Reply

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I was very happy to learn that there exists now a number of observations which are so complete that the theories can be tested, and it was a pleasant surprise for me to see the tests Dr. Lettau made. Nevertheless, I should like to make some remarks on Dr. Lettau's note.

The main point in the note is the comparison of the two theories with observations. As his fig. 1 shows, this comparison is obviously in favor of Dr. Lettau's theory. However, it seems to me that the comparison is not entirely fair. The theoretical model of the surface layer I suggested was kept as simple as possible. For sake of clarity, unknown empirical factors were

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assumed to be equal to unity. This was the case with the ratio K_h/K_m and the factor β in the Rossby and Montgomery approach (see table 1 of my original article), it being borne in mind, however, that it might be necessary to introduce these factors to obtain an adaptation of the theory to observations. However, in the first place, observations were required. From the data Dr. Lettau uses, it seems likely that an adaptation must be made. The most simple adaptation is obtained when the last term of (18) of my article is multiplied by a factor r', similar to the factor β of Rossby and Montgomery. The factor is given the symbol r' here, because it is used in the same way as Dr. Lettau uses r. Giving r' the value 1.6, one obtains nearly the same curve by my theory as by Dr. Lettau's. If one does this, there is from an observational point of view, as far as the O'Neill data are concerned (which I do not yet have at my disposal), no preference for either of the theories. From a theoretical point of view, my modification of Dr. Lettau's theory is a real simplification without loss of merit. From the preceding, it is obvious that the small difference between the two theories is not challenged by Dr. Lettau's note.

In his note, Dr. Lettau further states that I objected against the introduction of the semi-empirical parameter r. However, this must have been a misunderstanding, because I did not know of the parameter r at the time. But, after reading Dr. Lettau's 1952 paper, indeed I should like to object against the introduction of r. It is beyond the scope of this reply to go into detail on this subject, so I will observe only that it must be expected that r is dependent not only on the Richardson number but also on the roughness factor z_0. The constant factor r' does not necessarily involve the ratio K_h/K_m, but may be a similarity factor between frictional and convective turbulence. In any case, the point of adaptation of the theories requires further theoretical investigation.

The representation of the theoretical results in a diagram of the relative curvature of the wind profile β against the Richardson number Ri is nice and comprehensive, and probably gives the easiest way of checking the theories. However, it is not easy to construct from the β-Ri curve the wind profiles; neither is it easy to obtain a survey of the different states of the surface layer. This surveyability is obtained by use of the stability number S_n — which has a clear relation to Ri, and which can be regarded as a stability parameter independent of height [see (23) of my article] — *versus* the dimensionless height $\zeta = (z + z_0)/z_0$ (see fig. 1 of my article). The curves drawn in my diagram probably must be corrected; but they give an impression of the possible form of the structure of the atmospheric surface layer.

Finally, I should like to state that I am sorry for the misunderstanding about the concept of the mixing length in the discussion between Dr. Inoue and Dr. Lettau, and that I am glad that Dr. Lettau and I agree on this point.