Aust. Met. Mag. 47 (1998) 273-83

Physics, mathematics, and the environment: the 1997 Priestley lecture*

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(Manuscript received February 1998; revised April 1998)

Early years

Nearly 51 years ago, December 1946 was an important month for Bill Priestley and also for me. Both of us were in Melbourne. On December 23 Bill got off the ship and was having his first experience of Australia. Two days earlier I had been in the old Wilson Hall, having conferred on me the degree of Bachelor of Civil Engineering.

In the aftermath of World War 2, the Suez Canal was closed and Bill and his bride Connie had suffered the six weeks rigor of the voyage of the *Dominion Monarch* from Southampton around the Cape. Bill was packed in a crowded cabin for 10 men; and his bride in a crowded cabin for 10 women.

Bill had arrived into David Rivett's CSIR. Rivett's principle of scientific management was simple and effective: find the best man to head up the task; then give him the maximum freedom and help to get on with it. (Rivett recognised that, in uncorrupted English, words such as 'man' and 'him' serve for both genders when neither is specified.)

Though its brief was based in science, the British Meteorological Office sat fair and square in the Civil Service, with its attendant rigidities. One can envisage how attractive it was to the 31-year-old Priestley to be plucked from the ranks of the Met. Office and given carte blanche to set up a unit to pursue fundamental research into meteorology, even if it was at the other end of the world.

Early 1947 found Priestley, a caged bird released, stretching his wings and exploring his new world. He discovered Taffy Bowen in Sydney and beat a hasty retreat to Melbourne. He was set up there in an old flour mill in Flinders Lane; and it was from his flour mill that he began to find his way around Rivett's CSIR and to formulate his research plans.

Early 1947 found me also in Rivett's CSIR, though totally by accident. I was a precocious (and doubtless irritating) youth of 19. Conformist that I was, I tried, like the rest of my graduating class, to get a job as an engineer in the Victorian State Public Service. My contemporaries, old men of 22 or more, took up posts as engineers on the princely salary of £330 a year. On the other hand, I was found not be an adult: all I could be offered was an engineering cadetship at £205 a year. I hungered for money and refused the offer. The best-paid job I could find was that of labourer in the Spencer Street railway goods yard; and I was about to start work there when the University, for the first time ever, advertised for a graduate assistant in agricultural engineering.

The job paid £350 a year, with no questions asked about age. So, out of pure lust for money, I blundered into the line of work that has turned out, over the last 50 years, to be more fun than work.

The engineering course I'd just completed was doubtless useful in some ways; but the message I took away from it was: 'All things are understood, and all a young engineer needs to know is what handbook to use.' This suppression of curiosity and removal of intellectual challenge made the course utterly boring, and its products brain-dead. I guess the modern version of that old message is 'All things are understood, and all a young graduate needs to know is what software to use.'

This brain-dead youth was immediately seconded to what was then the CSIR Irrigation Research Station at Griffith in the Murrumbidgee Irrigation Area. To find myself in Rivett's CSIR was a sudden illumination. Agricultural scientists at Griffith had been struggling for a decade to understand the hydraulics of furrow irrigation, but suffered the handicap of having little or no physics and mathematics. They, at least, knew all too well that all things were not understood; and I found myself enjoying the pure luxury of being challenged to unravel phenomena not previously understood, and being actually encouraged to do so.

It soon became clear to me that even the elementary physics and mathematics of a callow young engineer could help with the vast array of unresolved problems of

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* Based on the lecture delivered at the CSIRO Division of Atmospheric Research, October 7, 1997. The paper is published with minimal editing in order to retain the flavour of the original presentation.

how water and other entities were transported through the natural environment of the soil, the plant, and the atmosphere.

In the following few years I was in Queensland as an engineer in the Irrigation Commission, which was experiencing a post-war renaissance. I found myself investigating, designing, and laying out a number of irrigation schemes. I was called on even to invent (what are now called) cost-benefit analyses of these quite large schemes. But this was a world short of basic data, let alone a sound understanding of the processes involved. I was able to carry out my duties only through a mix of guesswork, imagination, and sheer chutzpah. I grew progressively more unhappy not only with the rigidities of a State-run engineering bureaucracy, but also with the base of deep ignorance from which all our work sprang. I yearned to return to the atmosphere I'd breathed briefly at Griffith; and I knew that, if I did, I would never apologise for attacking any problem or studying any process at as fundamental a level as I could.

Liberation came when I took up a research position with CSIRO at Deniliquin in late 1951. But this great gain had some initial pain. Firstly, I had to cast aside my lust for money, and take a 25 per cent drop in salary. Secondly, Deniliquin was still suffering a serious post-war housing shortage: there was simply no accommodation. So we started life in Deniliquin in a tent on the banks of the Edward River. At the time I was so intent on hurling myself into my research that I hardly noticed the privations; but it must have been hell for my dear wife Frances who, for good measure, was pregnant. The low point in the tent came when an errant ewe not only did a Goldilocks on our bed, but left behind a generous fecal calling-card. I'm not much given to guilt; but in after years I have felt guilt over this early episode. Frances, stout soul that she is, insists she enjoyed it all.

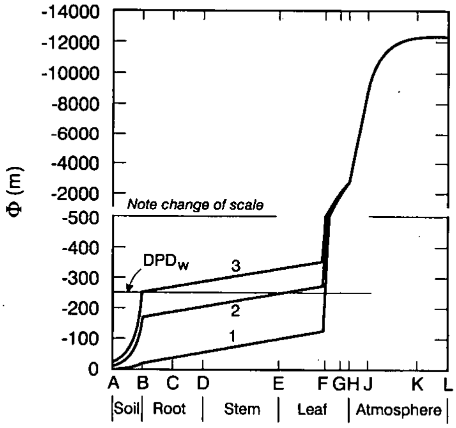
A primary concern of the Deniliquin laboratory was the irrigation of pastures. The water demand of vegetation and evaporation losses from the supply system were thus topics of concern. These meteorological connections led to my first personal contact with Bill Priestley, I think in early 1952. I made the pilgrimage to Highett, where Meteorological Physics was then based; and on that occasion and others over the next few years, I found myself trying to talk about evaporation and transpiration not only with Bill, but also with Bill Swinbank and Len Deacon. I have the impression that the sight of this bumptious, and presumably ignorant, Ocker yokel froze all three of them into silence; and the more silent they became, the more nervously the yokel would babble. They seemed to form an inscrutable and very English tripartite deity: Priestley the lordly Father, Swinbank the more down-to-earth Son, and Deacon the painfully shy and suitably vague Holy Ghost. But, of course, as time went by and the British nucleus was diluted with local recruits, the Trinity adjusted to Australian ways and became suitably hail-fellow-well-met.

In my early days at Deniliquin, I struggled to come to grips with the whole gamut of processes involved in the terrestrial hydrologic cycle. It was clear that this involved no one particular established scientific discipline but, at the very least, all three of soil physics, plant physiology, and meteorology. This was expressed and explained in the schematic picture shown here as Fig. 1 (Philip 1957a).

This diagram depicts the gradients of partial Gibbs free energy, water potential, activity (call it what you will) that drive the transport of water through the soil to the roots; from the roots through the plant to the leaves; out of the leaves through the somata into the adjoining air; and then upward to higher levels of the atmosphere. This figure, published in the 1950s,[1] was the first explicit recognition in the literature of the whole soil-plant-atmosphere thermodynamic continuum for water transport.

This continuum was firmly in my mind in 1953, when the CSIRO Executive set up a Committee to advise it on research in hydrology. The Committee was E. Sherbon Hills FRS, Professor of Geology at Melbourne University and Norman England, a pedolo-

Fig. 1 **The soil-plant-atmosphere thermodynamic continuum, as depicted and explained in Philip (1957a).**



The soil-plant-atmosphere continuum showing profiles of total potential Φ: 1, during normal transpiration; 2, during temporary wilting; 3, at permanent wilting. Points of the transpiration path: A, soil (a definite distance from the plant root); B, surface of root hairs and of absorbing epidermal cells; C, cortex; D, endodermis; DE, vessels and tracheids in xylem; E, leaf veins; F, mesophyll cells; FG, intercellular space and substomatal cavity; GH, stomatal pore; HJ, laminar sublayer if present; JK, turbulent boundary layer; KL, free atmosphere; DPD_W denotes Φ-value at incipient plasmolysis of root cells.

gist-cum-hydrogeologist with the NSW Water Conservation and Irrigation Commission, with me as Secretary, i.e. the person who actually did the work.

From 1930 the American Geophysical Union had recognised Hydrology as one of three fluid-earth Sections, the others being Meteorology and Oceanography. But, unlike its fluid-geophysical sisters, hydrology lacked a coherent intellectual framework: each practitioner tended to be immersed in his own special applied problem: hydrology meant wildly different things to hydrogeologists, urban drainage engineers, and irrigationists. The Committee recommended that CSIRO set up a Section of Hydrology to conduct research on basic processes of the terrestrial hydrologic cycle. Its report included the chart, shown here as Fig. 2, which explained how the various scientific disciplines impacted on the diversity of processes; and how understanding and control of these processes could lead to better use of land and water.

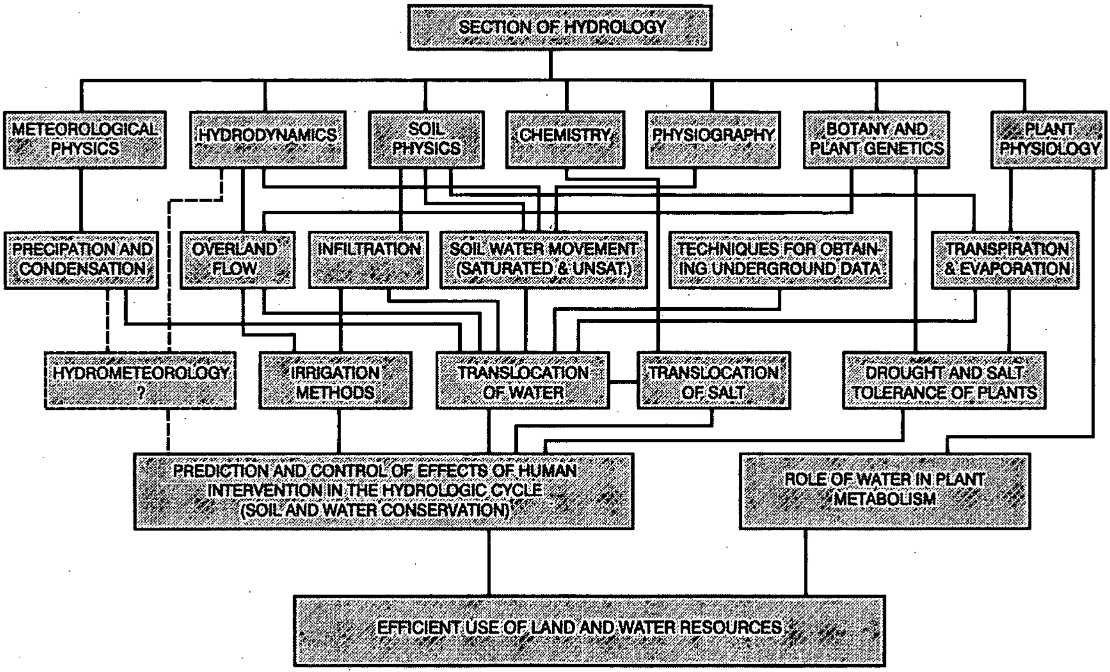
On December 8, 1953 the Executive put to six relevant Chiefs and Officers-in-Charge the proposal for a Section of Hydrology. The Emperor-Chiefs opposed the idea, and the Executive went into retreat. Lord Acton would have smiled. Here is an extract from the record of the meeting:

> ***It was agreed to record*** *that there was some support for the development of hydrology as a science in its own right both in its pure and applied aspects and* ***that there was both support and opposition*** *to the establishment of a separate hydrological group either in CSIRO alone or in collaboration with a university.*

The bold face is mine. Weasel words were as prevalent in 1953 as today. Two of the emperors were Otto Frankel and Bill Priestley. They were among the most enlightened of CSIRO Chiefs, and both were good friends to me and very helpful over the years, but I didn't think too well of them at the time. It is ironic that it is only now, in 1997, that CSIRO has an entity, Land and Water, with essentially the brief we proposed 43 years ago.

Opposition to these ideas came from many soil and water people darkly suspicious of an approach with a strong flavour of physics and mathematics. The art of abstracting physically well-based entities from the real world that are amenable to mathematics was not much understood. I remember trying patiently to explain to largely disbelieving audiences how physics and mathematics could illuminate their problems (Philip 1957b).

Fig. 2 **A proposed Section of Hydrology. Extract from report to CSIRO Executive, 1953.**



Water movement in unsaturated soil

I turn now to describing samples of physical-mathematical research undertaken in the group that started out in Deniliquin and ended up known as CSIRO Environmental Mechanics for more than half of its 45-year existence.

Water movement in unsaturated soil provides an excellent example of the fruitfulness of applying physics and mathematics in the environment. In its natural state, the soil is normally unsaturated: that is, it contains both water and air. Most water of the terrestrial hydrologic cycle spends all its time in unsaturated soil. Here in Australia about 93 percent of precipitation enters the soil; and 92 percent returns directly to the atmosphere, only about 1 per cent reaching the groundwater.

Among the processes of water movement in unsaturated soil of great concern to hydrologists are infiltration (the entry into the soil of water arriving at its surface); drainage and retention of water in the soil strata; extraction of soil water by plant roots and its subsequent transpiration; and evaporation of water directly from the soil.

The character of these everyday, but all-important, processes depends on the physical uniqueness of water, namely that its surface tension is so great, and that there are also adsorptive (and in clay soils electrostatic) forces between water and soil particles. This means that the soil can hold appreciable quantities of water against gravity. This makes possible the whole range of moisture conditions at the surface: in the absence of capillarity the land surface would be either desert or swamp, and plants would have evolved differently. Note that here we lump these various water-soil interactions under the convenient but loose term 'capillarity'.

A proper understanding of these processes came very late in the history of hydrology. Until the mid-1950s, they were treated more or less at the folklore level. Each phenomenon had its separate empirical and often inaccurate explanation, with no physical basis for quantitative prediction. Since then a systematic mathematical-physical analysis of the interplay of capillarity and gravity in these phenomena has made the study of water movement in unsaturated soils a reasonably coherent and quantitative branch of physical science.

Astonishingly, a thorough-going analysis of just how water soaks into soil wasn't made until 1953. The story goes back, however, to 1907 and the American physicist Edgar Buckingham.

Total potential and moisture potential in unsaturated soils

Because of the various forces between water and soil that we have mentioned, the water in unsaturated soils is not free in the thermodynamic sense. Buckingham (1907) was the first to appreciate that these conservative forces governing the equilibrium and movement of the water are amenable to treatment through their associated scalar potentials.

We define such potentials relative to the reference state of water at atmospheric pressure and datum elevation $z = 0$. Here z is the vertical space coordinate, conveniently taken to be positive downward. Then, for a nonswelling medium the total potential is

$$\Phi = \Psi - z \qquad ...1$$

The moisture potential Ψ is the potential of the forces arising from local interactions between water and soil.

In water-wet nonswelling soils, $\Psi = 0$ at saturation, and $\Psi < 0$ in unsaturated soils, decreasing with the volumetric moisture content θ to very large negative values (typically -10^4 m) at the dry end of the moisture range of interest. Figure 3 shows a typical $\Psi(\theta)$ relation.

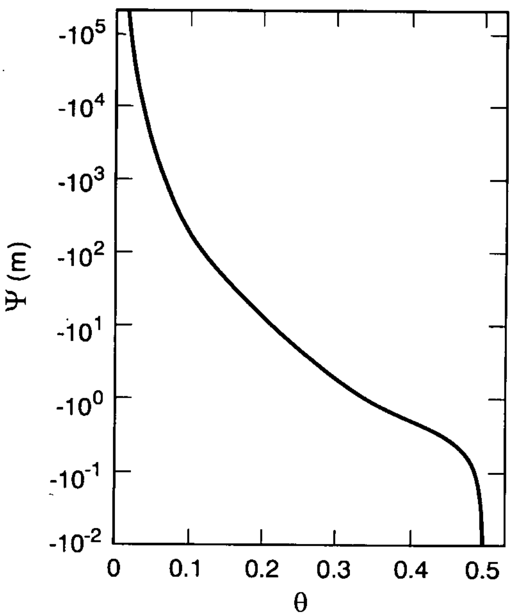
Darcy's law for unsaturated soils

In addition Buckingham suggested that, for unsaturated soils, Darcy's law should hold in a generalised form in which the hydraulic conductivity K was a function of θ. The appropriate form of Darcy's law is thus

$$v = -K(\theta)\nabla\Phi \qquad ...2$$

where v is the vector flow velocity. Figure 4 shows the $K(\theta)$ relation for the soil for which Fig. 3 gives $\Psi(\theta)$.

Fig. 3 **Relationship between moisture potential, Ψ, and moisture content, θ, for Yolo light clay (Moore 1939).**



The general flow equation

After Buckingham, nothing happened for almost a quarter century. Then Richards (1931), in his PhD work at Cornell, combined Eqns 1 and 2 with the continuity equation, obtaining the general flow equation for water in unsaturated soils

$$\frac{\partial\theta}{\partial t} = \nabla(K\nabla\Psi) - \partial K / \partial z \qquad \ldots 3$$

where t denotes time. For a uniform soil and nonhysteretic processes we may take K and Ψ as single-valued functions of θ. This gives the form of Eqn 3 with θ as dependent variable

$$\frac{\partial\theta}{\partial t} = \nabla \cdot (D\nabla\theta) - \frac{dK}{d\theta}\frac{\partial\theta}{\partial z} \qquad \ldots 4$$

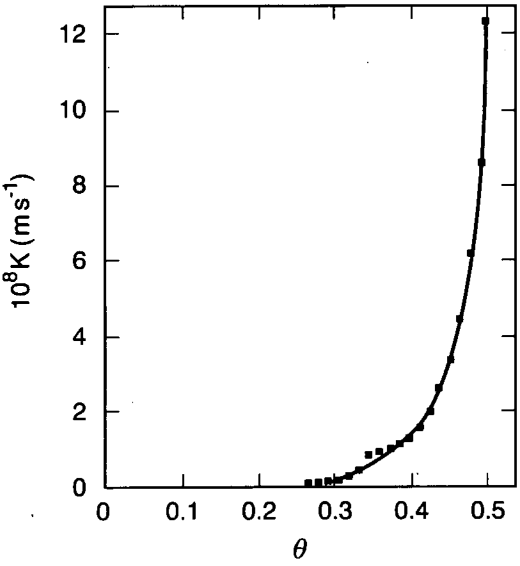
with the moisture diffusivity

$$D = K d\Psi / d\theta \qquad \ldots 5$$

In general, the coefficients D and $dK/d\theta$ are strongly varying functions of θ and Eqn 4 is a strongly nonlinear convection-diffusion equation. Figure 5 shows $D(\theta)$ for the soil for which we have already seen $\Psi(\theta)$ and $K(\theta)$.

For more than 20 years, Richards' Eqn 3 lay around like some strange object fallen from the sky. The natives looked at it with some awe, but knew not what to do with it.

Fig. 4 **Relationship between hydraulic conductivity, *K*, and moisture content, θ, for the soil of Fig. 3. The squares indicate experimental observations.**



Then Klute (1952), also in his PhD work at Cornell, gave a solution for horizontal one-dimensional flow, driven purely by capillarity, with no gravity. Reading his paper in Deniliquin, I saw the way to go. The unsolved real problem was that of infiltration, of how water made available at the soil surface enters the soil and moves downward in it. In formal terms what was needed was the solution of the one-dimensional vertical form of Eqn 4.

$$\frac{\partial\theta}{\partial t} = \frac{\partial}{\partial z}\left(D\frac{\partial\theta}{\partial z}\right) - \frac{dK}{d\theta}\frac{\partial\theta}{\partial z} \qquad \ldots 6$$

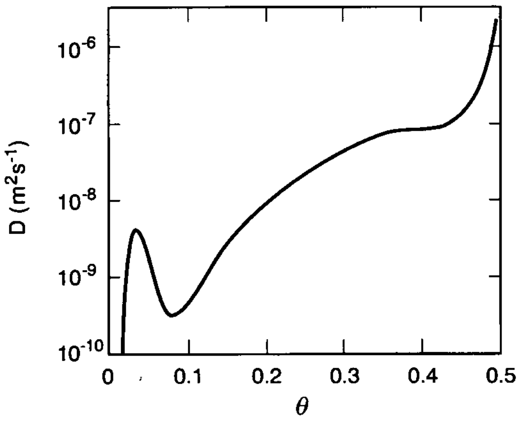
that describes what happens when water is suddenly made available at the surface of a soil initially at some uniform moisture content less than saturation.

The infiltration solution

Both D and $dK/d\theta$ are empirically known functions of θ, and are different from one soil to another. The problem looked horrible; and it seemed that, if I were to solve it, it would have to be by computer. At the time the one computer in Australia was CSIRAC, recently moved from CSIRO Radiophysics in Sydney to the Mathematics Department at Melbourne University. Early 1953 found me in Melbourne attending Tom Cherry's course in programming CSIRAC. Some of you may not know that in those far-off days programming was done in machine language.

I soon realised that CSIRAC had far too minute a memory and was far too slow to solve my horribly nonlinear partial differential equation; and, into the bargain, the waiting-list to use CSIRAC was at least three years long.

Fig. 5 **Relationship between moisture diffusivity, *D*, and moisture content, θ, for the soil of Fig. 3. (The peak at small θ arises from vapour transport, not discussed here.)**



In the event, this was for me terrific good luck. It forced me actually to use my brain. At small t all the action had to be at small z. And I saw that the dimensional makeup of the right side of Eqn 6 required that, with z small, the first term had to be large compared with the second. This meant capillarity dominated gravity at small t.

Klute had worked with the nonlinear diffusion equation you get with gravity dropped from Eqn 6 and arrived at the similarity solution

$$z(\theta, t) = \phi_1(\theta)t^{1/2} \qquad ...7$$

Here $\phi_1(\theta)$ was the solution of a nonlinear ordinary equation, easily found numerically (Philip 1955). Equation 7 had been known to Boltzmann (1894). Obviously the required solution of Eqn 6 was essentially a gravity-perturbation of Eqn 7. A little work showed that it was, in fact,

$$z(\theta, t) = \phi_1(\theta)t^{1/2} + \phi_2(\theta)t + \phi_3(\theta)t^{3/2} + ... \qquad ...8$$

$\phi_1(\theta)$ was as before; and $\phi_2(\theta)$, $\phi_3(\theta)$ etc. were solutions of ordinary equations that were linear, and so even easier to find than ϕ_1. The series on the right of Eqn 8 is rapidly convergent. Typically 3 or 4 terms suffice to give accurate solutions for quite large t-values (Philip 1954, 1957c, 1957d, 1969a).

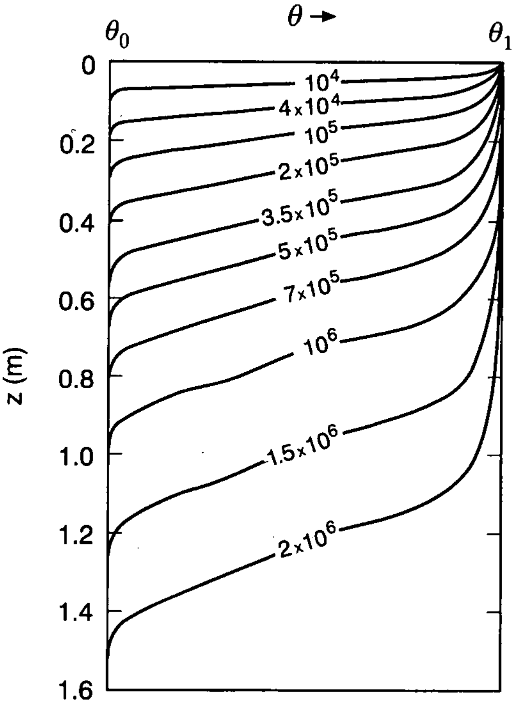
I found, further, that we could supplement Eqn 8 with the asymptotic travelling wave solution (Philip 1957e), valid for large t,

$$z(\theta, t) = (t - t_0)u + \zeta(\theta) \qquad ...9$$

The constant downward velocity of the moisture profile (travelling wave), u, follows simply from $K(\theta)$. The wave shape $\zeta(\theta)$ is given by a simple quadrature, and t_0 by a matching procedure. Figure 6 shows a sample solution for the soil for which we have already seen the $D(\theta)$ and $K(\theta)$ functions. Note that for $t \leq 10^6$ s the solution was calculated from Eqn 8; and from Eqn 9 for $t > 10^6$ s.

These solutions are a striking demonstration of the interaction between capillarity and gravity. The capillary-driven $t^{1/2}$ behaviour at small t gives way gradually to the travelling wave as gravity becomes more important. The wave expresses an ultimate dynamic equilibrium between capillarity and gravity (Philip 1969a).

Fig. 6 **One-dimensional infiltration into the soil of Figs 3-5. Computed profiles of moisture content, θ. Numerals on each profile represent value of t(sec) at which profile is realised. Profiles for $t \leq 10^6$ calculated from Eqn 8; those for larger t are based on Eqn 9.**



Some extensions

This has been simply a first episode in the story of using physics and mathematics in soil-water problems; but I have no time to go on and tell you of extensions to crusted and layered soils (Philip 1967, 1998), to two and three-dimensional systems (Philip 1966a, 1969a, 1989), and to the related processes of capillary rise (Philip 1966b) and evaporation (Philip 1957f) from soils.

Swelling soils

I must mention briefly, however, the extension to clay soils that swell and shrink and crack (Philip, 1969b,c,d, 1970, 1992, 1995). This work has depended very much on the stimulus and collaboration of David Smiles (Philip and Smiles 1969, 1982). We were able to show that, once some all-important modifications were grafted onto it, the foregoing non-swelling formulation carries over to one-dimensional flow and volume change in swelling soils.

Soils in the field are constrained to swell one-dimensionally – upwards: their particles must move upwards. So the process of wetting up such a soil requires that work be done against gravity; the specific gravity of clay particles is 2.65, whilst that of water is only 1. This has the radical consequence (regrettably not yet comprehended by many) that the role of gravity in the dynamics of water movement in wet swelling soils is essentially the reverse of what it is in non-swelling ones. Intuitions of hydrologists based on behaviour in non-swelling soils fail miserably if they are carried over to swelling soils (and they often are).

Some other physical/mathematical research

Somewhat connected to the swelling soil story is the development of the nonlinear diffusion formalism to analyse water flow and turgor changes in tissues and cell aggregations (Philip 1958). This arose also from our concern with the soil-plant atmosphere continuum, but we can note it only in passing. Other physiological work involving physics and mathematics included analysis of leaf-shading on the photosynthetic production of pastures (Davidson and Philip 1958); and the techniques of measuring foliage distribution, orientation, and density developed by John Warren Wilson (1965a, b) led to studies in geometrical probability (Philip 1966c).

Thermal transport in soils

In 1956 the effort at Deniliquin was enriched by the arrival of Daniel de Vries, a Netherlands physicist. De Vries brought new insights and knowledge and, though he stayed only three years, his influence on the directions of our research persists to this day.

Dan's PhD at Leiden had been on the thermal conductivity of soils and other porous media. It had been observed during the early 50s that the transport of H_2O down temperature gradients in such media exceeded the predictions of simple vapour diffusion theory by a factor often as great as 10. It was a considerable mystery; but Dan and I saw that the liquid islands about the points of contact between grains of the medium were not, as thought hitherto, barriers to vapour transfer, but were, rather, regions of accelerated transport. H_2O condensed at their upstream ends, rapidly passed through the islands by capillarity, and evaporated at their downstream ends. We were able to do the sums and establish the still-accepted theory of thermal transport in soils at moisture contents not too close to saturation (Philip and de Vries 1957).

Atmospheric convection - diffusion over heterogeneous surfaces

Local advection of sensible heat and water vapour

Dan de Vries looked around him at Deniliquin and saw that, for much of the time, the landscape was a patchwork quilt of extremes. Some patches were kept wet by irrigation, with the rest extremely dry much of the time. Some areas had lush vegetation and others almost none. It was clear to him that in this environment a micrometeorology based on the study of equilibrium profiles predicated on the assumption of homogeneity in the horizontal simply did not apply. One-dimensional micrometeorology was not enough: it needed to become two- and even three-dimensional.

So de Vries initiated at Deniliquin studies of local advection of sensible heat and water vapour between irrigated and dry areas (de Vries 1959). In doing this he founded a theme that has persisted in the group over the following forty years: research on the micrometeorological (and recently somewhat larger scale) effects of heterogeneities at the earth's surface.

It was our great loss when de Vries returned to the Netherlands at the end of 1958 to become Foundation Professor of Physics at the new Technical University of Eindhoven. There he prospered, becoming Vice-Rector of the University and one of his Queen's knights.

Our group then moved to Canberra, and we were joined there by Norman Rider of the British Met. Office. He had worked with Frank Pasquill at Cambridge; and he now set about developing the first carefully instrumented studies of local advection of sensible heat and water vapour. The experimental site was at the Fairbairn RAAF base at Canberra Airport. Dry-to wet and wet-to-dry experiments were run, with the test surfaces dry tarmac and well-watered closely-clipped grass lawn (Rider et al. 1963).

Roughness changes

Rider returned to England in 1962 and Frank Bradley took up the task. In the Fairbairn experiments we had tried to keep roughness changes between dry and wet to a minimum; but, of course, in the real world local advection of momentum between surfaces of different roughness is important also.

Frank now turned his attention to this process, studying the variation of wind profiles and shear stress downwind of abrupt changes of surface roughness. At the naval airbase at Jervis Bay, he worked with three different roughnesses. His drag plates let into the surface gave the first really accurate direct measure of surface shear stress and still lead the world. Bradley's observations remain a classic of meteorology (Bradley 1968 a,b). He was invited to bring his drag plates to the famous 1968 Kansas Experiment (Businger et al. 1971; Haugen et al. 1971). The various rival instruments intended to estimate Reynolds stresses turned out to be error-prone. Only Frank's drag plates gave reliable results, and supplied the standard against which the competing systems were judged. And the story was much the same for the 1976 International Turbulence Comparison Experiment carried out near Hay in the Riverina (Dyer et al. 1982; Dyer and Bradley 1982).

Topography

By the late 1970s our studies of surface heterogeneity extended to topography. In 1977 a 168 m steel lattice television transmission tower on 200 m high Black Mountain, 1 km from our lab, stood unused and awaiting demolition. We hated to see it going to waste, so Frank Bradley instrumented it and its upwind fetch and initiated our studies of wind flow over hills (Bradley 1980). He was joined in the ongoing effort by Peter Coppin; and Mike Raupach and John Finnigan became major players in the work, in parallel with their pioneering approaches to transport in plant canopies.

Wind tunnel

In the late 1960s Robin Wooding and Frank Bradley designed and supervised the building of our low-speed wind tunnel (Wooding 1968a,b). This has a large working section, designed to model micrometeorological processes. It is an integral part of our three-pronged attack on heterogeneity: field observation, wind-tunnel studies, and mathematical theory.

Surface heterogeneity. Blending heights.

I shall finish with a brief mention of some of my own simple adventures in applying mathematics to problems of heterogeneity. In the late 1950s I saw that the local advection of scalars due to a sudden change of surface conditions, as addressed by de Vries and Rider, could be treated rather simply, so long as we represented the vertical profiles of mean wind speed and eddy diffusivity as power-law functions of height. Similarity solutions then provided descriptions of internal boundary layers downwind of step-function surface changes (Philip 1959).

I have recently returned to the power-law representations. They are very old hat in the 1990s, and are much despised by the aficianados of turbulence. Of course they can do no more than produce an engineering approach giving, relatively simply, order-of-magnitude results to problems in complicated geometries. This is something in a world where sophisticated models of turbulence seem poorly fitted to coping with awkward boundary conditions.

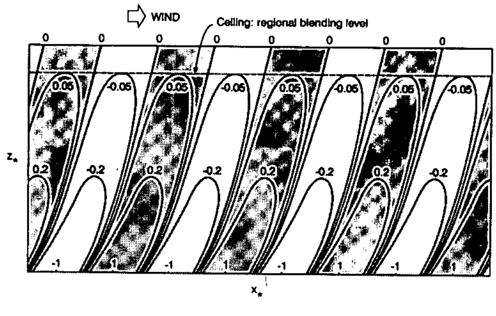
You are all familiar with the current industry of regional and larger-scale meteorological modelling. This has raised as an urgent issue the problem of regional parametrisation. How can we represent on the regional or global grid-scale, meteorological and hydrological conditions at a heterogeneous surface strongly varying on smaller scales? One answer is to take the floor of the large-scale model at the blending height, i.e. the height above the surface at which spatial fluctuations are decreased to a standard small fraction of their surface magnitude. Mike Raupach and John Finnigan put to me the problem of the dependence of blending heights on the wavelength of surface fluctuations. Figure 7 is a schematic picture showing periodic variation of surface conditions and its damping with height.

I solved the problem for surface boundary conditions periodic in one dimension and in two dimensions, including the general case with the wind oblique to the surface pattern (Philip 1996a, 1996b, 1997). Superficially the solutions of the convection-diffusion equation were horrible, being expressed in terms of Bessel functions of fractional order and complex argument. Fortunately the blending heights occur for arguments of the Bessel functions so large that we need use only the straightforward leading terms of their asymptotic expansions. Figure 8 shows, for an illustrative example, how the blending height varies with the obliquity, the angle between the directions of the wind and the surface pattern. We see that as the wavelength increases to 100 m or more, the blending height becomes sensitive to wind direction. In reality, of course, surface patterns will lack the exactitude of the model; but the results do suggest a directional effect on blending heights over man-made patterns such as city blocks.

Ship tracking

I'm almost finished; but I do want to say a brief word about some work in progress. Since the 1960s satellite images have revealed long tracks at low levels over large areas of the ocean. Figure 9 shows an image from 1987 was taken from altitude 850 km by the Advanced Very High Resolution Radiometer on the NOAA-9 polar-orbiting satellite. It is the 3.7 micron radiation image of the eastern North Pacific. You can see the West Coast of North America on the right side, with San Francisco Bay in the bottom right corner. It is important to stress that this image is for the 3.7 micron band. The tracks are almost invisible optically.

Fig. 7 **Schematic figure showing periodic variation of surface conditions and its damping with height. Numerals on the curves denote deviation from the mean as a fraction of the surface amplitude. After Philip (1996a).**



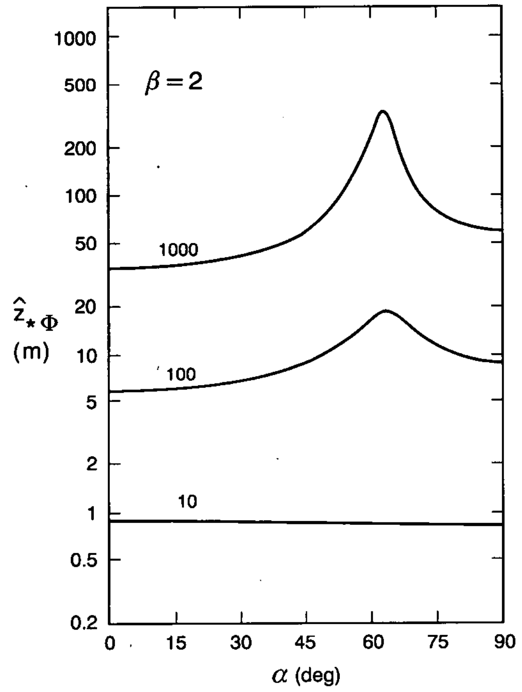


Fig. 8 **The variation of blending height for flux density, $\hat{z}_{*\Phi}$, with wind obliquity and check aspect ratio $\beta = 2$. Numerals on the curves are values of the smaller pattern wavelength (m). After Philip (1997).**

The explanation of the tracks is as follows. Marine environments far enough from the coast tend to have low concentrations of naturally occurring condensation nuclei. Accordingly the natural cloud cover over the ocean is made up of large droplets.

Diesel-powered ships emit large numbers of aerosols. These are transported by atmospheric convection-diffusion to the condensation level. There is thus an injection into the natural cloud of a very large number of extra condensation nuclei. The result is superposition onto the cloud cover of a contrail consisting of large numbers of very small droplets.

Absorption and reflectivity properties of the small contrail droplets differ strongly from those of the large natural droplets. It turns out that the reflectivity contrast is greatest in the 3.7 micron band, which gives the best image of the tracks.

I am most grateful to my friend Jim Rottman, working for Sciences Applications International Corporation, San Diego, for drawing all this to my attention, and for putting to me the question: Can we track a ship from satellite images of its contrail?

I've come up with a first exploratory answer, based on the most elementary mathematics. We simplify the problem by assuming the velocities of the ship and the mean wind are constant in time; and that the contrail boundary is also the contour of critical aerosol concentration on an assumed condensation level.

The contrail then behaves like an elongated kite towed by (or towing) the ship. The towing is not straight, but at an unknown angle to the ship's course. In plan, the invisible ship lies somewhere on the extension of the contrail axis; and we know also that the vector velocity of the contrail is also that of the ship.

I treated the problem as one of convection-diffusion from a moving continuous source in a shear flow. Preliminary work showed it was necessary to keep the Ekman spiral in the problem. As it happens, I built up the solution from that for the corresponding instantaneous source, published in the *Australian Journal of Physics* in 1962 (Elrick 1962). The author was Dave Elrick, a Canadian soil physicist with us at the time. It arose in his work on solute dispersion during water movement in soils.

Fig. 9 **A 3.7 micron radiation image of the eastern North Pacific, taken from altitude 850 km by the Advanced Very High Resolution Radiometer on the NOAA-9 polar orbiting satellite in 1987. The west coast of North America is visible on the right side, with San Francisco Bay in the bottom right corner. The author thanks Remote Sensing Laboratory, Meteorology Department, Naval Postgraduate School, Monterey, CA, USA, for kindly providing this image and allowing its publication.**

Here are the details of an illustrative example. The ship travels at 8 m s^{-1}; the mean wind speed at the exhaust height of 30 m is 10 m s^{-1}. The condensation level has the typical value 400 m. The eddy diffusivity between 30 m and 400 m was taken as 10 $m^2 s^{-1}$ and the vertical shear as 1.1 x 10^{-2} s^{-1} directed at 45° to the 30 m wind.

Figure 10 shows the overall set-up. The directions of the 30 m ('surface') wind and the shear are shown. The mode is the point of maximum reflectivity of the contrail. Relative to the mode, the ship may lie anywhere on the oval locus, depending on the ship direction. The contrail axis, in general, intersects the locus in two points. As Figures 11 and 12 demonstrate, properties of the contrail are highly sensitive to ship direction, giving ample clues as to which of the two ship positions is the right one.

Figure 12 shows the variation of contrail axis direction and mode distance with ship direction. Observed mode distances tend to be in the range of 10-20 km, agreeing well with our result. Figure 12 gives plots of the variation with ship direction of contrail length and maximum semiwidth. These are also consistent with observation. Average contrail length is 300 km, with semiwidths of order 5 km.

One final point. These results indicate that aerosols at the contrail mode originated from the ship about 30 minutes earlier. Those at its far end started out an average of 11 hours before. In the space age we read contrails, not entrails.

Fig. 10 **Ship tracking, illustrative example. Relative to the contrail mode, the ship may lie anywhere on the oval locus, depending on ship direction.**

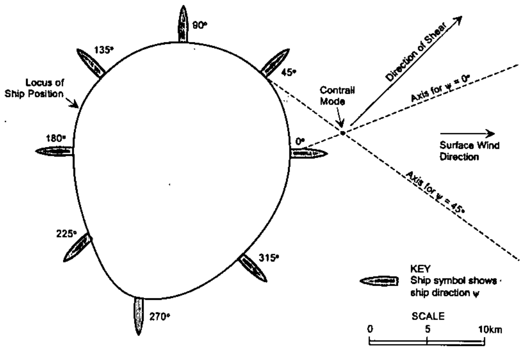


Fig. 11 **Ship tracking, illustrative example. Upper graph: dependence of direction of contrail axis on ship direction. Lower graph: dependence of distance of ship from contrail mode on ship direction.**

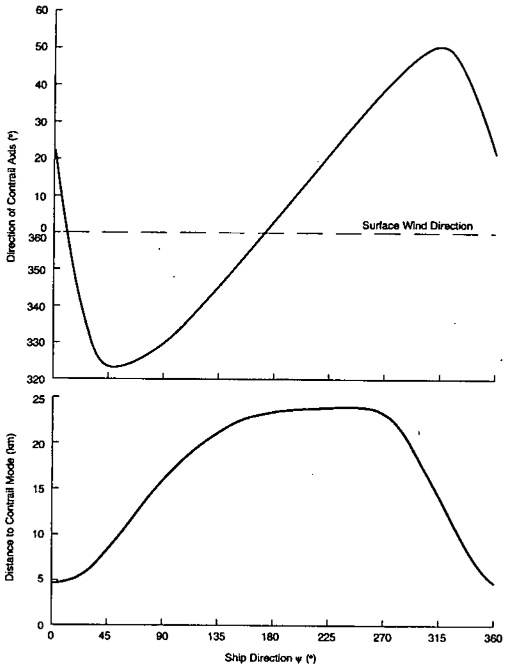
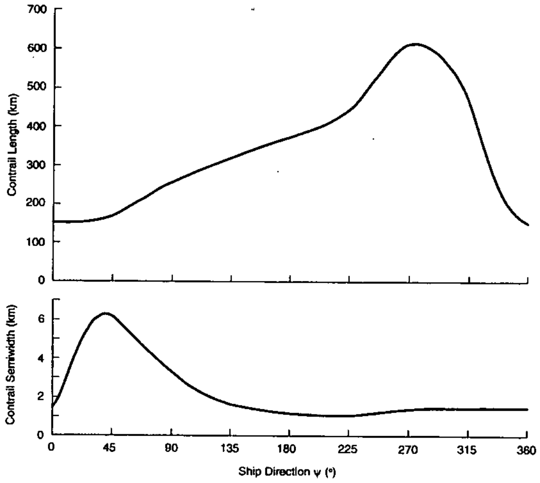


Fig. 12 **Ship tracking, illustrative example. Upper graph: dependence of contrail length on ship direction. Lower graph: dependence of contrail semiwidth on ship direction.**



Conclusions

I have offered you some reminiscences and some examples of advancing our understanding of the environment through physics and mathematics. By invoking relevant physics and subjecting it to appropriate mathematical analysis, in the spirit of Bill Priestley, we can ensure that our environmental research sits squarely and seriously in geophysics, not within the soft and slithery option sometimes called 'environmental studies'. I hope that, unlike the audiences of 40 years ago, you agree with me on this.

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