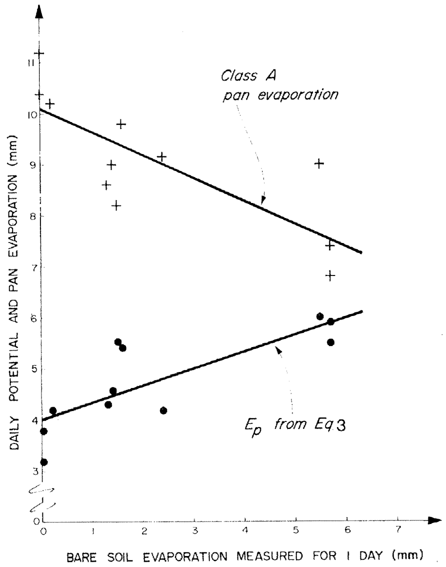


Fig. 1. For days with similar solar radiation (630 to 660 cal $cm^{-2}$), the relationship between bare soil evaporation measured over 24 hours $E_a$ and (i) potential evaporation $E_p$ (●), calculated according to Eq. 3 from measured solar radiation, surface temperature, and air temperature ($r$ = 0.804) and (ii) class A pan evaporation (+) ($r$ = −0.785).

ature parameters. In developing this relationship, Idso *et al.* used values of $E_p$ measured at the beginning of each series of drying experiments. This method, acceptable under the consistent desert environment of the experiments of Idso *et al.*, is not generally applicable in situations where large day-to-day variations in solar radiation can occur. Furthermore, if it is necessary to measure $E_p$, a major objective of their report is not accomplished—namely, the method of estimating evaporation from bare soil be adaptable to remote sensing.

Nor can the problem of estimating $E_p$ for a drying soil be overcome with the use of Eq. 3, since with this method the estimate of $E_p$ is not independent of $E_a$. When $E_a$ is significantly less than $E_p$, both $T_S$ and $T_A$ are affected. The more $E_p$ exceeds $E_a$, the more $T_S$ exceeds $T_A$ and, thus, the more negative is $L_N$ and the lower is $E_p$ calculated by Eq. 3.

Figure 1 illustrates how large the effect of actual evaporation on the calculation of $E_p$ can be if Eq. 3 is used. Experimental data are for days with similar solar radiation at Katherine, Northern Territory, Australia (14°28′S 132°19′E; elevation, 108 m). Evaporation was measured in terms of the loss of water from the top 12 cm of soil (a clay loam). Pan evaporation was the measured water loss from a water-filled aboveground tank (a class A evaporimeter).

Figure 1 shows that, although pan evaporation decreased with higher actual evaporation (probably as a result of lower vapor pressure gradients), calculated $E_p$ increased as a result of lower $T_S$ and hence less negative $L_N$. The agreement between calculated and measured potential evaporation ($E_a \simeq 6$ mm, Fig. 1) supports the generality of the formula of Idso *et al.* (Eq. 3).

In a different environment (Katherine, Northern Territory, Australia), we have found that the methods of Idso *et al.* appear to predict potential but not actual soil drying. A possible explanation is that the empirical expression for $E_p$ (Eq. 3) is likely to depend on the thermal conductivity characteristics of soil, which vary only modestly with soil type; in contrast, the empirical relationship of $(E_a/E_p)$ to $T_S$ and $T_A$ will depend strongly on hydraulic conductivity characteristics, which vary greatly between soil types.


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23 August 1976; revised 27 October 1976


One common theme running through both of the technical comments is that we did not have a theoretical basis for our approach to calculating potential evaporation. It is absolutely correct that our approach was purely empirical. We invoked no particular mechanism and inferred no specific causal relationships. The modest amount of symbol manipulation we engaged in was merely to develop the operational context for application of our calculation procedure. Thus, we expected no more (and no less) from our approach than that it work.

We are well aware of the equations cited by Kalma *et al.* and of the fact that $f(L_N)$ depends on a number of environmental factors. It was precisely for this reason that we developed our empirical approach. Our goal was to develop a simple way to estimate evaporation over large areas with a minimal number of ground-based measurements. Thus, the only significant question for the application field with which we were concerned is, "Does it work?"

With respect to this question, Kalma *et al.* offer no data, only opinions. With one of these we concur, namely, that local experiments should precede the utilization of our evaporation estimation technique in other environments. We indeed hoped that our report (*1*) would serve to instigate such studies. However, we cannot agree with their statement that "in monsoonal climates the relationships are different and change seasonally" without supporting data. This is particularly true in view of the fact that the environment in which we worked *is* characterized by a monsoonal climate (*2*), where in early July the atmospheric precipitable water content triples almost overnight as Arizona suddenly becomes immersed in moist air from both the Gulf of Mexico at high levels and the Gulf of California at low levels.

McKeon and Rose, on the other hand, do present data relative to the workability of our approach. Their figure 1, as they say, "supports the generality of the formula of Idso *et al.* (Eq. 3)." (This is the equation we originally presented with an empirical coefficient that they claim is erroneous.) They also present data to indicate that a more accurate method of estimating mean daily surface temperature may be required. If there is a better way of doing this, we would by all means urge that it be adopted.

We would like to make one further statement about the generality of our approach to estimating potential evaporation, since it is our inference of generality that seems most to bother the authors of both technical comments. We have now completed additional studies of potential evaporation from grass, field beans, and alfalfa at Davis and Brawley, California, as well as wheat and water at Phoenix, Arizona. In all cases, we found that not only did a similar approach work equally as well for these surfaces as for the bare soil we studied originally, but that the same equation with the same empirical coefficients accurately predicted the lysimetrically measured potential evaporation (*3*).


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19 April 1977