

Interactive comment on “On the role of thermal expansion and compression in large scale atmospheric energy and mass transports” by Melville E. Nicholls and Roger A. Pielke Sr.

Melville E. Nicholls and Roger A. Pielke Sr.

melville.nicholls@colorado.edu

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Main points

“1. As mentioned above, repetition of previous work should be minimized and the new results emphasized.”

We do think that it is good idea to summarize some of our previous results although perhaps we can shorten this to some degree. Reactions to our previous published work on this topic have on the whole been quite negative. It is clear that the idea that compression waves could be transporting considerable amounts of total energy at the

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speed of sound is regarded with a high degree of skepticism. Moreover most climate researchers who study energy transport seem to be unaware of this possibility. We set out in this paper to make a very clear case that there could be a significant large-scale transfer of dry static energy at the speed of sound. We think that presenting the basic ideas more succinctly helps readers to follow the paper without having to read in detail the previous articles. The point is well taken though and we should emphasize what is new about these results. In particular, while it was difficult to modify the model for double precision this has enabled larger scale simulations to be conducted and a good match between energy input and the net change in the total energy field to be achieved. By showing vertically summed energy fields it is apparent that there is transfer occurring at the speed of sound. This is important to establish since large-scale thermal compression waves have not been observed in the atmosphere yet and their existence and effect on energy transport is at this stage purely a prediction based on solutions of the fully compressible Navier-Stokes equations. We have also shown that for a very large-scale heat source there is a fairly small but nevertheless significant difference between a simulation that includes thermal compression waves and one that doesn't. The effects of Coriolis force have been shown to be significant and we have introduced the idea of a Rossby radius of deformation for these fast moving waves. We have shown in detail what happens when there is a convective-scale heat source in a fully compressible model and compared to results for a model that behaves more like an anelastic system. The case has been made that using semi-implicit time differencing methods that slow down thermally generated compression waves and gravity waves might be leading to larger inaccuracies than expected. These new results present a more significant challenge to the traditional view of total energy transport than our previous studies suggesting the current theory is incomplete.

"2. It would be useful to relate the ideas and experiments discussed here to the ideas of hydrostatic and geostrophic adjustment, e.g., Gill A. E. (1982), pp 191-, Bannon, P. R., (1995) *J. Atmos. Sci.*, 1743-1752. In particular, Gill (p194) discusses the energy carried away by Poincare waves in the (shallow water) geostrophic adjustment

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process."

This is an interesting suggestion. There are similarities between the analytical thermal compression wave solution and shallow water solutions. The energy discussion by Gill (p194) is based on wave energetics. For the linearized compressible equations a wave energy equation can be derived (e.g. Nicholls and Pielke 1994a, Eq. 36). In that manuscript it was shown in equations (37) and (38) that the wave potential energy is far smaller than the perturbation internal energy in a thermal compression wave. So two energy conservation equations can be derived, one that is wave energy (wave potential energy plus kinetic) and one that is total energy (internal plus kinetic), and the wave energy is far smaller in magnitude. It would be interesting to consider the geostrophic adjustment process for thermally generated compression waves in a more simplified theoretical framework.

"3. The full version of equation (15) involves the initial profiles $\bar{\rho}$ and $\bar{\theta}_v$, so some approximation to the full fluid dynamical equations seems to be involved. This then raises the question of whether the full version of (15) has exact, or only approximate, mass and energy conservation properties. These properties seem to be crucial to the whole exercise, but I wasn't able to quickly track this information down by looking at the references given."

Equation (15) does indeed use approximations. These are appropriate for regional atmospheric modeling simulations as long as the departure from the basic state is small. For the simulations in this paper these approximations are reasonable and energy conservation is not compromised too much. However if this model was used to simulate large-scale baroclinic eddies then modifications to the model equations would probably need to be made to obtain accurate mass and energy conservation. For instance, there are large meridional potential temperature differences from equator to pole and for a horizontally homogeneous basic state the potential temperature perturbations would be large and the departure from the basic state would not be small.

“4. The full version of (15) is compared with one that omits the first three terms on the RHS. This omission eliminates the thermal compression waves under study, but at the cost of sacrificing mass and energy conservation. Strong arguments have been made that numerical models, should solve ‘dynamically consistent’ equation sets, retaining appropriate conservation laws, including mass and energy, even when those equation sets are approximate. For example, the hydrostatic equations and at least some versions of anelastic and pseudo-incompressible equations are dynamically consistent in this sense, and they are widely used. It would therefore be of great interest to understand how such equation sets respond to a local diabatic heating, and how the energy budget is to be interpreted in such models. (It is conceivable that a similar adjustment process occurs to that in the fully compressible case, but instantaneously rather than at the speed of sound.) I believe that a comparison of the full equation set with hydrostatic and/or anelastic equations would be of much wider interest than the artificial system that involves dropping the RHS of (15).”

We agree it would be of great interest to compare the full equation set with other approximate equation sets and understand how the energy budget is to be interpreted. As Klemp and Wilhelmson (1978) point out there are similarities of the system Eq. (15) without the terms on the RHS with the anelastic system. There has been considerable interest in the applicability of soundproof models to large-scale motions (e.g. Benacchio et al. 2015). It is not clear to us how other models that are not fully compressible and do not simulate Lamb waves that propagate at the speed of sound would respond to localized heat sources and whether the redistribution of total energy and mass would occur in a realistic manner. We hypothesize that a hydrostatic large-scale model that includes Lamb waves as solutions that are not slowed down would probably be conserving total energy and mass and in a similar way to a fully compressible large-scale model. This is certainly an area that should be investigated further.

“5. P3 lines 16-17: ‘This decomposition tacitly makes the assumption...’ Actually the decomposition (3) is correct, but the tacit assumption is often made when interpreting

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(3). I think part of the problem is imprecise use of terminology in the community: the ‘transients’ in (3) morph into ‘eddies’ (a bit ambiguous) which morph into ‘turbulence’ (definitely wrong for the compression waves discussed here). See also section 2.4."

Yes this is correct and we should reword this sentence.

"6. Section 2.4. I think part of the problem is that, because of the historical use of acoustically filtered equation sets such as anelastic, or hydrostatic in pressure coordinates, the term ‘heat flux’ has become identified with the potential temperature flux, which in turn is closely related to the entropy flux. Thus the energy and entropy budgets have become confounded in our thinking. But, while entropy is carried along with the fluid, to a good approximation, energy is not. See also the discussion in Nicholls and Pielke 1994b. If the paper can help to clarify these issues then that would be a useful service to the community."

We agree that this is part of problem. It would be interesting to examine the entropy transports. We are not sure that there wouldn't be some entropy transport with the propagation of thermal compression waves. However it appears that a quantity more related to the usual view of "heat flux" can be derived from the equation for entropy, which is basically a potential temperature flux. We took this approach in deriving an approximate conservation equation in Nicholls and Pielke (1994b, Eq. 21). Maybe there is some wider applicability of this approach to large-scale circulations but we have not examined this issue.

"7. I found the results section hard work. I think it could be better organized to emphasize the points the authors wish to make, and to help the reader to make comparisons between different experiments."

We will work on improving the organization of the results.

Minor points

"P6 line 14: frequency should be period (or change 5 min to $2\pi / 5$ min)."

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We will correct this.

"P7 line 16: greater than 0?"

Thank you for noticing this error.

"Section 2.7: I am perfectly happy with the idea that these disturbances are waves, and for me this section is unnecessary (though perhaps the authors have met some resistance to the idea and so feel that the section is necessary)."

We will think about this section further. We do think that while these disturbances have wave-like characteristics, there are aspects that are not normally associated with waves.

REFERENCES

Bennachio, T., W. P. O'Neill, and R. Klein 2015: A blended soundproof-to-compressible numerical model for small- to mesoscale atmospheric dynamics. *Mon. Wea. Rev.*, 142, 4416-4438.

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