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Comments

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Development of atmospheric cosmic-ray showers. II

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In a recent paper Orford and Turver criticized one of our previous papers which had concluded that a change in cosmic-ray primary mass composition was required in the energy range 10^{15} to 10^{17} eV. It is suggested here, in reply, that the inconsistencies and shortcomings claimed by Orford and Turver are largely not substantiated in the light of available information and that, in the absence of new ideas, the original conclusions are valid.

In a recent paper[1] we offered evidence for a change in the primary cosmic-ray chemical composition between 10^{15} and 10^{17} eV per nucleus. The basis for our claim was the variation with sea-level shower size of the depths of cosmic-ray extensive-air-shower (EAS) maxima inferred from the measured time full width at half maximum (FWHM) of the atmospheric Čerenkov radiation signal in individual showers measured with a single detector. The depths of maxima of EAS's are expected to depend on the nuclear physics of the shower cascade process and also on the composition of the initiating particle. It is thought that progressive changes in the nuclear physics, combined with changes in total shower energy, cause the depth of maximum to increase progressively with increasing initiating particle energy for a fixed composition. However, if the depth of maximum changes rapidly with increasing shower energy (often measured by the number of particles at sea level, the shower size), the preferred explanation is probably a change in primary composition. This change in depth with energy (the elongation rate) appears consistent with a fixed composition above sea-level shower sizes of ~10^7 particles (about 10^{17} eV primary energy) but we offered evidence for a very rapid change in the two size decades below this. Orford and Turver[2] of the Durham group have recently suggested that this conclusion is invalidated by inconsistencies and errors in that work.

The criticisms of Orford and Turver are in four broad categories: They regard our assumptions on the form of the dependence of the Čerenkov FWHM with distance from the shower core as incorrect. They believe it is hard to do our experiment. They feel we have used an inadequate model of the atmosphere. They believe the data we presented are in conflict with a datum previously published by ourselves. We feel their points are interesting and believe there is substance in their criticism of our atmospheric model, a criticism that we have previously made ourselves.

The problem of the dependence of the FWHM on shower-core distance (r) is of central importance in the use of Čerenkov FWHM techniques in air-shower physics. The reason for this is that, in the data-analysis process, experimental data usually have to be standardized to a convenient reference core distance. Theory can be developed with most confidence at the larger core distances and a distance of 300 m from the core is now normally[3] chosen as a useful compromise for standardizing data and also comparison with theory. The problem for the experimentalist is then to determine a proper method of standardizing the data to a core distance of 300 m. This problem is particularly important to us since we have chosen to study the interesting energy region around 10^{16} eV primary particle energy where the air showers are small and consequently we have little data at such large core distances. Extrapolation is therefore necessary. It is usual to assume a functional form for the dependence of FWHM on core distance and two forms have been used by ourselves and others. These are either

$$\mathrm{FWHM} = cr^n \tag{1}$$

or

$$\mathrm{FWHM} = a + br^2 . \tag{2}$$

Either of these expressions can be an adequate representation of the same theoretical or experimental data, depending on the range of the variables being considered and the uncertainties in them.

The former functional form has been used mainly by ourselves and the Moscow[3] group and the latter mainly by the Durham[4] group. Here the values of a, b, c, and n are to be determined. The usual assumption is that a, b, and n are functions of shower development through dependence on H_m, the shower height of maximum (usually expressed in km above the observer). The appropriate value of n is clearly important and is the subject of much of the criticism made by Orford and Turver. The Soviet group[5] find a value of 1.6 [later revised to 1.7 (Ref. 6)] as a useful experimentally based value for them at larger core distances and shower energies. We[7] find that a value of 1.4 ± 0.2. fits our data as a best estimator for the FWHM at 300 m (from a multiple regression analysis). It is difficult to compare our data with Durham experiments since both we and the Soviet group use an estimate of the value of the measured FWHM after removal of the instrumental impulse response (by assuming that instrumental response and light pulse shape had added in quadrature[3,7,8]) and the Durham group display their data without any such subtraction. We have, however, taken some recent Durham data[9] (measured at their Dugway field station) and subtracted (in quadrature) their published impulse response to produce the data (with error bars) in Fig. 1. A power-law form appears reasonable with a value of n of ~0.9 being appropriate. This is of interest in demonstrating the reasonableness of a power-law form but the value of n cannot be directly compared with the other data since the Dugway array is at a different altitude from the others. Figure 1 also includes data presented by Hammond *et al.*[4] from a sea-level experiment with a system FWHM of 18 ns.[11] Again we have removed the system FWHM in the standard way. A value of n of ~1.1 seems appropriate to these data. We have demonstrated that the quadratic subtraction of the system impulse response works reasonably for our data[8] and the Soviet group uses a similar technique. It is clearly possible that this may not work well for the Durham data. Nonetheless, if the data is handled *consistently* and an experimentally derived relation used for n then the experimental best estimator of the FWHM at 300 m should be appropriate and at this core distance the effect of most system FWHM's is small.

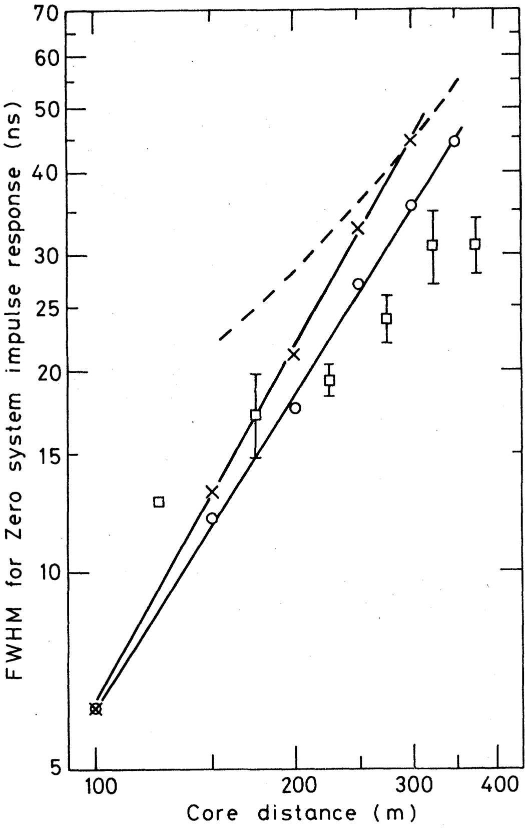

FIG. 1. Some representative data showing the dependence (theoretical and experimental) of the atmospheric Čerenkov pulse FWHM for a system with an ideal response on shower-core distance over the core-distance range relevant to the Adelaide observations. Crosses: Calculations by Gaisser *et al.* (Ref. 10) for a 5×10^{18}-eV iron primary observed at sea level. Open circles: Calculations by Gaisser *et al.* (Ref. 10) for a 10^{17}-eV iron primary observed at sea level. Open squares: Observations by Andam *et al.* (Ref. 9) made at a high altitude site. The system FWHM (6.7 ns) has been removed on the assumption that it had added in quadrature with the signal. Dashed line: Experimental relationship obtained for large sea-level showers by Hammond *et al.* (Ref. 4). The system FWHM (Ref. 11) (18 ns) has been removed on the assumption that it had added in quadrature with the signal. Solid lines are added to open circles and crosses for clarity.

Computer simulations of shower development can help and Fig. 1 also includes two relations calculated at Durham[10,12] for different shower developments observed at sea level. Values of n of ~1.8 and ~1.6 are found with the larger value corresponding to the lower value of H_m. Again, the power-law form seems entirely appropriate. Orford and Turver claim that Durham calculations[11] show a value of $n \sim 2.0$. We were unable to confirm this from their reference. The Soviet group[3] has also made calculations on cascades and Orford and Turver quote a value of $n = 2.0$ from this work. We have reservations about this result since there appears to us to be an inconsistency in this paper. This is currently the subject of correspondence between ourselves and the Soviet workers.

It appears therefore that experimental results in our range of core distances give values of n in the range of ~1.0 to 1.7 and theory fits $n \lesssim 1.8$. A value of n below 1.6 (the smaller of the Durham[10,12] theoretical values) seems appropriate to showers which develop somewhat higher than those discussed in the calculations. It is our opinion therefore that the value of 1.4 ± 0.2 found and used by us is probably about right. We have, however, previously conceded that a problem exists due to uncertainties in n and for purposes of comparison have also used formula (2) for analyzing our data.[8] We showed (Fig. 1, Ref. 8) that our data can be analyzed using either formula (1) *or* (2) and essentially the *same* result is produced. It appears to us therefore that while it is right to examine the core-distance dependence critically, the dependences we have employed are both reasonable and produce consistent results. We note that if we reanalyze our data with $n = 1.8$ (we regard this as an extreme case) the effect is to increase *all* our deduced depths of maxima by ~45 g cm^{-2}.

Orford and Turver next make a few comments on the technical difficulty of measuring useful FWHM's at core distances below 300 m. It is well known[8,11,13] that at ~70–100 m from the shower core FWHM's are practically invariant with depth of maximum and the ease of determining shower development (in terms of system time resolution) improves with core distance away from this region. On the other hand, signals have greater amplitudes at smaller core distances and there is much physical interest in the showers of smaller size which are difficult to detect at core distances ⩾300 m. It is with this in mind that we set up a system with good time response. Our 5.3 ns system FWHM was the best of any in the field until recently when we ourselves have set up an improved second system. The Durham workers currently have an impulse response FWHM of 6.7 ns but digitize at 10 ns intervals and have in the past used an impulse FWHM of 18 ns. We would certainly regard our data as being at least as well measured in terms of physically useful parameters as theirs; compare for instance, Ref. 14, p. 45 with Ref. 8, p. 107. We, of course, do not use data from showers with core distances close to 100 m. Contrary to the assertion of Orford and Turver, with a system FWHM of 5.3 ns, it is not too difficult to extract a useful *averaged* height of maximum data on variations of less than 100 g cm^{-2} with a sensitivity of ~3.5 ns per 100 g cm^{-2}. In fact, they have, for an extended period, used a system FWHM of 18 ns with a sensitivity at their core distance of 10 ns per 100 g cm^{-2}. In a sense, the consistency of the variation of the data in Fig. 1 of Ref. 1 would lead one to conclude that in our core-distance range, measurements of useful sensitivity certainly can be made. Any problems in the data definitely are not *statistical* uncertainties. We note that our errors as shown are reasonable for the spread in the data and are quite small enough to show trends in the data. An examination of the figure in our paper[1] makes this obvious.

Since we wish to determine the development of EAS in the atmosphere in terms of atmospheric depth in g cm^{-2} from the top of the atmosphere and since the Čerenkov FWHM gives us the height of that development above the observer, it is necessary to have a model for the atmosphere with which one can relate absorber depth to altitude. This problem is not trivial and it seems to us that it should be brought explicitly to the attention of EAS workers although we know it has been discussed privately many times. It is customary to approximate the atmosphere to one having exponential properties with a characteristic scale height. This is the crudest of models and Orford and Turver were correct to criticize us for using an atmospheric pressure scale height of 7.1 km. We have mentioned this problem in an earlier paper[15] in which we ourselves pointed out that at

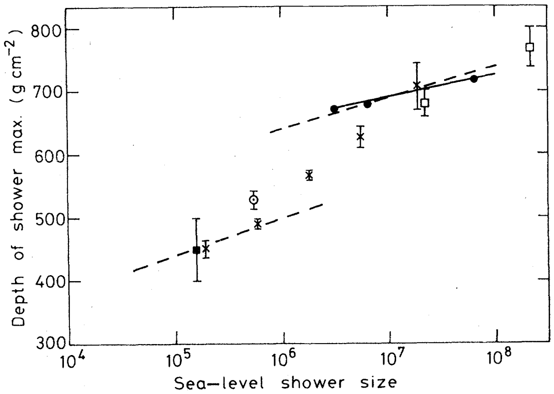

FIG. 2. The measured relationship between the depth of air-shower maximum and sea-level shower size. Where results have been given in terms of primary energy, the relationship sea-level size = primary energy $\times 10^{-10}$ has been used. This is derived from our measured shower-size spectra and primary energy (Ref. 12) spectra. Crosses are our Čerenkov observations (Ref. 1). Filled circles and the solid line are data from the Soviet Čerenkov observations (Ref. 3). Open squares are data from the Durham Čerenkov observations (Ref. 4). The filled square is from airplane particle data of Antonov (Ref. 16) interpreted by Watson and Linsley. Open circle, a mean value derived from early Čerenkov data of Thornton and Clay (Ref. 7).

our observing site, the appropriate scale height is 8.0 km. We therefore present here, in Fig. 2, a revised version of the figure in our previous paper[1] which displays our current FWHM data analyzed using the appropriate pressure scale height. We wish to make a more general comment here also, however. The value of 8.0 km we use is derived from local measurements (S. Young, private communication). We have found this local information most beneficial. As far as we know, some other EAS sites have not been so fortunate as to have this information although it is of most practical interest. The atmosphere is not isothermal, and although for many practical purposes (such as ours) an exponential *form* is adequate and useful, the scale height is not unique. The pressure and density scale heights are *not* the same and can be very discrepant. We make this point since this problem is related to both Čerenkov theory and experiment. Some EAS parameters depend on local pressure (e.g., the relationship between atmospheric depth in g cm^{-2} and height in the atmosphere) and others on local density (e.g., Coulomb scattering, Čerenkov production threshold). It appears that these differences are not always taken into account in theory (see, e.g., Ref. 12, p. 150) and we have always had difficulty in deciding the best procedure for interpreting our own data. The Soviet[3] calculations, for instance, use a scale height of ~7 km (appropriate to their observation site) and hence will use Čerenkov thresholds, scattering functions, etc., as functions of altitude which are slightly inappropriate to our needs. All observers have to contend with this problem to some extent (even particle EAS workers) since the scale heights are meteorological functions and vary by relatively large amounts at fixed geographical locations.

The criticism of our paper concerning inconsistency with previous work seems to merit little comment. It has been suggested that a value of 486 ± 13 g cm^{-2} is not consistent with our data at a mean size of 5.5×10^5. We would suggest that the concerned reader might plot this point on our Fig. 1 in Ref. 1. Alternatively, the point is included in Fig. 2 here with the depth appropriate to an 8-km scale height. The point with its errors is not statistically inconsistent with a reasonable line which one might draw through the total of our data and one would in any case expect a slightly high value since the derivation of this mean includes a group of larger showers with, as we show, rather larger depths of maxima than one might have expected. We should add that, despite the contrary assertion by Orford and Turver,[2] it is our understanding that a depth of maximum of ~500 g cm^{-2} for showers from iron primaries is quite appropriate in our size range.[10] This is not a conventional composition. In our first paper we displayed an interpretation of data derived from measurement by Antonov *et al.* at airplane altitudes on the height of maxima of small EAS. Watson and Linsley[16] have used more recent work of Antonov[17] and his collaborators to derive a depth of maximum for small showers. This point is included in Fig. 2 and appears to us to add strength to our conclusions.

To summarize, the criticisms of our work by Orford and Turver were fourfold.

(1) They were critical of our choice of techniques for deriving depths of maxima, particularly in the way we determine the estimated FWHM at 300 m. We have demonstrated here that a power-law form for the dependence of FWHM on r fits a broad class of published data including data from all three major groups in the field. Also, the power-law index we find is not by any means extreme and is consistent with Soviet experiment, internally consistent in our own data, and fits Durham calculations. In any case, alternative analysis procedures produce essentially the same final results.

(2) They pointed out that estimates of depth of maximum based on measurements closer to the core than 300 m are less sensitive than those made further out. This is obvious since the FWHM increases faster than $r^{1.0}$ with increasing core distance and it is the reason why we use equipment which gives us a very short system FWHM.

(3) We did not choose the best atmospheric model. This is true and we had already published material to this effect and revised our results.[15] We also note that as far as we know, other EAS workers have similar problems when their detailed procedures (theoretical and experimental) are examined.

(4) They thought there was an inconsistency with our previous work. The simple procedure of plotting the result they derived from our previous work on our figure should have demonstrated to them the considerable degree of agreement between early data and later analysis.

We conclude that Orford and Turver[2] have aired some interesting points and as a result we have revised our previously published results which are now shown in Fig. 2. The essential conclusions remain, viz, there is broad agreement with other observations for sea-level shower sizes of $\sim 10^7$. Considering known errors in depths of maxima for experiments on showers of sea-level size $\sim 10^5$, there is still good agreement. The elongation rate for showers with sea-level sizes of $\sim 10^6$ is still too high to be explained simply by a progressive change of nuclear physics with energy and can most simply be explained by a composition change.

There is perhaps a further point to be made on the subject of atmospheric Čerenkov measurements of the smaller air showers. We agree that difficult problems are encountered due to the generally small signals to be detected and that to overcome some of these problems it is necessary to work at core distances which make extrapolation necessary for comparison with theory. We do believe, however, that with sufficient accumulation of data it has become possible to disentangle the variables in the data and produce physically useful results. At the very least, Orford and Turver have conceded that our mean height of maximum for showers of mean size below ~10^6 is high. Even taking this datum and those of Protheroe and Turver[11] for primary energies of ~10^{17} and 10^{18} eV (we convert shower size to primary energy through the shower-size and primary energy spectra), there is a clear need for a very high elongation rate (>100 g cm^{-2}/decade in Ne). The paper they criticize says little *in principle* in addition to this except that details of the change in depth of maxima with energy are added.

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