Reply

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We thank Granberg for his meticulous interest in our paper. However, as there seems to be a misunderstanding of the general aims of the work, we should like to take the opportunity to reply.

First of all, and we would emphasize this strongly, Granberg is mistaken in assuming that our paper is merely a reiteration of the results given in Petzold and Kelly (1975); in our acknowledgments we have clearly qualified our source: "some of these results . . . in a different context . . . in less detail, etc." Much careful and thorough scrutiny and tidying up of the original material was done before the results were used in the final analysis that yielded Table 1 and Fig. 1. Specifically, from the original data we have rejected two sites where anemometers with high stalling speeds were installed, and we have imposed at least some low-speed cutoff at the reference sites. In general, we believe that the way the analysis was done was the only practical way, and that the resulting correlation coefficients do give good information on the tightness of the results. It is a matter of field experience to judge whether in such an ecosystem correlation coefficients of environmental variables that range from about 0.7 to 0.9 will yield acceptable results or not. For comparison, one of the true ecological relations H vs DBH had a coefficient of $r = 0.86$. If we could describe wind attenuation with comparable accuracy, we should be satisfied. This was our general aim in the paper, and not attempting to derive sophisticated, rigorous and detailed relations for which far more complicated instrumentation would indeed have been needed. This we have emphasized in several places (Section 4, ¶ 3), and we believe this is obvious to the reader.

Specific remarks follow:

2) SPATIAL VARIATION OF MOMENTUM FIELD AND TOPOGRAPHIC INFLUENCE

This would be a valid criticism, had we claimed to have tried to examine the short term microstructure of wind turbulence in the subarctic forest. We did not, and with the necessary 4 to 6 h measurement intervals, we had to assume that short term variations and most of the directional influence would cancel out. We were happy to see that in the reanalyzed results none of the sites showed any specific and significant bias to wind direction. With regard to topography, it is patently obvious that "given suitable depressions" or elevations, anomalous spots in the wind field can easily be found. As we do not claim to describe the microstructure of the wind with regard to every hollow and bump, but merely wish to give an overall yardstick that could be used on a much larger (say 1 km) spatial scale, we feel that we have made progress and our results have some large-scale applicability.

3) ANEMOMETER CALIBRATIONS

This criticism is valid, but the effect is certainly not as bad as Granberg implies. We have rejected the results from two anemometers with high stalling speeds, and installed all of them in such a way that the sensitive ones were in the dense sites, and the less sensitive ones were in the more open sites. This is such an elementary precaution which does not need any emphasis. It is obvious that the stalling speeds would lead to some underestimates of the wind at the woodland sites, but the true degree of underestimate could only be shown quantitatively if the spectrum of wind velocity were known in detail. If most of the momentum from the overhead airflow penetrates in high velocity gusts, then the error is less serious than it would be if it were resulting from an even velocity distribution. Our indirect confirmation for this was shown by the plots of site windspeed versus airport windspeed, both using the Casella totalizing anemometers, where none of the intercepts differed significantly from zero. Hence we can assume that the stalling speeds were not a significant source of error. Although the question has some relevance, we believe it is no more serious than the same type of inaccuracy which is also inherent in the climatological records of published average monthly or daily windspeeds calculated from the run of wind records. We should also like to draw Granberg's

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attention to Section 2, para. 3 where we describe our measurements with the lightweight Casella anemometers which he seems to have ignored.

4) ZERO INTERCEPTS, TOPOGRAPHIC INFLUENCE AND OVERALL ACCURACY

It should be relevant to point out here that subjective impressions are often misleading in trying to guess aerodynamic roughness correctly. Even for a stormy ocean, the characteristic features that determine its roughness length are the small ripples and not the towering waves. Admittedly, our assumed value of 1 mm may not be exact, but we believe it is near enough for the given site, and we can only ask Granberg to peruse the third paragraph of Section 2, in which we did stress the uncertainty in the reduction of the 10 m wind, and also gave approximate percentage estimates of the resulting possible error. Because our present paper bears little resemblance to Petzold and Kelly (1975), we feel that the comments beyond the first paragraph of this section are irrelevant, or already answered.

Finally, we should like to emphasize the philosophy expressed in our paper which Granberg unfortunately ignores. This was to provide a simple, realistic and quick method to estimate the overall degree of wind attentuation on a large scale in a difficult ecosystem that would give comparable accuracy with other environmental measurements obtainable there.