Reply

JOHN A. KNOX

Department of Geography, University of Georgia, Athens, Georgia

DONALD W. MCCANN

McCann Aviation Weather Research, Inc., Overland Park, Kansas

PAUL D. WILLIAMS

Department of Meteorology, University of Reading, Reading, United Kingdom

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1. Introduction

We thank Plougonven et al. (2009, hereafter PSZ) for their comments on Knox et al. (2008, hereafter KMW). We are grateful for the opportunity to refocus attention on the still-unsolved problem of clear-air turbulence (CAT). We are also happy to concur with PSZ's acceptance of the empirical evidence for the effectiveness, efficiency, and relevance of our approach to CAT forecasting.

PSZ's objections to KMW are as follows: 1) Lighthill–Ford theory cannot be applied to observed small–Rossby number (Ro), large–Froude number (Fr) baroclinic flows; 2) if this theory is applied, it is misleading to do so in a local sense, because there must be a spatial separation between the local balanced motions and the far-field gravity waves; and 3) if applied, the forcing term will contribute to both gravity wave generation and slow balanced dynamics, the latter being dominant. Although compatible with a selective reading of the literature, we contend below that these objections discount other relevant experimental, observational, and theoretical evidence to the contrary and are in some cases at least partly rooted in a lack of familiarity with the practical problem of CAT forecasting. With regard to their third objection, we in fact find support for our interpretation in PSZ's own analysis, and a lack of support for PSZ's alternative explanation for the success of KMW's approach. We encourage additional work to resolve apparent or real conflicts in the published literature.

2. Application of Lighthill–Ford theory to observed flows

In KMW, we knowingly and openly applied Lighthill–Ford theory outside of ''configurations such as those described by Ford et al. (2000),'' as PSZ note. As indicated explicitly in KMW, the inspiration for this application was the experimental fluid dynamics work of Williams et al. (2005, hereafter WHR05)—see also Lovegrove et al. (2000) and Williams et al. (2003, 2008).

The small-Ro, large-Fr results of WHR05 linking Lighthill–Ford forcing regions spatially with observed inertia–gravity wave generation provided strong motivation for the inference that Lighthill–Ford theory may be usefully applied beyond its formal bounds of validity. This was the genesis for KMW's work. The subsequent, surprisingly successful application of Lighthill–Ford theory to CAT forecasting in KMW is, in our view, still more motivation for this inference. We reject PSZ's claim that we have misinterpreted the theory; instead, we tested the limits of its application, which appear to differ from the formal theoretical limits emphasized by PSZ.

PSZ attempt to raise doubts about the connections between the gravity waves observed by WHR05 and the Lighthill–Ford diagnostic. With regard to the two supposed ''unresolved issues'' raised by PSZ, we reply as follows:

Corresponding author address: Dr. John A. Knox, Department of Geography, University of Georgia, Athens, GA 30602.
E-mail: johnknox@uga.edu

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1) There is nothing unusual about large-scale forcing generating small-scale waves. Various numerical (e.g., O'Sullivan and Dunkerton 1995) and theoretical (e.g., Medvedev and Gavrilov 1995, hereafter MG95) work demonstrates that the horizontal scales of wave forcing are smaller than those of the associated meteorological motions and are comparable to the scales of the excited waves.
2) There is also nothing unusual about a quadratic forcing leading to waves with amplitude linear in Ro. It was recognized long ago (e.g., Jeffrey and Kawahara 1982) that conventional asymptotic methods cannot describe this effect. Multiple-time-scale techniques such as those used in MG95 resolve this seeming paradox. The excitation of gravity waves found in MG95 is quadratically forced, linear in Ro, and in full agreement with WHR05. It is therefore noteworthy that the work of KMW is consistent with the theoretical results of MG95, who derived forcing terms describing the continuous generation of inertia–gravity waves by quasigeostrophic (QG) motions. For Ro < 1, the same scaling as in KMW, MG95 obtained forcing terms similar to those found by KMW, in particular the advection of relative vorticity [the leading-order term 2B in Eq. (6) in KMW; Eq. (23) in MG95].[1]

PSZ further attempt to call into question the results of WHR05 by downplaying the "spatial coincidence" of gravity waves and Lighthill–Ford forcing. However, the identification of such spatial and temporal coincidences has been the genesis for virtually all of the current CAT forecasting diagnostics in existence. Even PSZ admit that these spatial correlations are "of interest" and provide "an indication . . . for a generation mechanism." Indeed, this is why KMW pursued the matter further.

PSZ require as compelling evidence for a gravity wave generation mechanism "a more systematic investigation of the variations of the excited waves relative to the forcing" than was achieved by WHR05. Williams et al. (2008), which is not cited in PSZ, have already documented the changing amplitude of the excited waves as the forcing varies by a factor of 10, which is the largest variation the experiment will permit.

The results of WHR05 certainly warranted the further investigation in KMW. The subsequent success of KMW's approach implies that either Lighthill–Ford forcing is responsible for the gravity waves, or else some other mechanism just happens to be creating CAT in the regions of Lighthill–Ford forcing. We will address the latter possibility in section 5.

The reasons for the agreement between observations and theory found by WHR05 and KMW are not considered by PSZ but are the subject of work by others. As noted in KMW, T. Haine (2008, personal communication) finds that higher-order corrections to QG balance yield gravity waves that are mathematically slaved to the QG flow. The diagnostic for their appearance is the largest of the Lighthill–Ford subterms [i.e., term 2 in Eq. (3) in KMW]. Because this same diagnostic appears in both small-Ro and large-Ro theories, this may help to explain why both WHR05 and KMW found success outside of the original Lighthill–Ford parameter regime.

Thus, we contend that applying the Lighthill–Ford theory beyond its formal limits is consistent with other, successful experimental and theoretical analyses of gravity wave generation for the parameter ranges of relevance to CAT forecasting.

3. "Far field" and "local" in theory versus applications

PSZ raise concerns that interpreting the right-hand-side forcing term as a local indicator of gravity wave activity is "at the very least misleading" in Ro > 1, small-Fr flows. However, WHR05 found excellent spatial agreement between gravity waves observed in rotating-tank experiments and the Lighthill–Ford forcing terms for small-Ro, large-Fr flows. KMW then found quantitative agreement between a small-Ro scaling of Lighthill–Ford forcing terms [as employed in the algorithm of McCann (2001)] and CAT occurrences.

To explain this disagreement between PSZ and our work, we note that the forcing term is not the local indicator of gravity wave activity, but rather of gravity wave *excitation*. We seek to quantify this source of gravity waves rather than to solve the equations to obtain the wave field at a distance from its source. Thus, the scale separation is relevant to Ford's (1994) theory but not to our application. In fact, no such distinction between "local" and "far field" is required in MG95's small-Ro theoretical analysis. Furthermore, MG95 qualitatively identified maxima in the forcing terms with synoptic-scale flow patterns known to aviation forecasters as regions of CAT, such as regions of strong jet stream curvature (Lester 1994, 4–32).

In any event, any definitions of "local" versus "far field" are ambiguous when dealing with finite-difference forecast models and pilot reports of turbulence (PIREPs).

[1] PSZ attempt to decouple KMW and MG95 by claiming that the latter cannot be used to support KMW because it has not been substantiated by "full primitive equation simulations of synoptic flows exhibiting spontaneous generation." Although we agree that it is desirable to have more tests of the MG95 theory, we cannot accept this argument, as we read it, as being germane to the discussion.

Model resolutions of tens of kilometers, combined with known spatial and temporal errors of PIREPs of several tens of kilometers and a few minutes, respectively (Sharman et al. 2006), obscure any clear distinction between local and far field. Similarly, the fact that the Lighthill–Ford forcing regions in KMW have scales of hundreds of kilometers (e.g., Fig. 2b in KMW) suggests a blurring of local and far-field definitions. Although purists might prefer it to be otherwise, this is the messy real-life problem of CAT forecasting.

In summary, it would appear that any distinction between local and far field is less crucial to the application of the theory than is suggested by PSZ.

The following should be noted regarding our focus on the source, not the far-field propagation, of gravity waves: operational CAT forecasting algorithms have never attempted to forecast CAT by predicting the *motion* of gravity wave trains (e.g., ray-tracing techniques). Practical considerations have always limited forecasting approaches to the hypothesis that aircraft encounter turbulence near regions of strong forcing. This may be surprising to dynamicists, but it is still the state of the art in CAT forecasting and therefore was the starting point for KMW. This turns out to be of importance with regard to PSZ's discussion of slow balanced motions versus gravity waves (see below).

4. Slow balanced modes versus gravity waves in Lighthill–Ford forcing terms

The most sustained argument in PSZ regards the nature of the forcing terms—in particular, the partitioning of the forcing terms between slow balanced modes and gravity waves. In KMW, we did not evade this issue and duly noted the inexactitude of interpreting them as gravity wave source terms. However, WHR05 did, in fact, determine a direct relationship between large instantaneous values of the forcing terms and observed gravity wave generation, inspiring our own efforts. The admittedly nontrivial nature of exact separation of balanced and gravity wave contributions, as well as the wish for a tractable and operationally useful method, also motivated a simple, ''first cut'' approach to the problem.

PSZ contend that there is some ''small fraction'' of the forcing that projects onto gravity waves, and in their discussion claim that this would be altered by the inclusion of the background mean flow in the left-hand-side operator. On the contrary, the nonlinear forcing term is determined solely by the scaling (for $\mathrm{Ro} < 1$). The modification of the left-hand-side operator alters the linear propagation of the gravity waves but does not affect the excitation of the gravity waves.

Moreover, the conclusions of PSZ from their Eq. (6)—namely that the Lighthill–Ford term will force mainly slow balanced motions and that it is ''wrong'' to interpret the term as a source of gravity waves—appear to overreach. Although in the QG approximation of PSZ's Eq. (6) the forcing term is indeed the divergence of the **Q** vector, this equation is still a wave equation. Its solution represents a forced nonresonant gravity wave that is slaved to the right-hand-side term. This wave will remain near the place of its excitation after all transient (resonant) gravity waves forced by the term on the right-hand side disperse in space with passage of time. Because, as noted above, KMW's forecasting method (and all CAT forecasting methods) focuses on source regions, the gravity wave described in the QG approximation of PSZ's Eq. (6) is in fact quite relevant to our application.

Furthermore, the analysis of MG95 for $\mathrm{Ro} < 1$ indicates that the neglect of the local time derivative in the QG approximation of PSZ's Eq. (6) is not justified. With the inclusion of this term, the right-hand side of the equation is a source of both trapped and transient gravity waves. The proportional separation of the response to the forcing into trapped and transient waves is determined by the spatial and temporal dependence of the right-hand side, and by the wave operator on the left-hand side as well.

Therefore, contrary to PSZ's claim that ''non-zero values of the forcing terms … do not systematically indicate gravity wave generation,'' this discussion reveals that nonzero values of forcing [i.e., nonzero R in Eq. (3) of KMW] do, in fact, represent gravity wave generation! The confusion appears to result from the distinction between trapped and transient modes—but CAT forecasting methods need make no such distinction. Because, as noted above, CAT forecasting methods are based on the assumption that turbulence is encountered in or near the source region, trapped modes are arguably an ideal fit to the assumptions that underlie KMW's approach.

Rather than refuting our assumption that the forcing terms are related to gravity wave generation, PSZ's analysis has instead provided additional support for this claim and has inspired new thinking regarding the relationship between gravity waves and CAT.

5. Frontogenesis versus Lighthill–Ford forcing as an explanation for CAT

PSZ propose that KMW's forecasting success is based on ''reasons other'' than spontaneously generated gravity wave activity. PSZ's only specific alternative explanation for the success of KMW's approach is frontogenesis, which would lead to strong vertical shears and small-scale instabilities that would then translate into CAT.

PSZ appear to be unaware of the long history of frontogenesis-related CAT diagnostics. Widely used CAT diagnostics such as graphical turbulence guidance [GTG, as described in Sharman et al. (2006)] and the Ellrod index TI1 (Ellrod and Knapp 1992) are based on the frontogenesis function and/or simplifications to it using deformation. In GTG, which uses a statistically weighted combination of 10 different diagnostics at upper levels, at least 5 diagnostics are based on some aspect of frontogenesis, including the frontogenesis function and TI1. The frontogenesis function itself is the single most heavily weighted diagnostic of the 10, and TI1 is the next-most heavily weighted diagnostic. (As will become important shortly, positive values of frontogenesis are used as a CAT predictor in GTG; we infer from PSZ that they, too, correlate frontogenesis—but not frontolysis—with CAT.)

However, the reader is reminded that KMW's application of Lighthill–Ford theory to CAT forecasting outperformed the then-operational version of GTG in a nearly 5-month comparison. Given that PSZ have no quarrel with the empirical results of KMW, they are faced with the following dilemma: if KMW's approach is merely capturing the effects of frontogenesis, how can KMW's method outperform a CAT forecasting method that is largely based on frontogenesis as a CAT indicator?

In point of fact, the use of frontogenesis as a CAT forecasting method is insufficient and problematic. Knox (1997) pointed out that deformation-based methods will correctly forecast CAT for the wrong reasons, or fail to correctly predict CAT, in strongly anticyclonic regions. Partly for this reason, GTG incorporates other, nonfrontogenetic diagnostics (including one based on Knox 1997) to improve its performance.

Time constraints do not permit a reevaluation of KMW's entire dataset to compare frontogenesis to Lighthill–Ford forcing. However, we did compute and compare the two diagnostics for the case study in section 4 in KMW. The results, depicted in Fig. 1, are not encouraging for PSZ's alternative explanation. The region of moderate–severe and severe CAT over northwestern Illinois (see Fig. 2a in KMW for the location of pilot reports of CAT in this case study) that was correctly identified by the Lighthill–Ford diagnostic is coincident with a region of *frontolysis*, not frontogenesis (thick black lines, with negative values indicating frontolysis). Similarly, a report of moderate CAT over western Kentucky lines up with a maximum in Lighthill–Ford forcing as well as with a maximum of *frontolysis*. A region of mostly moderate CAT over extreme east-central Missouri and west-central Illinois is in proximity to local maxima in Lighthill–Ford, as well as a small area of *frontolysis*. Only in extreme northern Illinois does frontogenesis coincide with a Lighthill–Ford maximum and is in the vicinity of one report of moderate CAT.

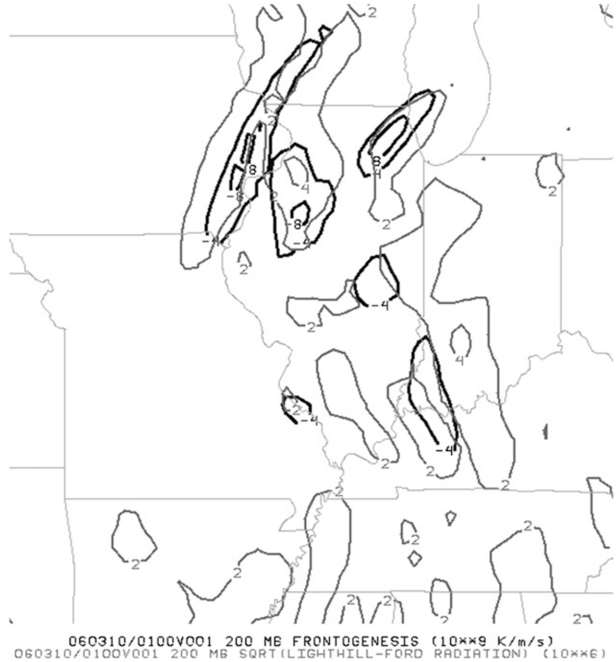

FIG. 1. The square root of the Lighthill–Ford forcing terms [thin gray lines; see Eq. (23) of KMW for details] and frontogenesis (thick black lines) at 200 hPa using the 1-h Rapid Update Cycle, version 2 (RUC2) forecast at 0000 UTC 10 Mar 2006, which is the case study period in section 4, Fig. 2 of KMW. Negative values of frontogenesis found over much of Illinois indicate frontolysis.

In summary, PSZ's attempt to explain away KMW's results as being the result of slow balanced dynamics appears to contain serious flaws. Frontogenesis has long been employed as a CAT diagnostic; its performance is not as good as the trial results of KMW; and in the case study motivating KMW's application, there was barely any frontogenesis to be found and KMW's Lighthill–Ford approach was far superior at identifying regions of reported turbulence.

This does not completely rule out the possibility of still other explanations. For example, perhaps PSZ can invent a frontolytical theory of CAT generation. But the connection between gravity waves and CAT is much better established (e.g., Lu and Koch 2008) and better in application than anything PSZ have so far proposed, and it is a much more plausible explanation for KMW's results.

6. How firm a theoretical foundation?

In KMW we stated that "our hope is that our approach may be used to place the subject of CAT forecasting on a firmer theoretical footing." PSZ, perhaps unaware of the history and current practice of CAT forecasting, take issue with this statement. The earliest CAT forecasting methods, still used in somewhat modified form today, relied on spatial pattern recognition of synoptic-scale flows, with limited reference to dynamical principles and almost no reference to gravity waves. Some of the most successful methods used today (e.g., Ellrod and Knapp 1992) are primarily kinematic rather than dynamical in nature. GTG, the best operational CAT forecasting approach today, emphasizes statistical skill rather than dynamical clarity with regard to the processes that lead to CAT. In other words, there is still a considerable gap between dynamical thinking and operational practice in CAT forecasting.

Our work in KMW is, to our knowledge, the first attempt to develop a CAT forecasting approach "end to end," starting from first dynamical principles of gravity waves and concluding with a diagnostic usable in an operational forecasting context. KMW's approach would surely be acknowledged by a dispassionate observer as a dynamical advance in forecasting beyond the pattern-recognition methods still in use (e.g., see "Map Patterns and CAT" in Lester 1994, 4–31). Indeed, the analysis in section 2c of KMW linking the Lighthill–Ford forcing terms with other CAT forecasting diagnostics is already stimulating new lines of research regarding existing methods (G. P. Ellrod 2009, personal communication). Rather than KMW's success being attributable to existing CAT diagnostics, the converse appears to be true: KMW may provide the foundation for new and better methods of CAT forecasting. Therefore, despite still-unanswered questions regarding both spontaneous imbalance and CAT forecasting, we reassert our hope, carefully stated, that our work in KMW has placed CAT forecasting on a firm*er* theoretical footing.

7. Conclusions

We thank PSZ for the opportunity to restate and clarify the motivations and results of our work. The spontaneous generation of gravity waves is a nascent area of research in atmospheric dynamics, with the inevitable lacunae in understanding. Through these and other exchanges, we hope that these gaps in knowledge may be identified and eventually eliminated.

Far from being an isolated "misinterpretation" of gravity wave generation theory, KMW follow in the footsteps of other experimental and theoretical works that have established similar linkages between Lighthill–Ford theory and gravity wave generation for small-Ro flows. The successes of WHR05 and KMW in making these linkages in both experimental and observational contexts imply that the theoretical objections raised throughout PSZ may have less real-world relevance than is apparent at first sight. PSZ's alternative explanation for the success of KMW's forecasting approach instead supports our claim that the forcing terms are related to gravity wave generation. Faced with the choice of explaining KMW's success via a gravity wave generation theory pushed beyond its formal limits, versus an alternative *deus ex machina* hypothesis that could require frontolysis as a CAT mechanism, we choose the former. We leave it to the reader and to the atmospheric sciences community to determine whether PSZ's judgment that our interpretation is "not founded and . . . does not bring any element to the debates on the generation of gravity waves or on their role in producing CAT" has any merit whatsoever.

We thank PSZ for their encouraging comments regarding the work of KMW as a CAT forecasting method. We, in turn, encourage additional collaboration between the experimental fluid dynamics, theoretical atmospheric dynamics, and operational aviation forecasting communities to help to shed light on this developing area of research and to help to make our theoretical understanding of it ever more firm.

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