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Interactive comment on "Temperature profiles, plumes and spectra in the surface layers of convective atmospheric boundary layers" *by* Keith G. McNaughton and Subharthi Chowdhuri

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1 Overview

We thank the reviewer for his careful reading of our MS and his helpful comments. He has identified a number of places where our assumptions have not been made explicit or where our explanations are inadequate or obscure. We have attended to these matters. Making these amendments has not changed the underlying arguments in our paper.

The reviewer has also expressed a more general reservation, and it concerns the nature of our paper. Our objective is stated in the last paragraph of the introduction. It is to extend a conceptual model that can explain the empirical scaling results reported in this and previous papers. The 2T model is important because it helps us define what we mean by ‘plumes’. However, it is only a Toy model, designed to clarify concepts rather than provide a basis for computation. We provide an exact definition of a plume for an idealized flow where plume boundaries are sharp and then say "real plumes are something like that". Importantly, we find that the idea of the cross-sectional areas of plumes is likely to have some validity, if not a definite value, beyond the confines of the 2T model. The reviewer thinks we should have developed the 2T model as a computational model. This lies beyond the ambition of the present paper. We address the reviewer’s comments below.

2 General Comments

The reviewer's first sentence is not quite correct, and the subtleties of its inaccuracy have a lot to do with our differing views of what the paper should be about. Firstly, we use a Toy model, the 2T model, to develop the concept of plumes and to justify talking about the cross-sectional areas of plumes. We then go on to use this concept to develop a conceptual model for the empirically-discovered scaling properties of temperature spectra, cospectra and profiles. The reviewer has not recognized the difference in our terminology of up-plumes and down-plumes, which might, or might not, be moving upwards or downwards locally, and his terminology of upwards-moving and downwards-moving plumes. The reviewer should also acknowledge that our model is a conceptual model intended to underpin a semi-empirical similarity model, not to become a computational model. We attempt to explain why our empirical scaling results are as they are, and we extend our purview to include the mean temperature profile. In our empirical analyses of the data from the SLTEST experiment we found that velocity and temperature spectra could all be collapsed onto universal curves in each of three spectral ranges. Further, we found that length scales needed to achieve this collapse were not just the simple length scales used in the orthodox Monin-Obukhov similarity theory, but that mixed length scales and doubly mixed length scales were also required. We have achieved a very large improvement in our ability to represent T spectra and wT cospectra in universal ways.The scales that allowed us to do this are new to science. Our question is how should we understand this zoo of new length scales?

In general terms, our longer-term project is to provide a replacement for the standard Monin-Obukhov similarity theory (MOST). This is a statistical model based, conceptually, on Richardson's assumptions about the local action of buoyancy and the irrelevance of flow conditions at the upper limit of the atmospheric surface layer (Richardson, L.F.: Proc. Roy. Soc, Lond. A, 97:354-373, 1920). In essence, Richardson's model is a linearized model of boundary layer turbulence—one where the each term of the RANS energy equation can be interpreted individually and without regard to context in the flow system. MOST can be regarded as Richardson's theory expressed in a more convenient form. In sharp contrast, our similarity model accepts that the atmospheric boundary layer is a dissipative system, with all that that implies about the importance of energy flows in sustaining the turbulence (Fig. 8) and about the cyclic nature of cause and effect in complex systems. We note that similarity models, such as MOST and ours, do not provide quantitative results themselves, but they do provide a framework by which experimental results can be represented in universal ways.

Though this is our underlying purpose, we delay any general discussion of similarity models until the end of the paper, which is to say until after we have shown that the set of scale lengths that we have discovered can be explained in terms of the cross-sectional areas of plumes, and the way these areas vary with height. This connection has no precedents in the literature of boundary-layer meteorology nor, so far as we are aware, in fluid mechanics generally.

When framing our discussion the first problem was to define plumes somewhat more closely than "patterns of scalar concentration". We wanted to use the idea of the cross-sectional areas of plumes, but without a definition of a plume boundary this is as elusive as an exact definition of a plume itself. Problems of definition are common when dealing with complex systems. (What is a species in ecology?) Our approach is to define a plume in a rigorous way for a toy model, then to say "a plume is something like that". We appear to have achieved our goal inasmuch as the reviewer understands what we mean and adopts the word without complaint. However, the reviewer also wants more than just a toy model, and for us to develop the 2T model into a computational model. Such a development lies beyond our present ability and purpose.

The reviewer points out that the 2T model is incorrect as it stands. He is strictly correct since we had omitted mention of our assumption that air density is independent of temperature. This has been remedied in the revised text. The 2T model, as a toy

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model, still serves our purpose.

To the reviewer's point about repetition: the same idea is repeated several times in section 3, but the details vary each time. Our purpose is to show that the various length scales shown in Table 1 and Figs. 6 and 7 can all be interpreted in terms of plumes, and the aggregation properties of plumes, in a consistent way. The point of the repetition is to show that the basic concept of composite plumes is consistent with the observed length scale in each case. While writing the paper, and while trying to achieve this consistence (aka repetition) we were lead to question some results presented in an earlier paper by J. Laubach and K.G. McNaughton (Bound. Layer Meteorol. 133:219-252, 2009), so the present paper also reports new analyses based on the more comprehensive data from the SLTEST experiment. The revised results are given in Figs 6 and 7 of the present paper. Including these has lengthened the paper beyond what was originally planned.

3 Specific Comments

1. The density of air is assumed independent of temperature in the 2T model, so (9) is correct. This assumption is now stated explicitly. We note that the 2T model is a Toy model, not a computational model. Its purpose is given at the start of section 3.3. With (9) accepted (11) follows from (10), and (12) then follows since $f_u(1-f_u)$ and $f_d(1-f_d)$ are both equal to $f_u f_d$, using (8). There are no tricks. [Original equation numbers.]

2. The derivations are exactly parallel to those of (11) and (12).

3. This is implied directly by (19). The comment is intended to encourage the reader takes a step back from the formalism and to think about plumes as physical entities.

4. Only formal aspects of the 2T model are set out in section 3.2. It is enough that we can define them and use them to identify scales. Particular results are referred

to, when needs be, in later sections. Implicit in our concept of plumes is that they are entities that exist in space and time. They therefore lie outside the purview of the Monin-Obukhov similarity model, in which scheme there is great confusion as to the fundamental differences between ‘eddies’ (patterns of motion) and ‘plumes’ (patterns of scalar concentration).

5. To make our point clearer we have added an equation for the mean temperature, new (10) to section 3, and have connected our interpretation of Fig. 2 to that equation explicitly in section 5.1. We have also rewritten our argument and, in particular, made clear our use of Taylor’s frozen turbulence hypothesis when making our “leap of faith” in the interpretation of Fig. 2. These additions should allay the reviewer’s concerns.

Throughout the paper we are very clear that that the 2T model is a toy model—useful for illustrating concepts and generating ideas but not an exact model. In a general way, and in real CBLs, we know that air is heated by contact with the ground and that this air then moves upwards, on average, to convey heat upwards into the bulk of the CBL. We can associate this, conceptually though not in detail, with the 2T idealization of up-plumes. We also know that air is entrained through the top of the CBL, and that this air is cooler, on average, than the air found close to the ground. This entrained air moves downwards, on average, throughout the CBL and we can associate this with the 2T concept of down-plumes. The question is whether that a useful thing to do? Can it help us to understand why the shapes of temperature PDFs change with height the way they do? Alternatively, what can PDFs say about real plumes?

6. The rationale for our scaling scheme is given in paragraph 3 of the Introduction. Going back to McNaughton et al (op. cit. 2007) we identify the velocity scale as the velocity scale appropriate to impinging outer Richardson eddies. The outer dissipation rate is independent of height above the ASL and the length scale is z because these eddies impinge onto the ground. A more complete statement is given in the paper by Laubach and McNaughton (op. cit., 2009). Re line 287-305: we are, in effect, stating our working hypothesis here. We are trying to explain what lies behind the empirical

observations reported here and in earlier papers. Below we test that this explanation is consistent with the observations in a range of situations.

7. The paragraph has been redrafted. Composite plumes are more than just conjecture. If we watch the smoke rise from a fire, or to subside and fumigate at ground level when the surrounding air is subsiding, we see that the smoke becomes less dense as the plume grows and spreads. We must conclude that a real smoke plume is not like a fixed-concentration up-plume of the kind defined in the 2T toy model. How can the 2T model then help us understand the real world? The problem is not that the 2T model neglects molecular diffusion since this must be unimportant at the meter or ten-meter scale of the smoke plume. The problem lies in the different identities of real plumes and up-plumes. The first step towards a reconciliation is to think about real plumes as composites of 2T-type, fixed-concentration up-plumes (i.e. undiluted smokey air) and down-plumes (clear air), with the filaments of each remaining conceptually distinct but increasingly entangled nearer the ground due to the mixing action of small eddies. Maintaining the fixed identities of smokey and clear air then gives rise to the concept of composite plumes. They provide a useful approximation of reality and they allows us to align observed scaling properties with the concept of plume areas made concrete in the 2T model.

The wider problem here seems to lie with our terminology—what we mean by up-plumes, down-plumes and composite plumes. We have added a paragraph on these at the end of section 3.3, and have made small changes elsewhere where confusion might arise.

8. The original paragraph is rather turgid and has been redrafted. We have no explanation for why the power is 1/2. We are now aware that the half power law found experimentally in the mixed scales for certain dynamical properties close to smooth walls have been derived by the mathematical technique of asymptotic matching of the profiles in the viscous sublayer below with those in the log layer above (N. Afzal, J. Mécan. théor. appliq.1:963-973, 1982). The method has since been extended to matching

passive-scalar profiles near the smooth floor of a channel flow, but not yet to matching temperature in the transition between mixed-layers and log layers in CBL flows.

9. We used Taylor's frozen turbulence hypothesis because it is conventional to do so in Boundary-Layer Meteorology, and because we had no alternative. This is not to say we are unaware of its limitations. The core of the problem is Taylor's statement that eddies are carried along by the mean flow (GI Taylor, Proc. Roy. Soc. Lond. A, 164:476-490, 1938). We have known for a very long time that plumes are not simply carried along by the mean flow: their phase velocities increase with eddy size in the ASL (DS Davison, Quart. J. R. Met. Soc.. 100:572-592, 1974). Recently Cheng et al. (Geophys. Res. Lett. 44:4287-4295, 2017) confirmed this by the splendid innovation of measuring temperatures along an optical fibre strung out horizontally over the ground. We have yet to incorporate their results into our interpretations of time series, but insofar as corrections for plume velocities can be scaled using the same set of similarity variables as we use elsewhere, the revision should entail just a change in the shapes of the spectra and cospectra, not a change in their scaling properties or universality. Our conceptual model will remain intact, though Figs. 6 & 7 will require adjustment. We endorse Cheng et al's point that a distorted wavenumber axis in our Fig. 7 will lead to an underestimation of the area under the wT cospectrum, and so to significant 'flux loss'.

10. The section has been rewritten and retitled to reduce repetition. The relevance of the mixing length model is now explained much more fully. The new material is important because it emphasizes the failure of Reynolds' analogy between momentum and heat transport.

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4 Technical comments

All terminology is standard for those familiar with transport of scalars in the atmosphere. For others we have checked that Wikipedia is a sufficient resource.