A NOTE ON THE BUSINGER–DYER PROFILES

(Research Note)

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Symbols

K_h eddy thermal diffusivity,
K_m eddy viscosity,

L Obukhov length $\equiv -\dfrac{u_*^3 \overline{T}}{kg\overline{w'T_v'}}$,

where u_* is friction velocity,
T is absolute temperature,
T_v is virtual temperature,
k is von Kármán constant,
g is acceleration due to gravity,

Ri Richard number $\equiv g/\theta \dfrac{\partial\overline{\theta}/\partial z}{(\partial\overline{u}/\partial z)^2}$,

where u is the horizontal velocity component,
z the height above the surface,
θ is potential temperature,

ϕ_m dimensionless wind gradient $\equiv \dfrac{kz}{u_*}\dfrac{\partial\overline{u}}{\partial z}$,

ϕ_h dimensionless temperature gradient $\equiv \dfrac{kz}{u_*}\dfrac{\partial\overline{\theta}}{\partial z}$,

where $\theta_* \equiv -\dfrac{\overline{w'T'}}{u_*}$,

γ a constant ~ 16,
ζ dimensionless height $\equiv z/L$.

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1. Introduction

In the micrometeorological literature, reference is sometimes made to the 'Businger–Dyer Profiles' or the 'Dyer–Businger profiles/relations' without referring to the origin of these relations. For example, in the textbook on 'Atmospheric Turbulence' by Panofsky and Dutton (1984) on p. 134, reference is made to the 'Businger–Dyer formula'. To add to the mystery, these authors refer on p. 141 to the Businger–Dyer–Pandolfo empirical result (Businger, 1966; Pandolfo, 1966) that in unstable air

$$\mathrm{Ri} = \frac{z}{L} \equiv \zeta \,. \tag{1}$$

So it seemed to me that it would be appropriate for this issue of *Boundary-Layer Meteorology* which is dedicated to Arch Dyer, to go back to 1965 and describe the circumstances that led to the above mentioned profiles as I remember them.

2. Aspendale, 1965

In the academic year 1965–1966 I found myself in Australia on a sabbatical leave. The first part of this leave was spent at the CSIRO Division of Meteorological Physics in Aspendale, Victoria. It was in many ways a good choice. The fall is exchanged for spring, SE Australia is a pleasant place to be in spring and summer, and the scientists in the division were hospitable and stimulating. Priestley, Swinbank, Dyer, Webb, McIlroy, Taylor, Clarke, Deacon and several others carried out an active research program.

I intended to work on one specific problem in turbulence, with the hope of solving it. After a few days in the library, I found out that the problem in question had already been solved. Moreover, the solution was so elegant that it made me feel quite humble. On the other hand, this left me completely open on what to do next. This gave me a wonderful sense of freedom to explore whatever struck my fancy. Thus I started to look at the excellent surface-layer data that had been collected in recent years by Swinbank and Dyer near Kerang and Hay.

One sunny afternoon, Bill Swinbank came into my office and showed me the precursor to Figure 1, a good confirmation of the Monin–Obukhov similarity. I agreed that it was an exciting graph on which to meditate. The plot of the Richardson number, Ri versus $\zeta \equiv z/L$ (where L is the Obukhov length) is a basic test of the Monin–Obukhov similarity because Ri contains only information about gradients and ζ contains only information about fluxes. Looking at the graph, it struck me that the relation was close to a one-to-one relation, i.e.,

$$\mathrm{Ri} = \zeta \,. \tag{1}$$

This impression was strengthened by the fact that I believed that ζ was overestimated because u_* was underestimated. Dyer (1967) also comments on the underestimation

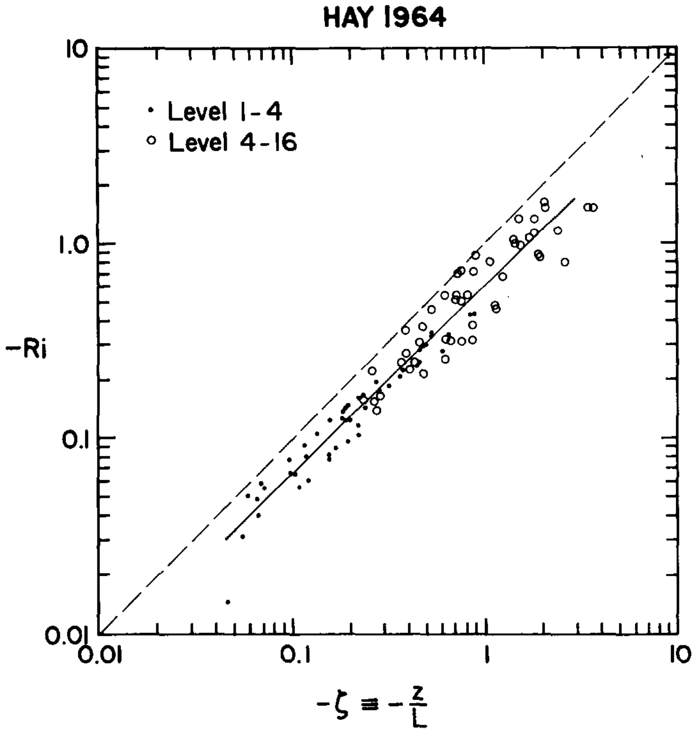

Fig. 1. Ri versus − ζ. Hay observations (after Swinbank, 1968).

of u_*. The reason for this notion stemmed from the fact that the stress had not been measured directly but was derived by applying a drag coefficient to the lowest windspeed measurement in the profile. The drag coefficient was obtained during neutral conditions when the logarithmic profile is valid. Under unstable conditions, the profile is no longer logarithmic but increases less rapidly with height. So if the lowest observed windspeed is an underestimate of the neutral windspeed with the same stress, the neutral drag coefficient applied to the observed wind yields a stress value that is too low and consequently u_* is too low. A 10% error in u_* translates into a 30% error in ζ. Such a correction would bring Swinbank's regression line very close to the one-to-one relation.

Whether or not this reasoning was valid is immaterial at this point. What matters is that it started a speculation on what the consequences would be of Equation (1) for the profile descriptions.

From the definitions of Ri, ζ, ϕ_m, and ϕ_h, we have the identity

$$\mathrm{Ri} = \frac{\phi_h}{\phi_m^2}\,\zeta\,, \tag{2}$$

which combined with (1) leads to

$$\phi_h = \phi_m^2 \tag{3}$$

and furthermore, because $\phi_h/\phi_m = K_h/K_m$ we find that

$$\frac{K_h}{K_m} = \phi_m^{-1} . \tag{4}$$

Using a simple mixing-length model (Fleagle and Businger, 1963 or 1980), I had derived the relation

$$\phi_m = (1 - \gamma \,\mathrm{Ri})^{-1/4} . \tag{5}^*$$

This combined with (1), (3), and (4) leads to the Businger–Dyer profiles:

$$\phi_m = (1 - \gamma\zeta)^{-1/4} , \tag{6}$$

$$\phi_h - (1 - \gamma\zeta)^{-1/2} , \tag{7}$$

and

$$\frac{K_h}{K_m} = (1 - \gamma\zeta)^{1/4} . \tag{8}$$

The result of (6)–(8) was quite exciting to me, so I showed it around. First I showed it to Swinbank. He was not very interested in this new profile formulation because he had just published the 'exponential profile' (Swinbank, 1964) based on an elegant derivation; however, the fit to the data was poor. Then I went to Arch Dyer and much to my surprise he had written the same Equations (6)–(8) on the blackboard in his office. Apparently he had been intrigued by the same plot of Ri versus ζ and came to the same conclusion. In fact, in a 1964 paper, he had analyzed heat flux data and found the same power law as (7) indicates. So, it is possible that his reasoning went from (1), (2) and (7) to (6) and (8). I regret not having asked him this question at the time.

Arch and I discussed briefly the idea of putting these profiles in a joint paper and publishing them. Unfortunately, Bill Swinbank was unhappy about competing profiles and felt that the staff of the Division should show some 'loyalty' and not prematurely publish the above results. Consequently Arch refrained from working on a joint paper

* Equation (5) combined with (2) leads to the KEYPS or O'KEYPS function (Panofsky, 1963; Businger and Yaglom, 1971), i.e.,

$$\phi_m^4 - \gamma'\zeta\phi_m^3 = 1 ,$$

where

$$\gamma' = \gamma \frac{K_h}{K_m} .$$

This function therefore requires thatK_h/K_m = constant. O'KEYPS stands for the initials of the various authors who derived this equation independently.

and I published the profiles as part of the proceedings of a meeting organized by the Rand Corporation (Businger, 1966). In a completely independent study, Pandolfo (1966) also arrived at (1).

3. How Good is the Assumption Ri = ζ?

It is of interest to see how well the assumption (1) has been verified by later experiments. Dyer and Hicks (1970) reported results of experiments at Hay (New South Wales) described by Swinbank and Dyer (1967) and at Gurley (New South Wales). In these experiments, the momentum flux was measured directly with the 'fluxatron' (Dyer *et al.*, 1967; Hicks, 1970). The results presented in Figure 2 suggest that (1) fits the data very well, indeed.

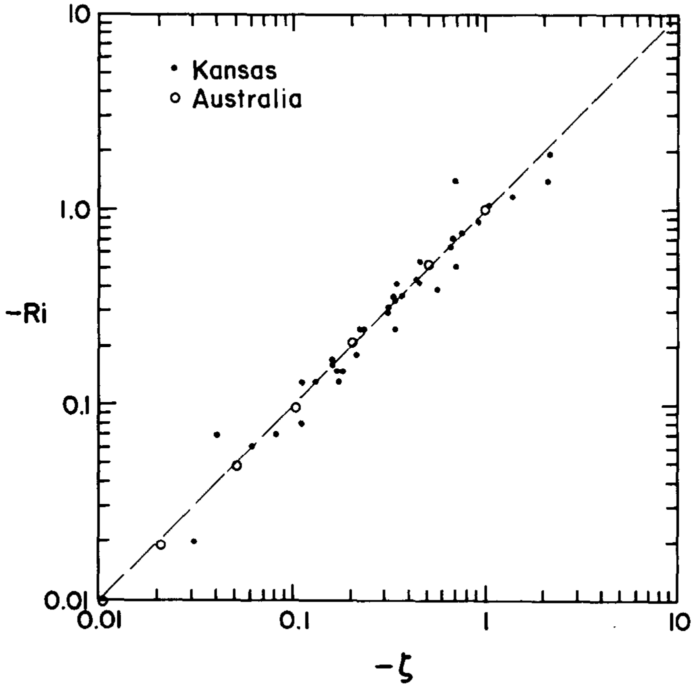

Fig. 2. As Figure 1 for Kansas observations. ● (Izumi, 1971), and Australian observations, ○ (Dyer and Hicks, 1970).

In the same period, the Boundary Layer Group of AFCRL (Air Force Cambridge Research Laboratories) carried out an experiment in Kansas. Again fluxes and profiles were measured independently. Results concerning (1) have been reported by Businger *et al.* (1971) and Izumi (1971). These results are also presented in Figure 2, again suggesting that (1) is a good assumption. The scatter is greater in this set because individual runs are shown, rather than the average of a series of runs.

The two data sets differ in several ways. The fluxatron used by Dyer and Hicks has a lower response time than the sonic anemometer used in Kansas and therefore a correction to the co-spectrum is needed. In the Kansas experiment no vapor flux was measured; therefore ζ was slightly underestimated. The von Kármán constant, k, that fit the Australian data best was 0.41, whereas the Kansas data suggested $k = 0.35$. Thus, in some sense the agreement between the two data sets in Figure 2 must be considered fortuitous.

More recent efforts to look at the relation $\mathrm{Ri} = f(\zeta)$ show a continuation of the trend between Figures (1) and (2). Dyer and Bradley (1982) give an analysis of the ITCE (International Turbulence Comparison Experiment) data taken in 1976. The result is shown in Figure 3. We see that in this case the relation is to the left of the one-to-one line, almost as much as Swinbank's (Figure 1) is to the right of this line. Webb (1982) reanalyzed the old data taken before 1965. Surprisingly this analysis gives almost the same $\mathrm{Ri} = f(\zeta)$ function as Dyer and Bradley find; see Figure 3. From these figures, we see that over the last 20 years $-$ Ri has steadily increased versus $-\zeta$. Is this a continuing trend, or is it part of an oscillation? As it stands, the differences between the various data sets and the analyses are still too large to feel confident of the results. Mortensen (personal communication) recently showed me preliminary results from a large data set taken in Denmark. These preliminary results agree with the analysis of Dyer and Bradley (1982).

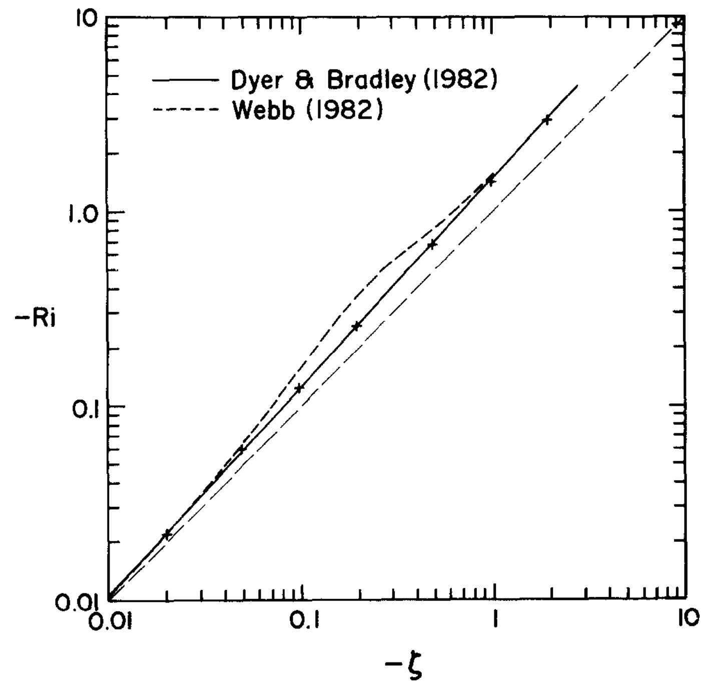

Fig. 3. As Figure 1 + according to Dyer and Bradley (1982) and dashed line according to Webb (1982).

To settle this and other issues on the structure of the atmospheric surface layer, more and better experimental data are needed. To paraphrase Dyer and Bradley (1982): "The enormous data scatter which is typical of this type of experiment points to the need for large amounts of data in order to obtain a statistically significant result... The evidence is mounting that the atmosphere does not follow the averaged laws at all places and at all times, even over an excellent site..." Mesoscale structures imbedded in the planetary boundary layer may, to a large extent, contribute to this large scatter. Therefore, the next generation of surface-layer experiments should be conducted in a setting where adequate documentation of the entire boundary layer is available.

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