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DATA INTERPRETATION FOR MODEL DEVELOPMENT AND VALIDATION

Workshop on the Parameterization of Mixed Layer Diffusion
October 20-23, 1981

Hosted by:

Physical Science Laboratory
New Mexico State University
Las Cruces, New Mexico

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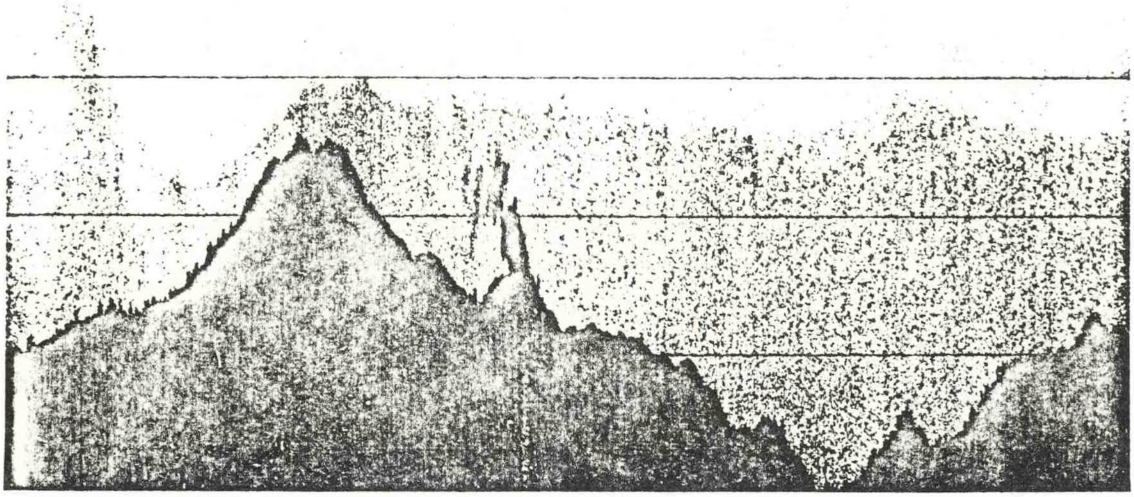

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# WORKSHOP ON THE PARAMETERIZATION OF MIXED LAYER DIFFUSION



20 - 23 OCTOBER 1981

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New Mexico State University
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# DATA INTERPRETATION FOR MODEL DEVELOPMENT AND VALIDATION

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## 1. INTRODUCTION

Consider a hypothetical experiment in which arcs of samplers are set up downwind of a fixed tracer release point. Let us suppose that all of the quantities that are important are measured. Anemometers measuring the wind velocity u are supported by covariance measurements of the friction velocity $u_*$. The sensible heat flux H is also measured by covariance, so that the Monin-Obukhov scale length $L = \rho c_p u_*^3 \theta / kgH$ is determined directly. Acoustic sounders (or lidar, etc.) are used to determine the depth of the mixed layer, $z_i$, so that the PBL convective scale velocity $w_* = (gHz_i/\rho c_p \theta)^{1/3}$ can also be evaluated. (Here, as elsewhere, notation is conventional: $\rho$ is air density, g is the acceleration due to gravity, $c_p$ is the specific heat of air at constant pressure, k = 0.4 is the von Karman constant, $\theta$ is absolute potential temperature, etc.).

In an experiment of this sort, ground-level sampling lets us investigate the surface stucture of the "plume", usually characterized by Gaussian-equivalent standard deviations $\sigma_y$ laterally and $\sigma_z$ vertically. The central question to be addressed by field experiments is how quantities like $\sigma_y$ and $\sigma_z$ vary with external parameters. Depending on the philosophy of the investigator and the intent of the study, velocity standard deviations $\sigma_u$, $\sigma_v$, $\sigma_w$ (longitudinal, lateral, and vertical, respectively) are sometimes considered to be sufficiently "external". Other workers consider these to be a more intrinsic part of the problem, i.e. to be deduced from other data as stepping stones to the desired result. Whatever the experimental philosophy, the common intent is to describe plume behavior in the most accurate manner, and in terms of available data. Sometimes voluminous information is available, but usually it is not. Likewise, the quality of data obtained is frequently insufficient to answer all of the questions that should be asked. However, if analysis is not carried out with care, answers might seem to be obtained which in reality are not proper solutions.

In the case of the field study hypothesized here, we might evaluate $\sigma_y$ and attempt to relate it to a measured quantity $\sigma_v$ via

$$\sigma_y = \sigma_v \cdot (x/\bar{u}) \cdot f(z, z_i, x, L, \text{etc.}) \quad (1)$$

where f is some function of controlling variables. The function f is intended to represent relatively minor variations, after first-order influences of x, $\bar{u}$, and crosswind turbulence $\sigma_v$ are accounted for by the preceding terms on the right hand side of (1). If we suspect that f will vary primarily with distance downwind, but that stability will also be a controlling property, we might wish to investigate the dependence of f, evaluated as

$$f = (\bar{u}/x) \cdot (\sigma_y/\sigma_v) \ , \quad (2)$$

on the dimensionless property x/L . There is obviously a problem in adopting this philosophy, since the quantity x is included as a major factor in both the evaluation of f and x/L . The variability introduced by the range of x over which the experiment was performed will lead to the appearance of a relationship that is not physical; when plotted on log-log paper, a slope of -1 would be expected solely as a consequence of f increasing as x decreases, and x/L decreasing.

## 2. SOME THEORETICAL BACKGROUND

There is little new in the arguments given above; the problem is an old one, yet it seems to have been overlooked in a number of analyses. It is possible to determine the sensitivity of different parameterization procedures to "counterfeit" results. Suppose we believe that raw data should be combined to form dimensionless properties $Y_1$ and $Y_2$, which can be written as

$$Y_1 = x_1 \cdot x_3^{\,a}$$
$$Y_2 = x_2 \cdot x_3^{\,b} \quad (3)$$

where $x_{1,2,3}$ are themselves combinations of raw observations. If the ranges of these variables are $D_{1,2,3}$, and if the distributions of observations within these ranges are the same, then it can be shown that a plot of $\ln(Y_1)$ versus $\ln(Y_2)$ will have a slope of

$$c = ab \cdot D_3^{\,2}/(D_2^{\,2} + b^2 \cdot D_3^{\,2}) \qquad (4)$$

when none of the $x_{1,2,3}$ are related (see Hicks, 1978). Equation (4) results from the application of conventional regression equations to the logarithms of the compound variables; the ranges $D_{1,2,3}$ are accordingly those of the logarithms of the quantities $x_{1,2,3}$ rather than of the quantities themselves.

Briggs and McDonald (1978) present a convenient tabulation of the results of a regression anlysis of dispersion data, as discussed here. Table 1 is based on their data. Three examples of regression analyses are selected for illustration; other possible examples are difficult to consider because ranges of the original data cannot be easily deduced from the published material. In every case, stable and unstable stratifications are considered separately, and the "near-field: (i.e., close to the origin in a graphical representation) and "far-field" distinctions applied by the original authors have been retained. Symbols are as used by the original authors: h is a measure of vertical dispersion, calculated from ground-level tracer concentration $\chi$, tracer release rate Q, and wind speed u, by applying

$$h = Q/(u \cdot \int \chi \, dy); \qquad (5)$$

x is downwind distance, and $\Delta\theta$ is the potential temperature gradient near the surface.

Without considering the effects of variable interaction, the "near" and "far" exponents determined by Briggs and McDonald would seem rather satisfying. In most cases, exponents are found to be near unity. However, the value of the exponent imposed by the method of analysis is also close to unity, and hence the experimental regression results are not as interesting as they first appear.

Correlation coefficients are also listed in Table 1. These are derived using a relationship similar to Equation (4):

$$r = ab \cdot D_3^{\,2}/((D_1^{\,2}+a^2D_3^{\,2}) \cdot (D_2^{\,2}+b^2D_3^{\,2}))^{\frac{1}{2}} \qquad (6)$$

(q.v. Hicks, 1978). The tabulated values all appear to be highly significant - about 30 sets of observations contribute to each case. The blind application of the concept of statistical significance would therefore lead to the conclusion that strong dependencies exist, which is not a correct judgement. Indeed, close inspection of Table 1 leads to the suspicion that only the "far-field" data in unstable conditions display evidence of a departure from random behavior.

## 3. TURBULENCE STATISTICS IN THE SURFACE BOUNDARY LAYER

It is obvious that the problems of analysis described above will be of greatest concern when experimental values are small and when resulting dimensionless quantities are therefore subject to considerable error. In practice, the relevance to atmospheric turbulence and its interpretation is largely confined to situations in which winds are light. In strong winds, most velocity components are large and statistical quantities like $\sigma_u/u_*$, $\sigma_v/u_*$, and $\sigma_w/u_*$ are fairly well determined. Figure 1 shows how these quantities (and the analogous ratio $\sigma_T/T_*$) appear to vary with atmospheric stability in the surface boundary layer in daytime. Each of the lines drawn is a published relationship, but the data that the lines appear to represent quite well are not real. In fact, these "data" are random numbers representing raw "observations" of velocity and temperature standard deviations, friction velocity, and sensible heat flux. These "data" have been manipulated as if they were real, to produce the averages and standard errors that are plotted. Details of how the surrogate data set was constructed are not critical; it is sufficient to note that the major constraint is that the values have the correct relationships on the average, and that the ranges of "observations" are representative of an experiment such as the Kansas micrometeorological study of 1968 (Izumi, 1971). It must be emphasized that there is no physically valid pattern behind the behavior of the average values plotted in Figure 1; the apparent dependencies on stability are consequences of the way the compound variables are structured and not of any feature of the atmosphere.

The question then arises as to the extent to which our knowledge of atmospheric turbulence has been affected by such contamination. The "data" used to produce Figure 1 were designed to simulate the 1968 Kansas field experiment. Figure 2 shows the actual observations obtained at Kansas, plotted in a rather unconventional manner against gradient Richardson number in order to avoid the possibility of compound-variable contamination. Several points are immediately obvious. In particular, the real data indicate that $\sigma_u$ and $\sigma_v$ approach each other as instability increases, and become indistinguishable at about Ri = -0.4. Beyond this instability, the quantities $\sigma_u/u_*$ and $\sigma_v/u_*$ maintain a fairly constant value, at about 3.0. (On the other hand, the effect of contamination evident in Figure 1 is to cause the appearance of a continuing increase

Table 1. Comparisons between regression analyses of results obtained in the "Prairie Grass" experiment (Haugen, 1959) and expectations based on statistical theory, assuming no relationship between the basic quantities involved. Anticipated slopes of power law "relationships" are derived directly from the data ranges $D_{1,2,3}$, using Equation (4); correlation coefficients r are obtained using Equation (6). Experimental ("actual") exponents are as tabulated by Briggs and McDonald (1978). Results obtained in stable stratification are indicated by S, unstable by U.

| Regression form | Variables $x_1$ | $x_2$ | $x_3$ | Ranges* $D_1$ | $D_2$ | $D_3$ | Anticipated Exponent | Actual Exponent "Near" | "Far" | r |
|---|---|---|---|---|---|---|---|---|---|---|
| $h/L$ on $u_*X/uL$ | h | $u_*X/u$ | $1/L$ | S:2.9 | 2.1 | 5.8 | 0.88 | 0.88 | 1.01 | 0.84 |
| | | | | U:3.8 | 2.1 | 4.1 | 0.79 | 1.02 | 1.83 | 0.65 |
| $h/L$ on $X/L$ | h | X | $1/L$ | S:2.9 | 2.1 | 5.8 | 0.88 | 0.84 | 0.68 | 0.84 |
| | | | | U:3.8 | 2.1 | 4.1 | 0.79 | 1.11 | 2.00 | 0.65 |
| $h\Delta\theta/u^2$ on $X\Delta\theta/u^2$ | h | X | $\Delta\theta/u^2$ | S:2.9 | 2.1 | 4.5 | 0.83 | 0.85 | 0.59 | 0.76 |
| | | | | U:3.8 | 2.1 | 3.4 | 0.73 | 1.10 | 2.01 | 0.57 |

* Ranges are quoted as natural logarithms; i.e. a variable that ranges from values of 4 to 200 will be characterized by a value ln(200/4) = 3.9.

in both quantities, which seem to remain separated). For $\sigma_w/u_*$, a continuing increase with increasing instability appears to be supported by the real data, much in the same way as in Figure 1 but with a slightly different neutral intercept; the line though the "data" of Figure 1 is shown as the dashed line in Figure 2.

Figure 3 shows the convergence of $\sigma_u$ and $\sigma_v$ more clearly, by plotting their ratio as a function of gradient Richardson number. Once again, approximate equality of these two velocity statistics is indicated for Ri less than about -0.4. The apparent trend for the ratio to become less than unity in highly unstable conditions seems to be contrary to physical expectations; the cause probably lies in some feature of the original data, perhaps related to some small error in the measurement of longitudinal velocity.

There is no evidence that $\sigma_w$ is similarly limited; in this case a continuing increase with increasing stability is an intuitively attractive proposition.

## 4. EXTENSION TO THE MIXED LAYER

While it is clearly valid to scale turbulence quantities like $\sigma_u$, $\sigma_v$ and $\sigma_w$ by the friction velocity in the surface boundary layer, extrapolation to greater heights is not an obvious matter. In the contemporary literature, the convective velocity scale $w_* \equiv (gHz_i/\rho c_p\theta)^{1/3}$ (Deardorff, 1970; Tennekes, 1970) is frequently used in direct substitution for $u_*$ in scaling considerations. Likewise, the planetary boundary layer (PBL) temperature scale $\theta_* \equiv H/\rho c_p w_*$ is often used to scale temperature fluctuations, in place

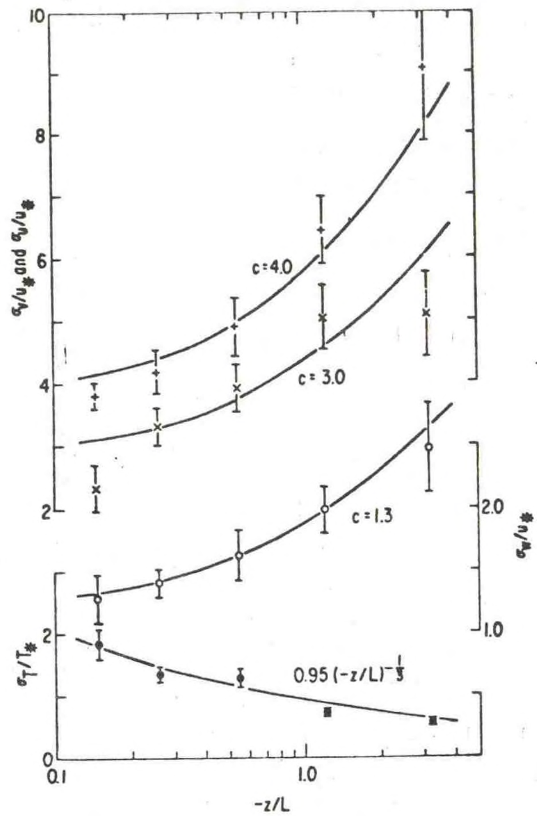


Figure 1: An analysis of random "values" of velocity and temperature standard deviations, sensible heat fluxes, and friction velocities, normalized and plotted as non-dimensional quantities in the usual fashion. Ranges of observations are selected to be the same as was experienced during the 1968 Kansas field experiment (Izumi, 1971). Lines drawn through averages and standard error bars are published relationships; for the velocity cases, these relationships are of the form $\sigma/u_* = c(1 - a \cdot z/L)^{1/3}$, with a = 3.0.

of the more familiar surface property $T_* \equiv H/\rho c_p u_*$. It now seems accepted that height within the mixed layer should be scaled according to its depth, but there continues to be some disagreement about the selection of an appropriate stability index. Whereas some workers express their results in terms of the Obukhov scale length via the ratio $z_i/L$, others prefer to extend surface boundary layer relations by simply continuing to use z/L, and still others employ the parameter $u_*/fL$ in which a role for the Coriolis parameter f is envisaged.

The PBL data sets obtained over water by Warner (1972, 1973) and over land by Izumi and Caughey (1976) provide sufficient information to test the applicability of the various stability parameterizations, to look for possible areas of interference by noise, and in particular to search for evidence of a significant height dependence of turbulence statistics above the constant-flux surface layer. The data of Warner were obtained by aircraft over the Coral Sea (16.5°S, 145.4°E); effects of water vapor buoyancy should therefore be anticipated and hence the following analysis uses virtual temperatures and virtual heat fluxes. The data of Izumi and Caughey were obtained by means of captive balloons, over flat land in Minnesota (48.5°N, 96.9°W). In this latter case, insufficient humidity data are available to permit correction for water vapor buoyancy effects; however corresponding corrections should have been small.

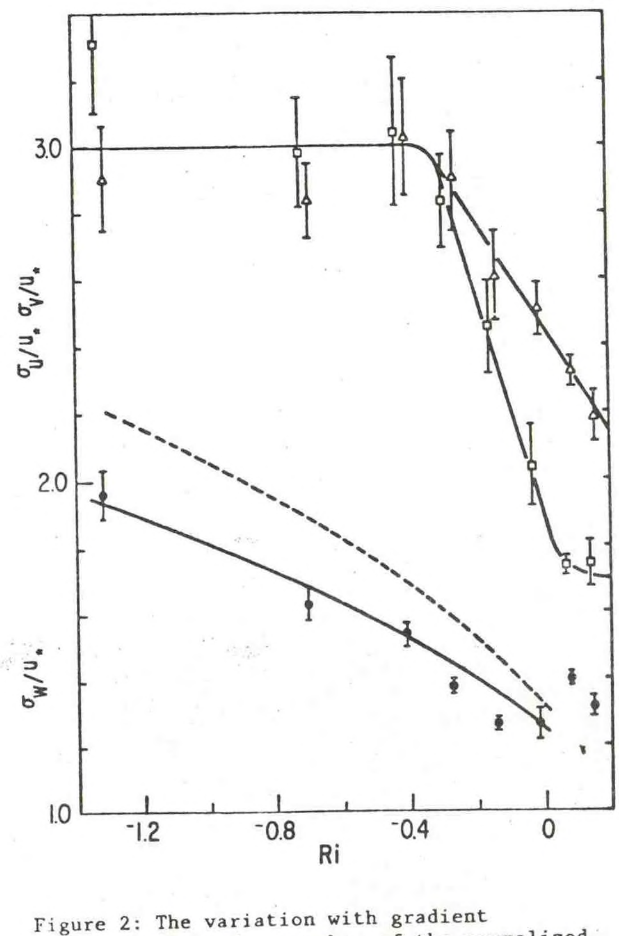


Figure 2: The variation with gradient Richardson number of the normalized velocity standard deviations observed during the 1968 Kansas study. The dashed line is the $\sigma_w/u_*$ line drawn through the artificial "data" of Figure 1.

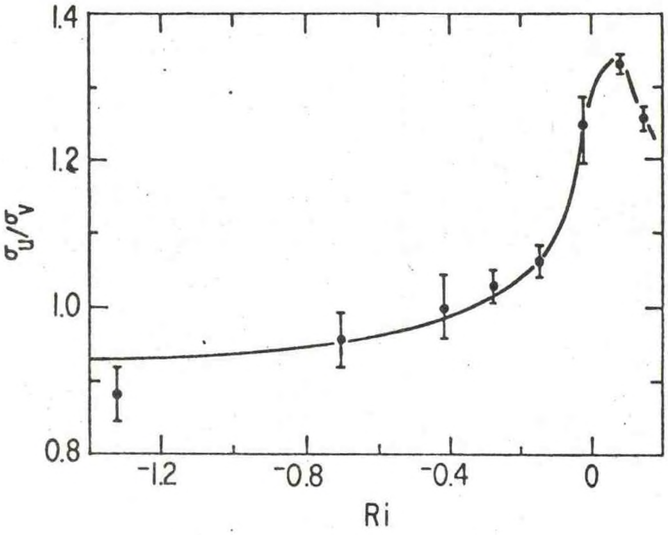


Figure 3: The variation with gradient Richardson number of the "shape ratio" $\sigma_u/\sigma_v$, derived from the 1968 Kansas field experiment data set.

Data obtained in the surface boundary layer proper will necessarily contaminate the PBL results if they are included in the analysis. Figure 4, which is based on the Minnesota data set, shows that selection of an appropriate height range over which to investigate PBL turbulence is not too critical. The diagram was constructed by normalizing each profile of $\sigma_w$, $\sigma_u$, $\sigma_v$, and $\sigma_T$ according to observation made at 4 m height, a process that should not introduce contamination due to other meteorological factors. There is little evidence for a strong height dependence of any of the plotted variables, over the height range of the diagram. The residual variation seems to be somewhat similar to predictions of laboratory simulations (q.v. Willis and Deardorff, 1974). On the whole, the figure indicates that $\sigma_u$ and $\sigma_v$ are fairly constant with height above 4 m in unstable conditions, that $\sigma_w$ increases until it attains a value about 2.5 times the 4 m value, and that $\sigma_T$ decreases until it reaches a value about 20% of that at 4 m. All of these quantities reach these fairly constant levels within the lowest 10% of the mixed layer depth.

Having seen that there is relatively little height variation of any of the velocity statistics through most of the mixed layer, it is of interest to investigate how alternative methods of analysis might cloud the issue. The following analysis will employ data obtained over the height interval $0.1 < z/z_i < 0.9$, so as to be above the surface boundary layer while at the same time below the level of direct influence of the top of the mixed layer.

Table 2 lists the results of a set of correlation analyses, using the Coral Sea and Minnesota data separately. All of the correlation coefficients between compound variables are significant, at the 99% level or better except in one case. However, it is incorrect to interpret high correlations with quantities like z/L as implying a height dependence, since the correlation with height alone is always low. Likewise it is not appropriate to interpret high correlations with $z_i/L$ as meaning a significant variation with $z_i$ alone, since these individual correlations are also much lower. Instead, the high correlations that typify the relationships between all compound variables considered in Table 1 are due to the way these variables are structured.

Figure 5 illustrates the problem graphically. The left hand side of the diagram presents observations of $\sigma_u/u_*$ as a function of the stability index $z_i/L$, both of which quantities depend on the friction velocity which is usually not well determined. The right hand side of Figure 5 shows a plot derived from the same raw data, but after the friction velocities have been randomized. The line drawn is a published expression intended to describe the dependence; it is seen to be a good description of the randomized behavior as well. In reality, it appears that the line drawn is more an artifact of the analysis than a meaningful representation of natural behavior; indeed it is evident that the values of $\sigma_u/u_*$ often exceed the value 3 - 4, which was earlier found to be a limit for the case of $\sigma_u/u_*$ in the surface boundary layer (see Figure 2).

## 5. CONCLUSIONS

There seems little doubt that much of what we believe to be true about turbulence in the lower atmosphere has been contaminated by spurious results generated by the method of data analysis. Obviously, care should be taken to guard against the possibility of biasing the interpretation of observations by applying a poorly-selected method of data reduction. Special care should be taken when normalizing one set of experimental data by another; this procedure will compound errors and will cause a significant cross-contamination in many instances. To test

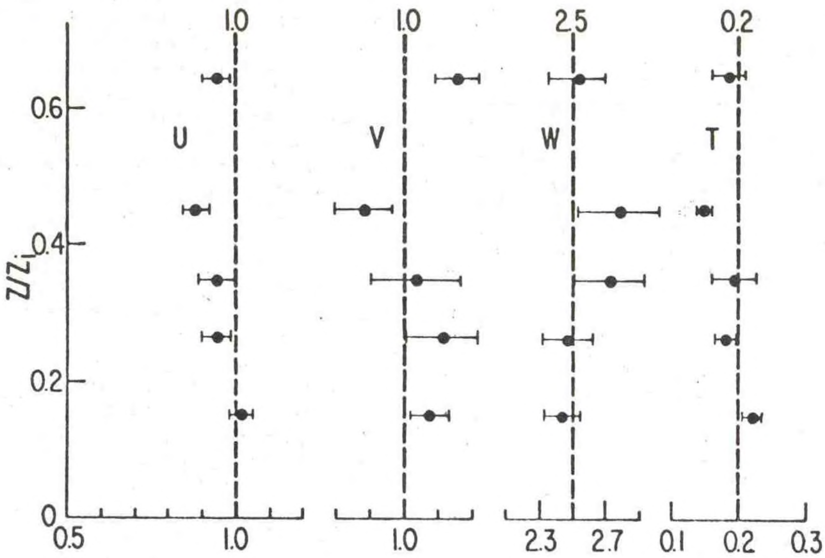


Figure 4: Variation with height in the mixed layer of the standard deviations $\sigma_u$, $\sigma_v$, $\sigma_w$ and $\sigma_T$, after normalization according to simultaneous measurements made a 4 m height. Data are derived from Izumi and Caughey (1976). Averages and standard error bars are plotted.

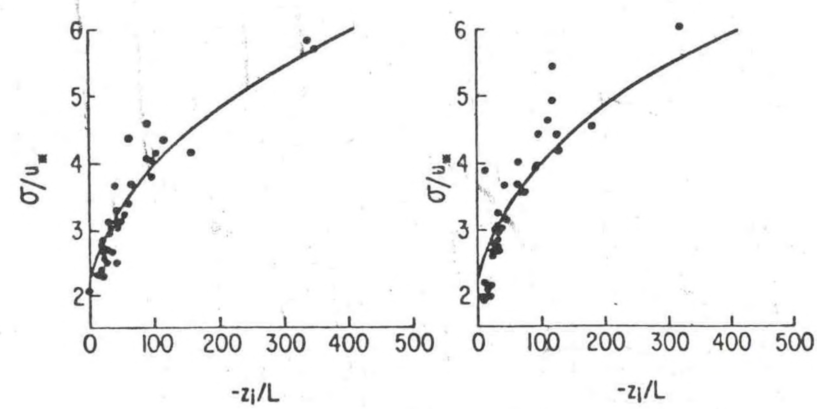


Figure 5: Plots of normalized velocity standard deviations (horizontal wind speed) against the PBL stability parameter $z_i/L$. The methods of handling the data are identical, but the right hand side makes use of randomized friction velocities.

whether the interpretation of experimental observations has been affected, it is useful to repeat the analysis with a second set of values, derived from the first by the simple expedient of randomizing some selected, critical component. In practice, the most logical variable to select for this purpose is usually the friction velocity or the sensible heat flux. If the randomized data set leads to the same sort of interpretation and conclusions as the original analysis, then we should question strongly whether any real understanding of natural phenomena has been developed.

Table 2. Correlation coefficients, obtained by analysis of the standard deviations and supporting data published by Warner (1972 and 1973) and Izumi and Caughey (1976). The former data set (Coral Sea) provides 23 sets of observations, the latter (Minnesota) provides 41. Data obtained in either the top 10% or the bottom 10% of the mixed layer are excluded.

| Variable Pair | Coral Sea | Minnesota |
|---|---|---|
| $\sigma_u$ on $z_i$ | 0.16 | 0.66 |
| $\sigma_u$ on $z$ | 0.22 | 0.40 |
| $\sigma_u/u_*$ on $z/L$ | 0.87 | 0.75 |
| $\sigma_u/w_*$ on $z/L$ | -0.56 | -0.95 |
| $\sigma_u/u_*$ on $z_i/L$ | 0.89 | 0.94 |
| $\sigma_u/w_*$ on $z_i/L$ | -0.72 | -0.88 |
| $\sigma_w$ on $z_i$ | 0.44 | 0.37 |
| $\sigma_w$ on $z$ | 0.40 | 0.37 |
| $\sigma_w/u_*$ on $z/L$ | 0.93 | 0.95 |
| $\sigma_w/w_*$ on $z/L$ | -0.68 | -0.63 |
| $\sigma_w/u_*$ on $z_i/L$ | 0.97 | 0.99 |
| $\sigma_w/w_*$ on $z_i/L$ | -0.87 | -0.90 |
| $\sigma_T$ on $z_i$ | -0.38 | -0.15 |
| $\sigma_T$ on $z$ | -0.26 | -0.15 |
| $\sigma_T/T_*$ on $z/L$ | -0.95 | -0.97 |
| $\sigma_T/\theta_*$ on $z/L$ | -0.91 | -0.78 |
| $\sigma_T/T_*$ on $z_i/L$ | -0.97 | -0.93 |
| $\sigma_T/\theta_*$ on $z_i/L$ | -0.80 | -0.53 |

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NOTE: QUESTIONS/ANSWERS TO B. HICKS ON NEXT PAGE.

QUESTIONS TO DR. HICKS

T. Yamada: Dr. Hicks, you have shown one picture which shows the sigma U divided by sigma V. In the stable side, I guess you had curves decreasing from a maximum at neutral condition. In the stable layer you have wind shear production which is the only production term in the turbulence equation. I suppose that turbulent energy is fed first in the U component, so I would expect the sigma U divided by sigma V must be larger than one. Therefore, I suppose the curve should increase on the stable side. I would like to hear your comment.

Bruce Hicks: I have got to agree, I think. I would have expected the same thing. What you saw was cold data, not cleaned up in any way. I think I do agree with you.

S. P. Arya: So what do you suggest? The plot is data without non dimensionalizations or that you cannot compare one observation with that taken anywhere else?

Bruce Hicks: The purpose of normalizing anything is to take a first cut at reducing the variance trying to pull it down in the same ball park as something else which you are trying to compare it against. That is fair game. I have no problem with that. But, once you do this, let us then compare the values you get with a prediction or some other data set in a manner so that you are not going to impose a result.

There are several ways of tapping the problem. One is to simply look at the theory and see what sort of consequences he would expect had there been no relationship in the first place. That can be done. The second thing you could do, and that is my plea, is simply randomize one of the critical data sets and pour it through the analysis again. If you reproduce the same figure, you have got trouble.

Willy Sadeh: I would really like to commend you for your comment about normalization because it is not just in this type of data but also in wind tunnel data. What is at stake here, I guess, would normalize to what should be the reference scale. Your last comment in answering to Dr. Arya, probably the best one, is that you must give some theory or some idea to know what should be the physical quantity to choose the reference scale.

I would also like to say that most of this type of data that you presented are essentially the realization of a random process. I think by and large most of us do not perform any check for the statistical stationarity of the data. This is a very important point. There are some checks available in the literature on how to look at this type of data and list the term. Also, to what extent are we faced by a stochastic process that we cannot define as being statistically stationary. Otherwise, if it is not, when you measured the second time you will not get the same results. This is a point I would like to hear comments about.

Furthermore, notice that we do not check for statistical stationarity. I guess we are using an assumption which is highly questionable. We essentially take one set of data and then draw a conclusion. What we imply in principal is to have an ergodic process which is even more questionable as to what extent it is true.

In regard to these two points, we should look very careful at what extent we can pick up one single set of data, crank our some correlation and draw far-reaching conclusions.

Bruce Hicks: To the first point, we have heard a lot of emphasis on the question of variability. In fact, what is happening is that in many instances our interpretation of the mean performance of models is being influenced by the variability in the data sets that the models are using and are addressing. Variability is tied up. It is not a separate issue. It is right in there at the same time as everything else.

Regarding the second point, I want to think about that.

Donald Walters: You made a point that I have been observing particularly with a lot of atmospheric turbulence data when you said earlier on that one of the points you would not want to address. For example, the standard deviation going negative. What you have done is chosen the wrong coordinate system. The way I look at it, and this is particularly true for example, structure function data like turbulence $C_T^2$ data, look at the probability distribution. If it is logged normal, then choose the log coordinates that do your averaging in. Do not do it in linear coordinates. It will converge most quickly to the proper means in the proper coordinates that the data is mostly Gaussian. Then, transform back if you have to. If you are in the wrong coordinate system, spikes, noises and things can throw it way out, particularly a log normal compared to a Gaussian. One noise pulse or a large, but infrequent, data value can throw a calculated mean way off. It is easy for the standard deviation to be large enough to drive the mean negative if log normal data is treated as Gaussian. You have got to choose the proper coordinate system to do your averaging in. That is almost never addressed.

Bruce Hicks: Yes, because the basic rule of thumb in the old adage that if you are talking about the ratio of too poorly determined quantities, then as a first rule, it has got to be logged normal and you better do everything using logrithms.

Allen Weber: I either misunderstood you or do not agree with your conclusions. In regard to the diagram, I thought I heard you say that it had a high correlation coefficient?

Bruce Hicks: Statistially significant.

Allen Webber: I do not see how that could be.

Bruce Hicks: It did not reproduce well in here, I must admit. But, the correlation coefficient on that came out to be about .4 or something of that order.

Allen Weber: But at what probability level?

Bruce Hicks: In answer to your question, what I mean by statistical significance is significant at probably the 95% level. I am trying to put a half finger on it because I believe they are around about 40 data points in that. So, the error on the correlation coefficient would be on the order of $1/\sqrt{.4}$ which would be around about .2 or less which means you are around about the 95% level.

Allen Weber: OK, that is hard for me to believe. I have been working with a number of data sets and model validation for the Savannah River Lab Krypton 85 data set and I just cannot quite accept those numbers. The other statements you made that I cannot agree with is that you cannot do model validation by plotting measured concentration against predicted concentration of a model. Why not?

Bruce Hicks: I hope I did not leave that impression. What I am saying is that if you present matters in a normalized manner, if you normalize the concentrations, and normalize it in a manner so that you will share in some quantity, then you are likely to be in some trouble. I do not want to get too far away from the question about statistical significance because I do not want to leave the thought in your minds that it was hardly significant. It is not. That was just an example where if I had found that correlation coefficient coming out, I would have looked further.

Allen Weber: Was that the correlation coefficient or the $R^2$ value? I could believe in R of .4 but not an $R^2$.

Bruce Hicks: Oh, correlation coefficient.

Allen Weber: Well, that means your $R^2$ is .16, which I could believe. But, I could not believe in $R^2$.

Bruce Hicks: Yes, now for a significant sums using F-test table.

Allen Weber: I want to make another comment on the remark about doing things in the logrithmic frame of reference. I still believe it is valuable to do things in a linear fashion because a lot of the quantities that we are worried about, such as concentration, are important as to their effects in a linear fashion. By taking logrithms of these variables then doing statistical manipulations presenting them to the people that have to make decisions, we are in effect, . . . . . . (Balance of comment not properly documented.)