Reply

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First, let us note that the comments by Hines (2005, hereafter denoted as H05) relate to a one-paragraph interpretative remark made in the concluding section of Broutman et al. (2004, hereafter denoted as BGE) and not to the main thrust of our paper. In BGE we set out to describe linearized internal wave packets in a spatially and temporally varying background flow, using either a Lagrangian or an Eulerian reference frame, and then to demonstrate that under the appropriate coordinate transformation there was a complete equivalence between the respective dispersion relations and ray equations. BGE were motivated in part by the work of Allen and Joseph (1989, hereafter denoted as AJ) and Hines (2002), where in particular the issue of internal wave ray paths in the respective reference frames is directly raised. In AJ, Hines (2001), and Chunchuzov (2002) (called collectively AJHCh by H05), a Lagrangian reference frame was used to study internal wave ensembles, in contrast to the more usual use of an Eulerian reference frame.

We understand from H05 that the principal results in BGE are not in dispute. Instead, the issue raised by H05 is the interpretative remark in BGE (see the second paragraph of section 5) relating to a result of AJ, who found that when they transformed a Lagrangian internal wave spectrum into an Eulerian spectrum, the Eulerian dispersion relation was not satisfied for the high wavenumbers. Our work shows that indeed the Eulerian dispersion relation should not be satisfied, since AJ used an incorrect form for their Lagrangian dispersion relation, one that does not account for refraction. This was our point, and even so we were (and remain) cautious, offering this remark not as a full explanation but only as one "possible factor contributing" to the result. H05 disputes this explanation, and also queries the validity and relevance of ray theory to examine the high-wavenumber component of the Eulerian spectrum.

H05 argues that there is a distinction between the "local" ray theory of BGE and the "global" statistical ensemble theories of AJHCh. We agree with H05 that there is indeed a distinction, but the point at issue is whether there is some consistency between the two theories in the parameter regime when one is considering short waves interacting with long waves.

In the theory of AJ (and in the related studies of AJHCh) one considers an internal wave frequency–wavenumber spectrum, in which each wave, or Fourier component, would, in the *linear* approximation propagate independently and satisfy the familiar internal wave dispersion relation that holds when the background state is one of rest with a uniform buoyancy frequency. But under the influence of nonlinearity these waves undergo various wave–wave interactions, where, in the usual spectral theory, the outcome is finally described only in a statistical sense. H05 argues that the individuality of a wave is lost in this process, and hence calls this behavior "nonwavelike." By this he means that a Fourier frequency–wavenumber decomposition of the *nonlinear* fields would fail to identify a single frequency with a single wavenumber, and hence this Fourier decomposition would fail to satisfy a dispersion relation. Allen and Joseph (1989) also use the term "nonwavelike" in this sense, as noted in BGE.

More specifically, AJ describe the Eulerian wave field only globally and statistically through its frequen-

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cy–wavenumber spectrum. Each Fourier component of the Eulerian frequency–wavenumber spectrum can fail to satisfy an Eulerian dispersion relation due to, inter alia, variable and intense Doppler shifting in space and time, as noted by H05. Allen and Joseph (1989, p. 216) describe such Eulerian "nonwavelike" behavior as an "interesting and potentially important result," although the result has probably been well appreciated for some time, and as implied by H05, it could have been inferred without the complication of a Lagrangian-to-Eulerian transformation. As a digression, we should point out that in our view, the use of the term "nonwavelike" is rather unfortunate and potentially misleading. A nonlinear wave field is just as "wavelike" as a linear wave field, although of course it is usually quite different in structure. The point can be made by analogy; the sea surface is universally considered to be wavelike, whether it is composed of an ensemble of two-dimensional (in plan view) interacting waves, possibly even containing some white-capping, or consists of smooth almost periodic one-dimensional wave trains. Nevertheless, in this short article, we will continue to use the term "nonwavelike" in the sense used by AJ and H05.

Of course BGE does not directly deal with these issues of the global statistical spectrum. As H05 correctly points out, it is a local linearized theory of the familiar kind. An internal wave packet, with a locally defined frequency and wavenumber, propagates through a background flow. It is clear from the literature (e.g., Fritts and Alexander 2003) that there is a widespread perception that the high-wavenumber end of the Eulerian spectrum can possibly be interpreted as the refraction of short waves by long waves. Whether this is so or not, BGE shows that such refraction cannot, in general, be removed by the use of a Lagrangian frame.

Nevertheless, H05 (see also Hines 2002) suggests that "nonwavelike" behavior would possibly be less severe for a Lagrangian spectrum than for an Eulerian spectrum. This suggestion is based on the observation that the Doppler-shifting effect in an Eulerian formulation is not explicitly present in a Lagrangian formulation. However, as shown in BGE, instead the Lagrangian dispersion relation has an explicit time and space dependence when the effects of a variable background flow are included. This corresponds spectrally to a range of Lagrangian frequencies for each Lagrangian wavenumber.

Consider, for instance, the example given in section 4 of BGE for a steady background flow with constant shear and uniform buoyancy frequency. Here a Lagrangian wave packet can refract from a turning point to a critical layer, preserving its Lagrangian wavenumber but changing its Lagrangian frequency through all values below the buoyancy frequency. Thus, while "wavelike" in the local sense of ray theory, the packet is grossly "nonwavelike" in the global spectral sense. The AJ model does not include steady shear, but it does include low-frequency near-inertial shear, for which we would expect similar behavior.

H05 also raises the issue of whether the scale separation needed for ray theory is realized in practice in the ocean and atmosphere, and even if so, he contests the usefulness of ray theory on the grounds that observations do not follow wave packets. We agree with the first point as a general cautionary statement, but note that neither point has prevented ray theory from providing useful predictions of internal waves, including short-wave spectra (e.g., Flatté et al. 1985). H05 goes on to claim that ray theory is in any case irrelevant for the present application, citing nonrefractive deformations of weakly interacting longer Lagrangian waves as sufficient to account for the short waves in an Eulerian frame. If correct, this would raise other issues, such as how is it that the strong interactions between short waves and long waves in an Eulerian formulation (reviewed in Fritts and Alexander 2003) could somehow disappear, or become weak interactions, in a Lagrangian formulation. In this context, we are planning to carry out Lagrangian ray-tracing experiments and will have more to say about Lagrangian rays and Lagrangian nonlinearity when that work is reported.

We now reply to the further comments of H05 made in response to the preceding paragraphs.

First, our use of a *locally defined* dispersion relation is completely standard.

Second, our comment about the use of the term "nonwavelike" relates more to AJ than to H05.

Third, we made the following comment above: ". . . H05 (see also Hines 2002) suggests that 'nonwavelike' behavior would possibly be less severe for a Lagrangian spectrum than for an Eulerian spectrum. This suggestion is based on the observation that the Doppler-shifting effect in an Eulerian formulation is not explicitly present in a Lagrangian formulation . . ."

This was based on the following comment from H05: the Lagrangian dispersion relation contains ". . . local variations in the spatial derivatives of fluid displacement rather than local variations in fluid velocity. But in practice the Lagrangian modifications tend to be far less severe than are the Eulerian modifications that arise at larger **k**."

Fourth, the steady background flow used in section 4 of BGE was not "morphed" into a low-frequency background wave. We suggested above that if short-wave refraction were strong for a steady background flow it might also be strong for a background containing a low-frequency near-inertial wave with shear similar in magnitude to that of the steady background flow. Near-inertial waves may not be the concern of Hines (2002), but they are an energetic component of the model spectra analyzed by AJ.

Fifth, we welcome H05's acknowledgement of the relevance of ray theory in the appropriate circumstances, a sentiment with which we completely agree!

Finally, with respect to the final paragraph of H05, we suggest again, as we have above, that these issues are best addressed through further work, specifically involving some actual ray tracings with the Lagrangian dispersion relation derived in BGE. If refraction is weak in a Lagrangian frame while strong in an Eulerian frame, this should emerge from such ray tracings.

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