Reply
to Comments by S. Saujani and T. G. Shepherd on "Balance and the Slow Quasimanifold: Some Explicit Results"

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1. Lighthill's idea

Lighthill's most important idea is in our opinion the following, when expressed in a form appropriate to geophysical fluid dynamics. The idea is that the spontaneous-adjustment[1] emission of inertia–gravity waves by unsteady vortical motion is sufficiently weak, in parameter regimes of interest, that the emission may be neglected when solving for the vortical motion. The *sufficient weakness*,

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[1] As in Ford et al. (2000), we (a) distinguish spontaneous adjustment from Rossby or initial-condition adjustment, and (b) avoid the term "geostrophic adjustment", since an adjustment toward balance is often an adjustment away from geostrophy. The example of a circular vortex adjusting toward ageostrophic, gradient-wind balance is enough to illustrate the point. The self-contradictory notion of "ageostrophic geostrophic adjustment" may be compared to other such notions, which tend to invade human language, such as "variable solar constant", "asymmetric symmetric baroclinic instability", and "fuzzy manifold".

in this sense, of the wave emission or "Lighthill radiation", as we also called it, is the fundamental reason — transcending all details of mathematical method and physical mechanism — why the concepts of balance, balanced model, slow quasimanifold, and potential-vorticity inversion are useful in parameter regimes of interest. The weaker the emission, the more useful the concepts.

Lighthill's idea also provides the surest way of seeing that these same concepts, in their full fluid-dynamical context, beyond low-order models, have ultimate limitations throughout the parameter regimes of interest. Though useful, indeed sometimes astonishingly accurate, these concepts are also inherently approximate. That was the key message of the Ford et al. (2000) paper. In other words, Lighthill's idea is the surest way of recognizing that the existence of a strict slow manifold — implying the exact "slaving of fast variables" and exact versions of all the above concepts — is overwhelmingly improbable.

For if the wave emission is so weak that one can regard the vortical motion as known, or knowable, independently of the emission to good approximation, then one can also regard the source term for the emission as known to good approximation, as soon as one knows the vortical motion. One can then confirm, as in the original analysis of Lighthill (1952) and in those that followed (e.g. Crow 1970; Crighton 1975, 1981, & refs.), taking note of the special quadrupole form of the source term, that the emission is indeed weak in parameter regimes of interest, yet almost always nonvanishing. See also Section 4 below, regarding the function $\alpha(t)$ defined in Ford et al.'s Eq. (54).

The *weakness* of the emission makes the whole picture self-consistent, as Lighthill recognized: the emission is, in his own words (op. cit., p. 565), "so weak relative to the motions producing it that no significant back-reaction can be expected." The *nonvanishing* of the emission, on the other hand, precludes the exact slaving of fast variables.

Thus it is the sufficient weakness of the wave emission that is the key — both to Lighthill's original analysis and to its generalization to geophysical fluid dynamics. The question of exactly how weak is a secondary question, from this viewpoint, interesting but less fundamental. It was with all these points in mind that we wrote in Section 2 of Ford et al. that *"the emission is very weak... helping to explain why balance and potential-vorticity inversion, though inherently approximate, can be far more accurate than might be suggested by the standard order-of-magnitude considerations and filtered balanced models."* And Lighthill's idea applies even more powerfully in the context of geophysical fluid dynamics than in its original context, aerodynamic sound generation, because *"Coriolis effects can be expected to weaken the emission still further"* (Ford et al., p. 1237b). The weaker the emission, the more secure the whole picture!

If, as speculated by Saujani and Shepherd (2002, hereafter SS), the further weakening manifests itself as exponential rather than algebraic smallness in the limit of small Rossby number R — as concretely illustrated by the Ford (1994) example quoted both by SS and by ourselves — then in that particular parameter regime the power of Lighthill's idea is very great indeed. Exponential smallness is, of course, far smaller — almost unimaginably smaller — than anything one might guess from "standard order-of-magnitude considerations and filtered balanced models".

We agree with SS that exponential smallness is generally speaking plausible, in the small-R limit, though unproven. A proof would require one to prove among other things that some counterpart of the function $\alpha(t)$ is infinitely differentiable.

2. Parameter regimes

In the thinking that led to the Ford et al. work, we were interested in all the parameter regimes where Lighthill's idea is applicable, i.e., in all regimes, including limiting cases and other cases, where spontaneous-adjustment emission is in some sense weak, not just the particular small-R limit discussed by SS. That small-R limit is, as they mention, the limit involved in a particular filtered model, the standard extratropical quasigeostrophic theory. We may specify the limit more precisely, following SS and using their notation and definitions, as $R \to 0$ with $L \sim L_R$, implying $F \to 0$ with $R \sim F$. Here L_R is the radius of deformation, F is the Froude number, and the flow is assumed, rightly or wrongly, to have a single lengthscale L.

A specific reason for our interest in a wider range of parameter regimes was the existence of the astonishingly accurate results described in Norton (1988) and in McIntyre and Norton (1990, 2000). These came from high-order balanced models of shallow-water flows on a hemisphere with strong, unsteady vortical activity deep within the tropics, showing initial-condition sensitivity. We were therefore especially concerned to include parameter regimes appropriate to the tropics in our thinking.

Standard quasigeostrophic theory is grossly inaccurate for these flows. As remarked in McIntyre and Norton (1990), a geostrophic computation for one of them "gives maximum velocities typically wrong by a factor ~ 2." That is no great surprise: although $L \sim L_R$, flow speeds are so high that R and F are anything but numerically small. Numerically, F attains values of 0.7 or more, in subtropical jets, and R is typically somewhere near unity, and of course infinite at the equator. For these flows there is no parameter limit, no rational hope of using asymptotic methods, no clear timescale separation and therefore no clear distinction between "fast motion" and "slow motion".

What was, by contrast, a great surprise, and remains noteworthy and very remarkable, is that the high-order balanced models are exquisitely accurate even for the flows just mentioned. By exquisitely accurate we mean that cumulative accuracy, over several eddy turnaround times, was such as to produce a final potential-vorticity distribution nearly indistinguishable from that of the corresponding primitive-equation evolution. Yet these highly unsteady flows involve vortex-merging events and were explicitly demonstrated to be initial-condition sensitive. The results can therefore be viewed as stunning vindications of Lighthill's idea, as understood here and in Ford et al., showing, quite unexpectedly, the power of that idea in circumstances far wider than the circumstances originally considered by Lighthill himself ($F \to 0$ with $R = \infty$, in the present notation) and far wider than the circumstances considered by SS ($F \to 0$ with $R \sim F$) — indeed, as already emphasized, far beyond the reach of any asymptotic analysis whatever.

3. The parameter limit $F \to 0$ with $R \gtrsim 1$

Why Lighthill's idea should be quite so powerful is still an unsolved mystery, though clearly it must be related to the short-range character of potential-vorticity inversion operators when $R \sim F \sim 1$ (e.g., McIntyre 2001). Faced with that mystery, we opted in Ford et al. to consider another parameter limit, $F \to 0$ with R constant, formally $R \gtrsim 1$ (as stated in our abstract and on pp. 1238b and 1239b). This was not because we thought it the only interesting parameter regime, but first of all because it is arguably relevant to the tropics, and second because it allowed spontaneous-adjustment emission to be analysed in a precise and detailed way, building on the work of Crow (1970) for $R = \infty$.

In considering so subtle and so surprisingly weak a process as spontaneous-adjustment emission, it seemed to us that there would be great value in having a class of examples that would allow us to look at the mechanistic details, with explicit representations of the very weak back-reaction or radiation reaction of the emission upon the vortical motion, and of the interplay of multiple spatial scales — an interplay that is excluded by low-order, spectrally truncated models, as SS recognize, but liable to occur in the full fluid-dynamical context because of the way in which frequency matching works. Such explicit and detailed examples would be valuable, we thought, even if unable to cover the entire parameter space of interest.

Frequency matching is not, of course, a matter of choice. It is simply an automatic and inescapable property of any wave emission process. Any waves emitted must have frequencies that match frequencies in the spectrum of the wave source. One might say that when one hears a Mozart symphony the air carrying the sound has performed "multiscale frequency matching" with the source; it is "multiscale" for the bass notes at least, whose wavelengths can dwarf the size of the sound source. Alternatively, and more clearly and simply, we think, one might say that the frequency spectrum of the waves emitted is governed by the (known) frequency spectrum of the source, having regard to the possible range of frequencies of freely propagating waves.

We therefore, having regard to the range of frequencies of freely propagating inertia–gravity waves, agree with SS and with Errico (1981) that the strength of the wave emission in the limit $R \to 0$ depends exclusively on the high-frequency tail of the source spectrum. That is why, after the remark that *"Coriolis effects can be expected to weaken the emission still further,"* we went on to say that Coriolis effects would *"not…make it exactly zero, even for arbitrarily small Rossby number…because of the expectation that typical vortical flows, being chaotically unsteady,…will have a frequency spectrum with no high-frequency cutoff."* The absence of a high-frequency cutoff has never been proven rigorously, to our knowledge, but seems overwhelmingly probable for chaotic vortical flows.

4. Cases in which R is numerically small

Although the detailed analysis in Ford et al. does not formally cover the limiting case $R \to 0$, the analysis does, arguably, point toward what must happen in such cases. It does so through the complex-valued function $\alpha(t)$ defined in Eq. (54). The frequency spectrum of $\alpha(t)$ governs the leading-order wave emission. The definition says that $\alpha(t)$ has real and imaginary parts proportional to certain second spatial moments of the potential-vorticity distribution, quantities that are nonvanishing, and temporally fluctuating, for all vortical flows outside a tiny set of exceptional cases. The reader who would like to see this last point illustrated in more detail may consult the recent work of Bridges and Hussain (1992, 1995), in which a function closely analogous to $\alpha(t)$ is examined, together with the associated wave emission, both theoretically and through high-precision laboratory experiments on three-dimensional, axisymmetric aerodynamic sound generation.

The dependence of $\alpha(t)$ exclusively upon second spatial moments stems directly from the multiscale spatial structure of the emission problem, and from the fact that the zeroth and first moments cannot fluctuate and hence cannot contribute to wave emission. The zeroth and first moments are constrained to be steady by the laws of free vortex motion, indirectly expressing the quadrupole nature of the emission source. The third and higher moments are relevant but only as small corrections. All this structure is present in the emission problem as soon as F becomes small. Ford et al.'s analysis does cover cases in which R is numerically small, provided only that R is bounded away from zero as $F \to 0$. In the usual manner of an asymptotic analysis, the formal condition conventionally written as $R \gtrsim 1$ has no absolute numerical significance when we take the limit $F \to 0$.

Making R numerically small tends to reinforce, not to diminish, the multiscale structure. For given $\alpha(t)$ the emitted wavelengths are lengthened, not shortened, and the dominance of second moments should be enhanced. This is a direct consequence of the dispersion properties of inertia–gravity waves. It is therefore reasonable to speculate that even for arbitrarily small R the strength of the emission will continue to be governed to some first approximation by the function $\alpha(t)$ or, more precisely, by its Fourier transform $\tilde{\alpha}(\omega)$, in the frequency range $|\omega| \geqslant f$ of freely-propagating inertia–gravity waves, where f is the Coriolis parameter. If $\tilde{\alpha}(\omega)$ has an exponentially decreasing tail as $|\omega| \to \infty$, as seems likely for chaotic vortical flows, then we have a clear pointer not only toward exponential weakness of the emission but also, again, toward its nonvanishing for arbitrarily small R, precluding the exact slaving of fast variables.

5. Velocity splitting

Cases like those mentioned in Section 2 pose an even greater challenge to our understanding. When neither F nor R can be considered small, the multiscale spatial structure disappears. Asymptotic methods are no longer applicable, Lighthill's idea has no formal justification, and the function $\alpha(t)$ is sure to be replaced by something much more complicated. Why Lighthill's idea nevertheless seems to survive as numerically valid in such extreme circumstances remains an unsolved mystery. There is no a priori expectation that spontaneous-adjustment emission is necessarily weak even though, in cases studied so far, we have striking evidence that it is, nevertheless, weak in some numerical sense, as mentioned in Section 2. That evidence comes solely from numerical experiments.

However, the other aspect of the problem with which we were concerned, the nonvanishing of the emission for unsteady vortical motion, and its most fundamental implication, the nonexistence of a strict slow manifold, has recently been illuminated from an unexpected new angle that may be worth mentioning briefly.

The new insight comes via the so-called "velocity splitting" phenomenon, first noticed in the context of Hamiltonian balanced models (Salmon 1988, McIntyre and Roulstone 2002, & refs.), but now known to be a general property of high-order non-Hamiltonian balanced models as well (Mohebalhojeh and McIntyre, in preparation). By directly analysing the (rather complicated) equations defining a general class of high-order balance conditions and potential-vorticity inversion operators — which includes those that produced the accurate results mentioned in Section 2, and those that produced the further such results of Mohebalhojeh and Dritschel (2001) — we have found that such models are inherently "schizophrenic" in that they possess not one but two velocity fields. One field advects the potential vorticity while the other advects the mass; the two fields are nearly but not quite equal.

The key point is that such velocity splitting or schizophrenia is not, as one might at first think, merely the result of imperfections in formulating the high-order balance conditions. Rather, it is an inherent property of any balance condition and potential-vorticity inversion operator that exceeds a certain standard of accuracy. The critical accuracy is that of the well known Bolin–Charney balanced model. The splitting of the velocity field for any balanced model of greater accuracy may seem paradoxical until one recognizes it as simply another strong line of evidence against the existence of a strict slow manifold, and against the vanishing of spontaneous-adjustment emission. A fuzzy *slow quasimanifold* or stochastic layer, regarded as a constraint on the motion, is itself, so to speak, schizophrenic; and this becomes noticeable as soon as one is computing with enough accuracy to see the fuzziness.

6. Remark concerning SS Figure 1

We note finally that, in interpreting SS Figure 1b, the sloping lines $\omega = \omega_V$ need to be pictured as being smeared out, filling the entire figure upward and leftward and thus showing the overlap of the hyperbola $\omega = \omega_G$ with the weak but infinitely broad spectral tail appropriate to chaotic vortical motion. This is necessary in order that the figure express the relevant frequency matching. As in all wave emission problems, as already noted, the frequencies of waves emitted must agree with frequencies in the wave source spectrum. The symbol "$\sim$" in SS Eq. (1), replacing the symbol "=" and acknowledging the "nonlinear broadening of the zero-frequency linear mode", needs to be, as it were, stretched out infinitely in order to acknowledge the infinite nonlinear broadening involved in the spectral tail.

Postscript by MEM: The foregoing may well be Rupert Ford's last scientific publication. It has had to be revised in his absence, in response to the final revision of SS, using extensive notes made during our last co-authors' consultation. The revision tries to echo Rupert's wonderful spirit of generosity, engagement, and enthusiasm, and above all tries to be something like what would have emerged had he still been with us — conveying some sense of his penetrating insight and rigor and of his joy in serious intellectual endeavour, of his joy in trying to bring understanding to a difficult problem area. There are a few more remarks about Rupert's life and about his brief yet brilliant research career, so tragically cut short in March 2001, in the obituary published in the *Quarterly Journal of the Royal Meteorological Society*, **127**, 1489–90, April 2001 B. The Royal Meteorological Society, in which Rupert was active as Secretary of the Dynamical Problems Specialist Group, now administers a Rupert Ford Memorial Fund supporting travel and exchange among young scientists of any nationality.

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