EVAPORATION: AN INTRODUCTORY SURVEY [1]

H. L. PENMAN

Physics Department, Rothamsted Experimental Station, Harpenden, Herts

INTRODUCTION

Evaporation of water plays an important part in most human activity designed to satisfy basic needs. While in the technology that produces clothing and shelter evaporation processes are under some measure of control, in the open-air operations that lead to production of food and drink evaporation is usually beyond control, and amounts and rates are often very difficult to estimate. Hence our problem. Before passing on to the "food" aspect, we might note that in the "drink" aspect the water engineer regards evaporation of water as a loss, whether it occurs from reservoirs, from streams, from bare soil or from land carrying crops. Water supply and agriculture must often be in conflict. The farmer's attitude varies from time to time. While waiting to start spring cultivations he will regard evaporation as an essential preliminary to seed-bed preparation; later, the evaporation from bare soil between the growing plants will tend to be regarded as a waste of water that would be more profitably used in passing through the plant. At all times he will regard transpiration as helpful, because it is an essential condition of plant growth.

Transpiration must be our main concern too, but there is secondary interest in evaporation from bare soil and from open water. Although of minor technical interest in the present context, open water evaporation must be considered because many physical ideas about evaporation are most easily checked on open water. As meteorological or agricultural physicists it might be of interest to learn what some of our predecessors thought of our problem. Here is SYMONS in 1867: "evaporation is the most desperate branch of the desperate science of meteorology", or MALUSCHITSKI in 1900: "no correlation can be established between the evaporation from a water surface and that for a cultivated soil, and still less in the case of a soil covered with plants". CLEVELAND ABBE in 1905 was scornful: "of course (we) need to know the loss of water by evaporation but in nature this is so much mixed up with seepage, leakage and consumption by . . . plants that our meteorological data are of comparatively little importance".

It would be easy to join these and other distinguished workers of the past

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[1]) It was a great honour to be given the privilege of presenting this survey to an audience that included so many who could easily have changed places with me. Apart from relatively unimportant differences in arrangement, style and emphasis, their surveys would in great measure have covered much the same ground. Parts of the subject are still controversial and personal opinions intrude, so that no two surveys could be in entire agreement. The informality of the conference invited the expression of such personal opinions, and I have assumed that what was acceptable in my talk, will, in a reduced form, be acceptable in the printed version. Hence parts of my survey remain in the first person singular. I apologise to those who may be irritated by this departure from the impersonal objectivity that ought to characterise a scientific publication.

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in drawing up a list of difficulties, of the many factors that might affect evaporation rates out-of-doors. Easy, but not very helpful. For the moment difficulties will be ignored in an attempt to see how far present-day knowledge of physics can be used to establish some fundamental ideas; then we can consider – if needed – the departure of the real systems from the ideals postulated. In doing so nothing new will be said, nor will there be detailed discussion of any of the topics. That will come later, and is the main reason for the meeting.

The most convenient way into the survey is by the most respectable route into any physical discussion – via ARISTOTLE. MENGEL (1936) quotes ARISTOTLE as asking whether sun or wind is the most important factor in evaporation, and answering in favour of the wind because it carries the vapour away. Here is the source of controversy that runs right through the history of the subject, the clash, or rather the apparent clash, between *'turbulent transfer'* and *'energy balance'* approaches to the problem. Let us see first what the modern disciples of ARISTOTLE have to say.

TURBULENT TRANSPORT

In normal conditions out-of-doors the air movement is characterised by a bulk movement horizontally with a mean direction of flow that tends to remain constant over periods longer than those to be considered now. The velocity in this mean direction is usually symbolised by u and over a period the mean sideways component, $\overline{v}$, and the mean vertical component $\overline{w}$ will be zero. At any given time and place however, the turbulence in the moving air can be expressed through eddy velocities u', v' and w', representing instantaneous departures from these means. The vertical eddy velocities are particularly important for their effect in vertical transport of momentum, characterised in the end by the establishment of a profile of horizontal velocity in which there is zero value of u at the surface, a rapid increase with height, z, over a short height range, and then a more gradual increase.

Associated with this eddy transport of momentum are similar transports of other atmospheric characteristics, two of which must be considered. First there is transfer of sensible heat; second there is transfer of water vapour. The immediate concern is with the second, but it is undesirable to consider the eddy flux of water vapour by itself, for two good reasons. First, it is essential that this problem be seen as a whole, realising that the vapour transport is only one aspect of the problem of the rôle of weather in transport phenomena; second, the limited view is impracticable, for vapour transport cannot be discussed in isolation. Sooner or later the others will intrude.

Taking them in now, we can write down three formal transport equations:

$$\text{Momentum} \quad \tau = \varrho \, K_M \frac{\delta u}{\delta z}$$

$$\text{Heat} \quad K = - \varrho \; c \; K_H \frac{\delta T}{\delta z}$$

$$\text{Vapour} \quad E = - \, K_V \frac{\delta \chi}{\delta z}$$

where the density and specific heat have been introduced so that all the transport constants, K_M, K_H, K_V have the same unit of cm^2 sec^{-1}.

In the early stages of work on turbulence it was argued that the transport constants should be the same whatever the entity transferred by turbulence. It would simplify our task considerably if this were so, and the argument will arise at two stages in this survey. To justify the form of the equations and yet to allow for possible inequality, write

$$K_M \overset{?}{=} K_H \overset{?}{=} K_V$$

Relations between transport constants

For the moment it will be assumed that in the evaporation equation there is no difficulty in measuring the gradient of vapour density with height: so the only real problem is to find a value for K_V in terms of measurable physical quantities. This cannot be done on any existing theoretical basis, but by a mixture of empiricism and theory the momentum transport coefficient, K_M, can be expressed in terms of measurable quantities. Hence, *if* $K_V = K_M$ then the evaporation equation can be solved in practice. Later it will be seen that another approach to the evaporation problem depends on the identity of K_H and K_V. Much fine experimental skill has gone into efforts to measure these quantities, and it may be useful to summarize here two of the many statements of results. RIDER (1954) has given individual values leading to mean ratios:

$$K_H/K_V = 1.14 \pm 0.06;\ K_H/K_M = 1.48 \pm 0.27;\ K_V/K_M = 1.23 \pm 0.17.$$

To these can be added SWINBANK's summary of a recent paper (1955). "The data show that the heat transfer coefficient consistently exceeds that for momentum in lapse conditions, the effect becoming more pronounced with increasing lapse rate and height above ground. Indirect evidence is adduced to show that this is true also when heat transfer is compared with that of water vapour, while a difference between heat and momentum transfer in the opposite sense is indicated under inversion conditions." There is no obvious conflict between RIDER and SWINBANK. For future use, note in passing that the departure from equality in the constants increases with the height above ground at which measurements are made.

Direct technique of SWINBANK

The problem of getting experimental values of the vapour transport constant is one of measuring the vapour density gradient and the evaporation rate simultaneously. Before discussing the various possible ways of measuring evaporation directly, it is worth noting the fundamentally simple technique suggested by SWINBANK (1951). If at a particular reference level the water vapour pressure is e and the vapour density is χ (related through the standard gas equation) then the eddy motion that gives rise to fluctuations w' in the vertical air velocity will also give rise to fluctuations in vapour content, expxressible as e' or χ'. At the chosen reference point, at any given time, the upward flux of water vapour is obviously $w\chi$, and over a period the mean flux is $E = \overline{w\chi}$. SWINBANK shows by straightforward mathematics that this reduces to

$$E = (0.622/p)\ \overline{(\varrho w')e'} \qquad (1)$$

where p is the atmospheric pressure, and χ the density of the air.

At its simplest the technical problem is one of measuring simultaneous values of upward eddy velocity and fluctuation in vapour pressure. SWINBANK (1951) has described his technique for getting a continuous photographic record of these fluctuations (and of air temperature too) over periods of five minutes. Analysis at one second intervals involves 600 measurements on the record, the computation of 300 products and then the addition and division to get the mean value of the products. Similar measurements and computations are needed for the heat transport. The labour is great, but is now greatly reduced by use of a machine (TAYLOR and WEBB, 1955).

As far as is known this technique has not been used elsewhere for estimating evaporation, and there is not much direct experimental evidence to show that it gives the right answer. Repetition for the heat transfer measurement has been attempted at Kew, and though the first report by LANDER and ROBINSON (1952) failed to get agreement between the SWINBANK type of estimate and that from a method based on the BOWEN ratio, the source of the discrepancy has since been found.

It seems, then, that here is an excellent research tool, restricted at present to periods of the order of a few minutes, but having the immensely important feature of being independent of the nature of the surface over which the measurements are made, and independent of measurements of, or inferences about the physical properties of the surface.

Transport theory, assuming identity in constants

The next group of studies of turbulent transfer is characterised by the general assumption of identity between the transport coefficient for momentum and that for vapour transfer. The literature is very voluminous, but fortunately there is no need to make a detailed survey. To do so would repeat much that is in SUTTON's book on "Micrometeorology" (1953), or in the preliminary paper (1950) and the first technical report (1952) from Lake Hefner. What follows should be sufficient as an introduction to later discussions.

Before doing so, it may be helpful to make a distinction that has been found useful. The conditions under which evaporation takes place out-of-doors range between two extremes. Perhaps better names could be found for them, but they can be called "mid-ocean" and "mid-desert" conditions. The first is almost self-explanatory: it is the state in which evaporation takes place from a limited area in the midst of an infinite sheet of the same kind of surface. A square kilometre in mid-ocean satisfies this condition, but it may be equally well satisfied by a field in the midst of an extended area of similar vegetation. At the other extreme, "mid-desert" conditions are those in which a reservoir or other evaporating surface is surrounded by an infinite plane from which no evaporation can take place. An evaporation pan in the Sahara satisfies this condition; an irrigation area in an arid area will come very close to it. For the purpose of formal analysis the distinction is necessary because at the "mid-ocean" extreme the size of the area studied is of no importance, but under "mid-desert" conditions the size may be very important. Some of the theoretical formulae therefore include the dimensions of the area; others do not, because they are not needed.

The treatment of turbulent diffusion associated with the names of THORNTHWAITE & HOLZMAN (1939) and of PASQUILL (1950) come into this "mid-ocean" class. Early work on turbulence used mixing length theories with surprising success, in attempts to express the momentum transport coefficient and the wind velocity profile in terms of measurable quantities: e.g. it was possible to deduce the logarithmic variation of wind speed with height. Later work, however, has preferred to take the logarithmic profile as a fact of experience, to assume that surface properties can be expressed through a roughness constant z_0 and/or a "displacement of zero plane" d (to be defined more fully later), and to assume that τ in the momentum equation is invariant with height, i.e. is equal to the surface value τ_0. The mathematical argument is simple and familiar: all that is needed here is to list the definitions or assumptions, and the deductions. They are

Assumptions

$$\frac{\tau}{\varrho} = \frac{\tau_0}{\varrho} = K_M \frac{\delta u}{\delta z}$$

$$u = \frac{1}{k}\sqrt{\frac{\tau_0}{\varrho}}\ \ln \frac{z}{z_0}$$

$$E = -K_V \frac{\delta \chi}{\delta z}$$

$$K_V = K_M$$

Deductions

$$K_M = uk^2z/\ln(z/z_0)$$

$$E = \frac{k^2(\chi_1 - \chi_2)(u_2 - u_1)}{\ln z_2/z_1)^2}$$

$$E = \frac{\varrho k^2(q_1 - q_2)(u_2 - u_1)}{(\ln z_2/z_1)^2}$$

where χ is in gm./c.c. air
q „ „ gm./gm. air.

The heights z_1 and z_2 are usually measured from the ground surface, but when there is a surface cover of comparable height account must be taken of the effective displacement of the zero plane. This displacement is represented by d so that the evaporation equation as used by RIDER (1954), for example, is

$$E = \frac{\varrho k^2(q_1 - q_2)(u_2 - u_1)}{\left(\ln \dfrac{z_2 - d}{z_1 - d}\right)^2} \qquad (2)$$

Many have used this equation, including some of those already named and also Dutch workers in the Rottegatspolder about whose activity too little is known. No detailed discussion of the use of the equation over land surfaces will be attempted; instead, here are a few brief comments, the first personal, the rest general and probably universally acceptable. The personal comment is that I feel that the equation needs more direct field checks than it has had so far. In saying this there is no question of indulging in any carping criticism of work already done; I merely wish to use this as a place to emphasize what is certain to be the main problem in subsequent discussions. Soil physics is perhaps 50 years old as an organised directed branch of science; in that period its main pre-occupation has always been with soil water; and yet we cannot answer the first and most important question about soil water, namely, how much of it there is in the soil at a given place and time.

As general comment it is worth noting:

(i) That the validity of the equation depends on the constancy of shearing stress at all heights, a topic recently briefly discussed by DEACON (1954). He concludes: "While this work has given no evidence for any systematic departure of the variation of shearing stress with height in the lowest 30 metres from that expected theoretically, individual pairs of observations can show large departures, even under neutral conditions and recording periods as long as 10 minutes".

(ii) For several reasons, including the validity of $\tau = \tau_0$, the closer to the surface observations are made the more reliable the equation becomes.

(iii) The equation applies to an adiabatic or neutral state of the lower atmosphere.

(iv) The method is admirable for getting average evaporation rates over periods of 10 minutes and is extremely useful for the study of changes of rate during days on which atmospheric stability is such as to justify use of the equation.

Finally (v). The equation can be used for measurements over any kind of surface, but is not completely independent of surface properties.

Test on open water, using surface parameters

Open water offers many advantages for testing evaporation equations: the position of the surface is known, its physical properties are known, and it is reproducible. If the surface is taken as the lower boundary of the test range, i.e. $z_1 = 0$, then $u_1 = 0$ and χ_1 is the saturation vapour density at the surface temperature of the water. Hence at Lake Hefner in addition to testing the THORNTHWAITE and HOLZMAN equation, among many others tested was one given by SVERDRUP (1946), effectively:

$$E = \frac{\varrho k^2 (q_0 - q_8)\, u_8}{(\ln 800/z_0)^2} \tag{3}$$

using wind and vapour measurements at 8 metres above the lake.

The comparisons with observed daily evaporation showed that estimates based on observations at 2 and 8 metres had an enormous scatter, but except at high values of E the mean was about right. On the other hand, estimates based on observations at 0 and 8 metres showed much less scatter but the computed evaporation was about twice the observed throughout the range. As a purely personal comment, I consider that the surface temperatures were consistently overestimated and hence q_0 and $q_0 - q_8$ were too big.

Effectively these computations treated the lake as being of infinite extent. In the test of SUTTON's (1949) equation the finite size of the lake was taken into account. The equation, expressing the average evaporation rate over the whole area, is

$$E = C'\, u^{\frac{2-n}{2+n}}\, r^{\frac{-n}{2+n}} (e_0 - e_i) \tag{4}$$

where C' is a very complex algebraic expression mainly involving the stability factor n, other true constants for a fixed observational level, but also a complex function of $u_x z_0$ where $u^2{}_x = \tau_0 / \varrho$, and z_0 is the roughness of the surface.

n is the value used in the assumed power law of variation of wind with height, appearing in the equation

$$u_1/u_2 = (z_1/z_2)^{n/2-n}$$

a normal value of n being $n = 0.25$

r is the effective radius of the lake

e_0 is the surface vapour pressure, assumed constant over the lake

e_i is the vapour pressure of the air upwind of the lake and assumed constant with height.

u is the wind velocity at height z and assumed constant over the lake.

The test of SUTTON's equation was very successful (a result that has produced more embarrassment than delight in its author), but it should be noted that the assumption of constant e_o and constant u were not realised over the lake. Further, the effect of a wind-dependent term in the constant C' has the effect of making the power of u (normally about 7/9) greater than unity; and if my personal opinion is correct then the values of e_o used are too great. There is then some justification for feeling that the agreement between observation and formula may have a fortuitous element in it – a feeling shared by Sir GRAHAM SUTTON himself.

There have been hints that further experiments on Lake Mead have not confirmed the Lake Hefner results. The Lake Mead report has not yet been published, and prudence suggests that we await this report before attempting a final judgment about the validity of any of these theoretical formulae involving surface values of vapour pressure.

Evaporation pans

SUTTON's formula does lead however to the root of a most important practical problem – how far can small surfaces be used as guides to what will happen over large surfaces? In a more direct form: Is an evaporation pan any use?

There are many empirical formulae for pans (e.g. ROHWER, 1931, BRAAK, 1936, PENMAN, 1948), all different, but commonly in the form

$$E = f(u)\,(e_o - e_d) \tag{5}$$

where e_o and e_d are the vapour pressures at the surface of the water and in the air above (usually at screen height). The vapour pressure difference term is theoretically sound, and there is ample experimental evidence to confirm it. The difficulty is in $f(u)$. Obviously, for the mid-desert conditions of SUTTON's equation, the dimensions of the tank should appear in $f(u)$: as a numerical guide, the ratio of the dimensional factors for Lake Hefner and a standard BPI tank (6 feet diameter) with which comparisons were made, is about 2.5 for $n = \frac{1}{4}$. If, following standard engineer's practice, we introduce the concept of a pan factor, p, defined by

$$E_{lake} = p \cdot E_{pan} \tag{6}$$

then, other things being equal, the BPI pan factor for dimensional reasons should be $p = 0.4$. In fact, on average it is nothing like as small as this; the other things are not equal. At the moment ideas about pan factors are in a state of flux, and it is not easy to present a reasoned case for accepting or rejecting any particular value, or particular expression for a pan factor. A few generalisations can, however, be made with safety. The main reason for need of a pan factor is aerodynamic, but an almost equally strong reason lies in the differences in energy exchanges between the water and the atmosphere. This imposes a marked seasonal effect on the pan factor, shown at Lake Hefner by a range of $p = 1.56$ in November 1950 to $p = 0.22$ in February 1951. The energetic aspect can be largely eliminated by redefining the pan factor and setting the open water evaporation as say

$$E_o = \lambda\, f(u)\,(e_o - e_d) \tag{7}$$

in which the energy aspect of the evaporation would be taken care of by the difference in e_0 for a small pan and a large lake. The quantity λ would then be primarily dependent on dimensions, provided that $f(u)$ is independent of size. Lake Hefner results are a little confusing here. It seems clear that wind velocity was *not* the same over the lake as it was at the shore, and it is not possible to indulge in much profitable speculation about the theoretical form of $f(u)$. For the moment it may be sufficient to quote the empirical results to indicate the kind of data that some day must be brought inside a unified theory.

Pan factor (BPI ; 6′ diam.)	$\bar{p}$	$= 0.91$
Pan equation	E_P	$= 0.253 \ (e_o - e_d)\ (1 + u_0/63)$
Lake equation	E_L	$= 0.051 \ (e_o - e_d)\ (1 + u_2/18)$
or		$= 0.0030 \ (e_o - e_d)\ u_2$

where E_P and E_L are in mm/day, $e_o - e_d$ in mm Hg and u_0 and u_2 are in miles per day.

In my own work I use an equation of this type, based on experiments with a green vegetation surround, which as an evaporating surface, might be expected to make the test area more nearly mid-ocean than mid-desert. My equation has not been substantiated at Lake Hefner, and though I suspect e_0 for the lake is too great, the possible error cannot be big enough to account for the difference. So for a year now (Penman, 1954) I have been using a modified equation, effectively a compromise between my own experience (which I have no reason for completely abandoning) and Lake Hefner experience (which I cannot ignore). My old and new expressions are :

Old $\quad E_o = 0.35\ (e_o - e_d)\ (1 + u_2/100) \qquad$ (8a)

New $\quad E_o = 0.35\ (e_o - e_d)\ (0.5 + u_2/100) \qquad$ (8b)

At 100 m.p.d. at 2 metres (and, for convenience, say 63 m.p.d. at pan level) the numerical values of $E/\Delta e$ that emerge are

	$E_p/\Delta e$	$= 0.50$
	$E_L/\Delta e$	$= 0.34$ or 0.30
Old	$E_o/\Delta e$	$= 0.70$
New	$E_o/\Delta e$	$= 0.52$

Making no attempt at a re-capitulation of this section, it is probably sufficient to say that while there are still physical problems in the aerodynamics of evaporation, the main feeling at present ought to be one of appreciation of the hard and skilful work that has achieved so much already.

Energy balance

The fundamental basis of the energy balance approach is unchallenged : the challenge is to our ability to measure or estimate all the quantities needed to exploit the principle of the conservation of energy. Evaporation is a change of state demanding a supply of energy as heat of vaporization ; the problem is to measure or assess all other sources and sinks for energy, to leave evaporation as the only unknown.

Irrespective of kind of surface, it is possible to work in terms of a heat budget (H), which, as income, can be regarded as a radiation balance, and, as expenditure, can be regarded as a non-radiant heat balance. As income

$$H = R_I\,(1 - r) - R_B \qquad (9)$$

where R_I is short-wave radiation reaching the surface, r is the reflexion coefficient and R_B is the net long-wave radiation exchange between surface and atmosphere.

As expenditure

$$H = E + K + (S + C + M) \qquad (10)$$

where E is the energy of evaporation (latent heat), K is the heat transfer to the air (sensible heat), S is the heat transfer to the absorbing sub-surface (soil or water) (sensible heat), C is heat conducted in or out of the sub-surface system (sensible heat), and M is energy used in melting of snow (latent heat). There will be places, times and conditions in which S, C, or M may be important. They will be ignored here because the main problem in the energy balance approach is to handle the equation

$$H = E + K$$

Taking the parameters in turn:

R_I. This can be measured with reasonable ease and accuracy within a few per cent. Unfortunately the number of sites at which it is measured is rather small, and, as a result, for practical purposes, it is often necessary to fall back on an empirical relation of the form

$$R_I / R_A = a + bn/N \qquad (11)$$

where R_A is the theoretical maximum radiation that would be received if there were no atmosphere. Tables of R_A by latitude and season are available. The 'constants' a and b are empirical and change somewhat with latitude (BLACK, BONYTHON and PRESCOTT, 1954) though I have found recently that the values for south-east England also apply on the Gold Coast of West Africa where BLACK, BONYTHON and PRESCOTT would suggest significantly different values. The ratio n/N is the actual duration of bright sunshine as a fraction of the maximum possible for a cloudless sky.

r. The reflexion factor will depend on the kind of surface and on elevation of the sun. As orders of magnitude, $r \simeq 0.05$ for a clear open water surface, and $r \simeq 0.20$ for fresh green vegetation.

R_B. This represents the net long-wave radiation leaving the surface. Adequate discussion would need too much detail, so, somewhat too simply, it can be stated that the surface appears to behave as a black-body at mean air temperature, the radiation loss being reduced by atmospheric water vapour and cloud. Assuming a complementary relation between mean cloudiness, and mean sunshine factor, n/N, then the net back radiation can be expressed by an equation of the following type:

$$R_B = \sigma T_a{}^4 \, (0.56 - 0.09\sqrt{e_d}) \, (0.10 + 0.90\, n/N) \qquad (12)$$

Everyone would like something better, but even this crude expression is proving very useful.

At this stage it is worth noting: (i) that as presented here the heat budget is independent of the kind of surface except for the reflexion coefficient; and (ii) that H can be estimated from standard weather data.

As expenditure, the problem is the sharing of H between evaporation and sensible heat transfer. *For an open water surface* equation 5 holds, i.e.

$$E_o = f(u) \, (e_o - e_d)$$

Assuming identity in the heat transfer and vapour transfer coefficients then

$$K_o = \gamma \, f(u) \, (T_o - T_a)$$

where γ is a constant to keep units consistent and T_o and T_a are surface and air temperatures. Hence BOWEN's ratio, β, is

$$\beta = K_o / E_o = \gamma (T_o - T_a) / (e_o - e_d) \qquad (13)$$

identifying γ as the constant of the wet- and dry-bulb psychrometer equation. Then, with surface and air parameters measured,

$$E_o = H_o / (1 + \beta). \qquad (14)$$

Obviously if $\beta \longrightarrow -1$ it becomes essential to know its value with great accuracy, which is impossible if there is inaccuracy in the assumed identity of K_H and K_V in the aerodynamic transport equations. So even those who have a personal bias in favour of the energy balance approach must realise that the key to success lies with those who favour the aerodynamic approach.

The whole problem is very thoroughly discussed in the first Lake Hefner report, but all that is needed now is one short quotation from p. 109 of the report. "The BOWEN ratio appears to be sufficiently accurate for computing energy utilized by evaporation for most conditions." In my own attempts to make evaporation estimates of agricultural importance, checks have shown that the values of β, being used implicitly, are of the order of 0.10.

The energy balance method can thus give measures of open water evaporation if adequate estimates of H can be made, if BOWEN's ratio can be accepted, and if the necessary surface and air parameters can be measured. American faith in the method is such that in the Lake Mead experiments the energy budget method is being regarded as the *direct* method of measuring lake evaporation: there is no water budget as at Lake Hefner.

COMBINED AERODYNAMIC AND ENERGY BALANCE ESTIMATE

The success of the energy balance estimate depends on measurement of surface temperature, a very difficult measurement to make, and one rarely recorded anyway. Similarly, in some of the aerodynamic equations the surface vapour pressure is needed, and for open water this is known if the surface temperature is known. Setting down the two kinds of equations in their shortest form, with surface temperature implicit as a variable in both,

$$E_o = H_o / (1 + \beta) \qquad (14)$$

$$E_o = f(u)\ (e_o - e_d) \qquad (5)$$

These are two simultaneous equations in E_o and T_o (or e_o) and can be solved for E_o. The algebra is simple, starting from the equation $H_o = E_o + K_o$, and introducing a new function $\Delta = de/dT$, the slope of the saturation vapour pressure curve at temperature T. Thus, for small vapour pressure differences and for small β,

$$\begin{aligned} H &= E_o + K_o \\ &= E_o + \gamma\, f(u)\ (T_o - T_a) \\ &= E_o + \frac{\gamma}{\Delta}\ f(u)\ (e_o - e_a) \\ &= E_o + \frac{\gamma}{\Delta}\ f(u)\ [(e_o - e_d) - (e_a - e_d)] \\ &= E_o + \frac{\gamma}{\Delta}\ E_o - \frac{\gamma}{\Delta}\ E_a \end{aligned}$$

where $E_a = f(u)\ (e_a - e_d)$.

Then

$$E_0 = \frac{\frac{\Delta}{\gamma} H_0 + E_a}{\frac{\Delta}{\gamma} + 1} \quad \text{(Penman, 1948)} \qquad (15)$$

Note that $e_a - e_d$ is the saturation deficit of the air at mean air temperature, i.e. E_a is a measure of the drying power of the air. It might be noted in passing that Δ/γ is effectively a weighting factor in assessing the relative importance of available energy (H) and drying power of the air (E_a), and that if it ever becomes clear that a better average value of K_H/K_V can be adopted, this new value can be incorporated in Δ/γ. For the important part of the agricultural year in N.W. Europe the value of Δ/γ is about 2.0 and the value of H is usually about twice or three times that of E_a. Hence, fortuitously perhaps – but we must exploit these happy accidents – the exact form of E_a (i.e. of E_0 in the aerodynamic equation) is not of critical importance. It is possible to have 100% overestimate in E_a leading to only 10% overestimate in E_0.

The expression for E_0 is obviously too simple to be true for all conditions, but it is adequate for many. The observational data needed are those obtainable at ordinary weather stations: duration of bright sunshine, mean air temperature, mean air humidity, and wind speed.

As an indication of what the equation will do under the best conditions Table 1 gives a few monthly values (and annual totals) for Lake Hefner, calculated from the new form for E_0 and using $a = 0.18$ and $b = 0.55$, in the expression for R_I. The 'corrected' values make allowance for the changes in heat storage, data for which appear in the first report.

Table 1. E_0 for Lake Hefner (inches).

Month	Observed	Calculated	
		Uncorr.	Corrected
Aug. 1950	6.8	7.4	7.8
Nov. „	6.0	2.4	5.7
Feb. 1951	0.4	1.9	1.0
May „	4.4	6.1	4.0
Aug.–July	54.9	57.5	56.6

It might be noted that Ferguson (1952) has given a similar combined estimate starting from a heat budget equation, and an empirical equation for sensible heat transfer.

Biological aspects

So far there has been little reference to vegetation, though it should be noted again that the Swinbank and Pasquill methods of measuring evaporation will apply equally well to water and land surfaces. The broad concepts already outlined for evaporation from open water will apply to evaporation from other surfaces, with additions needed because two new factors enter – soil and plant. It is still possible to treat the problem as a physical problem, but because of a continuing ignorance of important physical facts there is a need for speculation, assumptions and, at times, guesses.

In our own work at Rothamsted, and in that of Thornthwaite at Seabrook in New Jersey, there has been great use of the concept of "potential transpiration" – often unnecessarily expanded to "potential evapo-transpiration". My usual symbol for this is E_T, which can satisfy everybody, though it began as meaning evaporation from turf.

The idea is familiar enough to most agricultural physicists so the definition can be brief. It is *the amount of water transpired in unit time by a short green crop, completely shading the ground, of uniform height and never short of water.*

One of the most important aspects of the idea is quickly seen from an extension of the heat balance argument. For a cropped surface the heat budget as income is

$$H_T = R_I \ (1 - r_T) - R_B$$

differing from that for open water only in the reflexion factor, r_T.

As expenditure, again ignoring changes in heat content of the soil,

$$H_T = E_T + K_T$$

where K_T is the sensible heat transfer to the air from the cropped surface. In the income the only important plant factor is the reflexion coefficient; the remainder are purely meteorological. In the expenditure equation, if K_T is a small part of H_T, or is an approximately constant fraction of H_T, then conditions that lead to constant H_T will also lead to approximately constant E_T Hence two important broad generalisations can be made:

1 For complete crop covers of different plants having about the same colour, i.e. the same reflexion coefficient, the potential transpiration rate is the same, irrespective of plant or soil type.

2 This potential transpiration rate is determined by prevailing weather.

It is obvious that very close scrutiny of the physical argument is needed before either of these conclusions can be accepted as physically sound; and that, even if convinced, most physicists would ask for experimental confirmation before accepting them as trustworthy in agronomy. So, at this stage, though they mean more to me, I merely offer them here as working hypotheses, with the assurance that there is direct and circumstantial evidence to support both.

The topics for immediate discussion are now clear. How can the second proposition be exploited to get an estimate of potential transpiration from weather data? What new physical or biological concepts are needed to account for any difference between potential transpiration and the evaporation that would take place from an open water surface exposed to the same weather? What happens when soil water becomes a limiting factor? What happens in orchards or widely separated row crops? How far is it necessary to maintain transpiration at the potential rate in order to ensure maximum growth of the plants?

To the first question it is probably sufficient to give only two answers – Thornthwaite's (1948) and our own. There are others e.g. Blaney and Criddle (1950 and earlier papers), Olivier (1953).

The THORNTHWAITE *formula*

THORNTHWAITE knows better than most physicists that solar radiation and atmospheric turbulence are the important factors in natural evaporation; and he also knows that the necessary records are available in few parts of the world and over large areas are non-existent. In his quest for a simple universal way of estimating potential transpiration he devised an empirical formula, based on rainfall minus run-off data for several American catchments, between potential transpiration and mean air temperature, with some auxiliary variables that give the actual formula an untidy look, much of which is removed by tables.

Those familiar with the THORNTHWAITE formula will know it is impossible to explain in a few words. The basic equation for a 30-day month is

$$E_T = 1.6\ (10\ T_a / I)^a \qquad (16)$$

where T_a is the mean air temperature in °C,
I is a heat index which is the sum of 12 monthly indices i given by
$i = (T_a / 5)^{1.514}$,
and a is a cubic function of I.

This expression for E_T has to be adjusted for length of month and length of day.

Rationalising somewhat in 1954, THORNTHWAITE writes "Temperature can serve as an index to potential evapotranspiration because there is a fixed relation between net radiation used for heating and that used for evaporation when conditions exist to achieve the potential rate".

This quotation is taken from near the end of THORNTHWAITE's paper in a series of reports of world-wide attempts to check the formula experimentally, (MATHER, ed., 1954). His next sentence but one is: "The problem of developing a formula for potential evapo-transpiration remains unsolved".

Here THORNTHWAITE is being too pessimistic. Considering its inherent simplicity and obvious limitations, his own method does surprisingly well. LEEPER (1950) has suggested that some of the original data on which the formula was based, and a wide range of Australian data could equally well be fitted by a simple linear regression on mean monthly air temperature and mean annual air temperature.

Rothamsted work on potential transpiration

Our own approach has had two phases, one empirical, the second analytical. The first was based on the assumption that for any site we can calculate a value E_o of the evaporation rate from a hypothetical open water surface using equation 15 and then apply a conversion factor, f, to obtain

$$E_T = f.\ E_o \qquad (17)$$

Experimental measurements of E_o and of transpiration from sub-irrigated short grass showed a very great scatter in short-period values, considerable scatter in monthly values, but, over a period of two years, revealed a marked seasonal cycle. As a recognition of the scatter the values of f are given without any attempt at precision. For S.E. England they are:

Table 2. Values of E_T / E_o.

Season	f
May–August	0.8
S. O.; M. A.	0.7
Nov.–Feb.	0.6
Year	0.75

Table 2 and the meteorological estimate of E_o have been used now for nearly ten years in a variety of applications in hydrology and agriculture, without disclosure of any major errors.

The analytical approach has attempted to find a theoretical value of the factor f. In the first stage (PENMAN and SCHOFIELD, 1951) the approach was aerodynamic, attempting to evaluate the resistance to watervapour diffusion from inside the leaf to the outside as a function of stomatal geometry and population. (Recently BANGE (1953) has set out a similar mathematical argument in detail). This leads to a stomatal term, S, such that, other things being equal, S $= f$. The order of magnitude is about 0.9, but is always less than unity. For most plants stomatal opening is a day-light phenomenon, and hence transpiration opportunity is limited by length of daylight: so, making some simplifying assumptions to get an order of magnitude it is possible to derive a daylength factor, D, in the form

$$D = \frac{N}{24} + \frac{a}{b\pi} \sin \frac{N\pi}{24}$$

where a and b are vapour pressure differences with a usually $=$ or $< b$. (For $N = 12$ and $a = b$, $D = 0.5 + \frac{1}{\pi} = 0.8$). This term, D, is in fact the main determinant of the seasonal cycle in f; near the equator the annual cycle will be small.

Both S and D are thus less than unity, indicating that E_o will always be greater than E_T. Hence on energetic grounds we should expect the transpiring surface to have a higher mean temperature than an open water surface, i.e. in the overall diffusion equation $e_T - e_d$ will be greater than $e_o - e_d$. Our rather crude measurements confirmed this, so in f there is a temperature-dependent term greater than unity. Roughly, very roughly, this just about balances the S term, leaving daylength as the dominant factor in f. Checks on Rothamsted data were good enough to suggest that the analysis had probably included the important physical factors. Even so it was still necessary to estimate E_o as a first step in finding E_T. This has now been overcome (PENMAN, 1952) by a combination of aerodynamic and energy balance approaches for a fully transpiring surface, assuming that the surface temperature of the plant cover (which determines the sensible heat transfer to the air) is the same as the inner leaf temperature (which determines the saturation vapour pressure inside the leaf). The two formal equations are

$$E_T = f(u)\ (e_T - e_d)\ SD$$
$$H_T = E_T + K_T$$

and the solution is

$$E_T = \left(\frac{\Delta}{\gamma} H_T + E_a\right) \Big/ \left(\frac{\Delta}{\gamma} + \frac{1}{SD}\right) \qquad (18)$$

Comparing this with the corresponding expression for E_o, equation 15, then because $H_T < H_o$ and $SD < 1$, E_T is always less than E_o.

This simple expression conceals the complication in getting values of S and D: it is not yet a working equation. It does, however, offer a basis for a third generalisation about potential transpiration :

3 The transpiration of a short green cover cannot exceed the evaporation from an open water surface exposed to the same weather.

Statements to the contrary are sometimes made : they should be received with caution, if not disbelief.

Plant and soil factors in transpiration

There is not time for any adequate survey of the tests and applications of either the THORNTHWAITE or the Rothamsted estimates of potential transpiration. THORNTHWAITE's formula has been widely used by geographers while its author was patiently seeking evidence to establish its truth. THORNTHWAITE's comment on the lengthy volume of data edited by MATHER has already been quoted, and to this can be added a further quotation from the summary of a recent paper by THORNTHWAITE and MATHER (1955), "The methods of.... computing (potential evapo-transpiration) from readily available climatic data have undergone a thorough re-investigation during the course of an intensive program to develop a world wide network of evapo-transpiration measuring stations. As a result of recent improvements in instrumentation and observations it appears feasible to employ the energy balance method, long used in computing evaporation, in making estimates of the maximum possible water loss from different vegetative-covered areas."

Rothamsted work has been used, rather than tested, in a wide variety of applications, including studies of the running of field drains, the water balance of catchment areas, mean annual evaporation for the British Isles and for parts of Europe, the estimation of the frequency of irrigation need in Britain, and the control of irrigation operations in field experiments on farm crops. There are published accounts of all this work (PENMAN, 1949–54, PEARL, 1954). Elsewhere, overseas workers have discussed or applied the formula, notably ROBERTSON in Canada (1953), VAN BAVEL and WILSON in North Carolina (1952), VAN WIJK and DE VRIES (1954) and MAKKINK (1955) in Holland, CALEMBERT (1954) in Belgium, and UHLIG (1954) in Germany. My own general impression of the performance of the formulae is that there has been a tendency to overestimate potential transpiration (a defect that is partly cured by the use of the new expression for E_a), that on an annual basis it gets within $\pm$ 10% of the truth, and that seasonal errors may be much greater. There have been occasional indications that because of neglect of the changes in heat storage in the soil the predicted time of seasonal maximum is earlier than it in fact occurs.

Any meteorological physicist will find it easy to criticise the formulae for their sweeping simplifications of complex meteorological phenomena. Soil physicists, however, might support a claim that crude as the method is, it can give estimates of changes in soil water content as accurately as any simple field method at our disposal, realizing that the errors of estimation can sometimes be matched by errors in assessment of rainfall, and are usually less than the errors in estimating how much irrigation water has been applied to a chosen area.

It has already been carefully stated that this work has been used rather than tested. In nearly all of the applications there have been other important unknown quantities about which assumptions or guesses have had to be made. The most important of the decisions that has had to be made is: when does the actual transpiration rate equal the potential rate, and when does it fall below the potential rate?

It will shorten discussion to accept the following as an axiom: The maintenance of maximum transpiration rate is a necessary condition for maintenance of maximum growth rate. *This may not be a sufficient condition for maximum* growth, but stating the problem in this way will avoid an undesirable confusion between transpiration and growth, a confusion that appears in most discussions of available water, including the very fine collection of opinions and evidence in the discussion of a recent paper by VEIHMEYER and HENDRICKSON (1955). The immediate problem then is: Given a deep soil initially at field capacity and carrying a complete cover of a short fresh green crop, how long will the transpiration rate be maintained at its initial value in weather that imposes a steady daily value of the potential transpiration rate? Alternatively, how dry can the soil become before there is a check to transpiration? And what happens after that?

VEIHMEYER's (1927) answer is well known, an answer that I was as ready as any to challenge before I read the paper carefully. Growing small peach trees in tanks he found that the transpiration rate per unit area of leaf was the same for tanks kept near field capacity as for tanks that were allowed to dry until the plants were near wilting before rewatering. His conclusion was that all the water in the soil above the water content at which plants would permanently wilt is equally available for transpiration at the same rate. On the evidence presented in 1927, I accept his conclusion for the soil type, the plant type, the root distribution in his tanks and the peculiarity of exposure employed in his experiments. At the 1955 discussion he and his supporters produced evidence to show that his generalisation was true of other plants and soils; his opponents criticised the old and new evidence, and produced their own to show plants and soils of which it was not true.

It is doubtful if anyone can confidently state whether VEIHMEYER has discovered a general rule to which there are a few exceptions, or has stumbled upon a few special cases. Certainly VAN BAVEL (1953) and I (PENMAN, 1949) have gone some way with VEIHMEYER in our attempts to decide when soil dryness will check transpiration. VAN BAVEL's experience with very shallow soils in North Carolina has led him to postulate a *steady transpiration rate up to a limiting* value of soil dryness fixed by the depth of soil. Then there is an abrupt cut-off because all the available water has been used. He is not ready to accept the water content when plants wilt as the lower limit of available water. My own speculations have been based on behaviour of deep soils in which the quantity of readily available water is limited not by depth of soil but by the depth to which roots have time to penetrate; and as plant roots vary in their foraging ability available water can be regarded as a plant characteristic rather than a soil characteristic. Hence the name 'root constant' used in Rothamsted discussions of available water. So, like VEIHMEYER and VAN BAVEL, we think of the soil profile as being capable of yielding water to plants at the potential rate until readily available water round the roots is used. Thereafter, unlike both, we consider that limited supplies can come from the soil below rooting depth: there is a fairly sharp transition from the uniform maximum rate of withdrawal but while the later rate is relatively smaller, it is significantly greater than zero.

In contrast, there are many who will say that the rate begins to decrease below the potential rate as soon as the soil water content falls below field capacity. Some of the evidence is irrelevant (though important in its proper context) because it shows decreases in *growth rate* as soon as the soil falls below field capacity. There is relevant experimental evidence on transpiration rates, (THORNTHWAITE & MATHER, 1954) showing transpiration rate as a continuously decreasing function of increasing soil dryness.

In essentials this is a problem in soil physics, the problem of water flow in an unsaturated soil under a suction gradient. Recent and current work by STAPLE and LEHANE (1954) in Canada, and by CHILDS and COLLIS-GEORGE (1950, 1953) in Cambridge – among others – will help greatly when the biological problem is more clearly defined.

Transpiration and growth

While it is essential to aim at complete knowledge of the three phases of transpiration, namely, water movement in the soil to the root, water movement in the plant to the leaf, and vapour movement from the leaf to the air, we must remain aware of the agricultural importance of what is being done. We must be careful to avoid regarding the purpose of land and crop management (which includes irrigation) as a way of maintaining maximum transpiration. Apart from economic factors that may demand the retardation or acceleration of development to meet a particular market at a particular time, it must be remembered that the parts of plants we harvest vary greatly: roots, shoots, fruits, flowers, leaves, seeds. To get the maximum harvest at the right time may demand an equally wide variety of seasonal cycles of soil water régime. Some simple generalisations are possible: such as for example, that keeping the soil near field capacity will encourage leaf growth, but each crop may present its own particular problem. This is our attitude in current irrigation experiments on farm crops. The amounts and times of watering are based on meteorological estimates of potential transpiration calculated week by week from weather data taken on the site, and the principal objective is to determine for each crop what soil moisture deficit the crop can tolerate without major check to growth. So far, it is clear that grass and early potatoes give maximum yield when the soil is kept close to field capacity, while barley and sugar beet can tolerate dry soil and need irrigation only in relatively dry summers.

In thinking of the relationship of growth and transpiration there is little value in the concept of 'transpiration ratio', for there is no reason to suppose that a plant must transpire a fixed quantity of water to produce a given quantity of dry matter. The transpiration rate is dominated by weather; the growth rate admittedly depends on the same weather (but we are still only groping for the solution of this fundamental problem in agricultural meteorology), but can show enormous variations because of differences in soil fertility or incidence of disease.

Here it may be useful to make a distinction between growth and development. As an example, the poor gardener grows flowers that are usually small and miserable; the expert grows flowers that are usually large and magnificent. The two lots of flowers are equally fully developed – all the parts are there – but the growth is different. THORNTHWAITE (1953) has made the interesting suggestion, based on experience, that the rate of development of a plant is proportional to his potential transpiration – and has profitably exploited the idea in the planning of the planting of peas for canning. It *may* be that there is a link between development and transpiration; alternatively it is possible that the link is between development and air temperature since his figure for potential transpiration is essentially a complex function of temperature. ROBERTSON (1953) can be thought of as supporting the alternative as he has been solving the simultaneous equations for heat budget and turbulent transport to find values of the surface temperature (instead of evaporation). His report shows promise of a useful link between this calculated temperature and the development of crops in the extreme north of Canada.

CONCLUSION

This survey of evaporation as an agricultural phenomenon has been limited to an outline of basic ideas needed for subsequent discussion of the topic. The omission of reference to field aspects of the problem has been deliberate, partly to restrict the size of the survey, and partly because discussion of experimental methods and experimental results might be expected to take up the major part of the time of the conference.

There are problems at three levels. First there is the purely scientific problem of the physics of the evaporation process, the solution of which involves complex ideas, complex experimental techniques, and may at times appear completely irrelevant to field problems. Progress here has been encouragingly good, along three lines. The newest, and most in need of further exploration, is the study of fluctuations of water vapour content and upward air movement in eddy motion, offering reliable short-period estimates of evaporation over any kind of surface. Next there is the older aerodynamic method, dependent on estimates of transport constant and vapour pressure gradient, not entirely free from assumptions and arbitrary constants, but already being successfully used to give hour-to-hour estimates of evaporation rates over land surfaces. The energy-balance method approaches the ideal in theory, but in practice there are great difficulties in measuring some of the terms other than evaporation, and for the sensible heat transfer to the air it is necessary to fall back on aerodynamic ideas. Though in all three lines the complexity of ideas has been resolved, more or less completely, each of them has its own particular technical difficulties in measuring relevant air and soil parameters. Common to all is the difficulty of getting a direct measure of evaporation to compare with any calculated value: it is a measure of progress that meteorological estimates of evaporation from land surfaces appear to be more accurate than any available field method of measuring changes in soil water content.

The second group of problems is partly biological and partly pedological in origin. Under ideal conditions (often realized in the field and therefore not absurd agronomically) the plant can be regarded as a passive channel between the water in the soil and the atmosphere above. Transpiration is then dictated by prevailing weather, but non-ideal conditions may exist because of limited water supply in the soil, inadequate root range of the plant, marked resistance to water movement through the plant (a problem not considered in the survey), stomatal closure in the leaves, or for other reasons. The reaction of the plant to non-ideal conditions, in its transpiration rate, in its development, or in its growth may vary from one kind of plant to another, and for a given type may be markedly dependent on its stage of development.

The third group of problems is a combination of the other two groups: it is concerned with the field aspects of water use by agricultural crops. From the first group it is necessary to take some solution of the physical problem such as the empirical solution of BLANEY and CRIDDLE, or the combination of aerodynamic and energy estimates used with some success at Rothamsted, obtaining in one way or another an estimate of potential transpiration rate. From the second group must be taken some assumption or guess at plant water needs, what stress can be tolerated without check to growth, perhaps what stress may be needed to induce maximum development of the part of the plant to be harvested. The essence of the first contribution is to get a way, or ways, of estimating evaporation over large areas and long times without the use of complex equipment, or time-consuming analysis of multiple records. The required degree of accuracy will vary with circumstances, but it will rarely need to be better than the accuracy with which irrigation water can be measured. There seems a fair prospect that this can be achieved in humid areas or in irrigation areas. There is much more uncertainty about the second contribution, as there are still major differences of opinion, and clashes of evidence, about which plant and soil factors are unimportant enough to be negligible, and which are so important that they must be taken into account.

Though the physicist still has some problems he can solve by himself, much of his future contribution to understanding of evaporation in agriculture must be in collaboration with the biologist and the soil scientist.

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