Dr.
Editor
Geophysical Research Letters

28 July 2010

Re: 2010GL043834R (Editor -): Decision Letter

Dear Dr. :

Thank you for sending us the referees' comments concerning our work on the pressure gradients produced by condensation. We would like to respond to the concerns of Referee 3, as it can be easily shown they do not disprove any of our conclusions but are based on a misunderstanding. We feel that the importance of the potential implications of our study as well as the positive interest in our work shown by the other two referees provide grounds for you to re-consider your decision with regard to our manuscript.

The referee claims that Eq. (3) for condensation rate is incorrect because in the limit of pure vapor atmosphere, $N_v = N$, the condensation rate is equal to zero. We emphasize that Eq. (3) describes condensation rate in the adiabatically ascending air in an atmosphere with hydrostatic adjustment. The ascending air as a whole is assumed to be in hydrostatic equilibrium. If the atmosphere consisted of pure water vapor and is in hydrostatic equilibrium, no condensation will take place in the ascending saturated air. It is easy to see that the moist adiabatic lapse rate in this case would be too low to sustain condensation, as discussed on p. 9, Section 3, in our present manuscript. This issue was considered in greater detail by Makarieva and Gorshkov in a recent ArXiv.org preprint, see http://arxiv.org/abs/1003.5466, p. 10. So $S = 0$ for pure vapor atmosphere is correct under the assumptions to which Eq. (3) conforms and does not contradict any of our results.

(We note in passing that the limit $N_v \to N$ is not relevant for the real atmosphere where $N_v \ll N$. Many conventional meteorological formulae in this limit give incorrect results. For example, the widely-used formula for the moist adiabatic lapse rate (e.g., http://amsglossary.allenpress.com/glossary/search?id=moist-adiabatic-lapse-rate1) in this limit (with vapor mixing ratio tending

to infinity and dry air concentration to zero) gives a result that depends on the molar mass of dry air. Clearly, a pure vapor atmosphere does not "know" anything about the specific substances that are absent from it, so this result is obviously incorrect. However, as the limit of infinite mixing ratio of water vapor has little practical implication, this inconsistency is rarely discussed.)

The remaining arguments of the referee represent trivial algebraic operations performed on the continuity equation. We fully agree with the referee that if one does not input some independent physical content, one cannot obtain anything. This is illustrated by the referee, who shows that Eq. (3) cannot be obtained from the continuity equation *under any assumptions,* including such exotic ones adopted by the referee as putting $u = 0$ and $\partial w/\partial z = 0$.

This is due to the fact that the continuity equation does not contain any information about the condensation rate (the topic of our paper). It does not know whether condensation takes place at all. Indeed, the mass balance (continuity) equation is equally valid for condensation, evaporation or absence of phase transitions altogether ($S = 0$). It universally applies to any circulation event. Meanwhile our task in this paper is to study the effects of *condensation.* Thus, Eq. (3) specifies the rate of condensation to be used in the continuity equation. Eq. (3) says several things, in particular: (1) in the considered volume the only source of phase transitions is condensation; (2) this condensation is caused by the adiabatic ascent of moist saturated air (no condensation occurs if the air moves horizontally because of isothermal surface) and (3) that the moist saturated air is close to hydrostatic equilibrium. We stress that none of these fundamental physical assumptions are contained in the continuity equation as it is universal with regard to its general applicability. (In contrast, Eq. (3) would not be valid, for example, for the case of adiabatic *descent*, or for a motion along a non-isothermal surface, etc.)

In summary, our work is precisely in line with the perspective implicitly required by the referee. It is by *adding specific physical information* about the nature of the term S into the universal continuity equation that we have been able to obtain a new and important result. We hope that the apparent misunderstanding of our work by the referee is resolved by these clarifications. We would also

be grateful if you could share with us the feedback of Referee 2. We have spent considerable time answering their comments and value their interest in our work. Please, note that the present manuscript had been discussed in sufficient detail with Prof. prior to submission. Perhaps Prof. (if not yet) could be contacted as a referee and be asked to judge about the statements of Referee 3 and our reply.

Thanking you once again for your time and efforts we look forward to hearing from you in due course.

Yours very sincerely,

Anastassia Makarieva