Reply to Referee #1

I writing to give you my impression about this paper and reasons why I am declining to provide any further comments nor a detailed report as suggested for this work.
I think the paper is poorly written and most of its statements are wrongly and poorly motivated.

Reply: Sorry for my late reply. In fact, I'm wondering why you can make such totally negative comments on our work. I find your negative comments are mainly about the basic ideas following up our previous NPG paper (i.e. Zhao et al, 2011). These issues were fully discussed with two reviewers and editor of Zhao et al (2011). I think if the editor can still find their comments of that time, he will see very different comments from yours. I provided a copy of Zhao et al (2011) when I submitted the present paper, because this work is a continue of Zhao et al (2011). However, I'm not sure if you have received or read it. Even so, I'd like to explain further as below.

While the title and the core abstract suggest that this work is about meso- and synoptic-scale convection, there is nothing in its content that speaks about this subject.
I don't see how someone can claim on studying atmospheric convection without involving moisture and precipitation or even some kind of thermal forcing such as radiation and/or surface heat and/or moisture fluxes.

Reply: A fully moisture convection is too difficult issue and have never been understood well even by people of numerical model. But this does not need to mean we cannot use dry convection or some highly simplified moisture convection in the theoretical study of the *dynamical aspects* of meso-scale system (in the typhoon study of our paper we do introduced a simplified moisture heating). Otherwise, all early theoretical studies such as symmetric instability become meaningless. This is obviously not true. Are you sure theoretical studies on symmetric instability are nothing? If not, please check whether most of them consider moisture convection exactly or not.

At best this work is about stratified turbulence and/or nonlinear interactions between gravity waves and slowly evolving vortical flows. These two subjects have been extensively studied during the last few decades and the present work is far from making any new contribution of some kind.

Reply: I think this is the key point why our work is not understood properly. There are two kind of modes in the stratified atmosphere with unstable domain of stratification or other instability. Basically, unstable modes are regarded as convection modes, while stable modes the inertial-gravity waves. The work is mainly about interaction between (at least dry) convection modes (*turbulence is at too small scale)* and the slowly evolving vortical flows. It also deal with interaction between inertial-gravity waves and the slowly evolving vortical flows. Our new contribution are that : in the former case of convection modes, we performed a study on the triggering mechanism of convection, while in the latter case of inertial-gravity waves we reduced it into a simplified theory of balanced flow adjustment, which give potential to study typhoon properties in section 6.

The mathematical study which is based on asymptotic expansions and looks at possible resonant interactions between gravity waves and the vortical motion; this is standard in this business and the authors have nothing new to offer.

Reply: Yes, method of asymptotic expansions is nothing new, but it is used to study triggering mechanism of convection, which is new. Let me correct your above saying of "*resonant interactions between gravity waves and the vortical motion".* It's not gravity waves, it's convection modes. Don't you think a study on triggering mechanism of convection is new enough topic ? Moreover, the triggering is related to the unbalanced nature of vortical flow, such as unsteady flow, vorticity advection and density advection, and all these can have potential application in meteorological study.

Moreover, I have serious doubts that the present work is of any use. The equation they use to built their theory, I quote, "is not closed". I don't see how someone can claim growing or decaying and balanced or imbalanced solutions for a non closed equation.

Reply: The equation itself is not closed, but it is derived from a closed set of equations, which means all these equation are satisfied simultaneously. So our equation have same balanced/imbalanced solution with the original closed equations. I suggest also that you can read Zhao et al (2011) or the present paper about what we defined by "apparent instability" for growing or decaying.

I find referee #2 of this paper has a very good description about this issue, he says in his comments "the equation is somewhat complicated but nonetheless amenable to analysis of its expected behaviours". I can make a further explanation about that as below:

Since vorticity equation, divergence equation and thermodynamic equation are substituted into (1) in the derivation, constraints form basic dynamical and thermo-dynamical laws that one might expect in convective systems in the real atmosphere still work to a certain extent in (1). Nevertheless, equation (1) is more appropriately to be regarded as a diagnostic equation for the relationship between unbalanced basic flow and convection/IGWs. Although it alone is not closed and cannot serve as the governing equation to decide the motion, it does serve as one of the constraints for the motion. So, it may unable to describe all aspects of the motion, but it can describe qualitatively one aspect: relationship between unbalanced basic flow and convection/IGWs.

I'd like to give a simple example to explain what the difference between closed governing equations and a constraint (or diagnostic equation) is. The continue equation can be written as below

$$\delta = -\frac{\partial \omega}{\partial p} \qquad (a)$$

Here δ is the horizontal divergence, i.e.

$$\delta = \frac{\partial u}{\partial x} + \frac{\partial v}{\partial y}$$

If there is a motion with horizontal divergence $\delta > 0$, we cannot know vertical motion ω everywhere of any time, because (a) is not a closed governing equation, but as (a) is a constraint to the motion, we know there must be a convergent vertical motion with $\frac{\partial \omega}{\partial p} < 0$. This is also the case of our equation (1), which is a compound constraint of basic dynamical and thermo-dynamical laws, but is not closed governing equation. In typhoon study, it describes a imbalance forcing and balanced flow adjustment relationship, which can be explained mathematically as below. In typhoon study (for issues we discussed, there are strong balance), we use following equation

$$\frac{\partial^2 \delta_n}{\partial t^2} - c_n^2 \nabla^2 \delta_n + f^2 \delta_n = \Re_n(\varsigma, \phi)$$

it can be written identically as

$$\delta_n(\mathbf{r}, t) = \frac{c_n^2}{4\pi} \int_{c_n(t-t')>|\mathbf{r}|} \iint_{\Omega} \Re(\mathbf{r}', t') G(\mathbf{r} - \mathbf{r}', t - t') d\mathbf{r}' dt'$$

if r is far from meso-scale region, δn is nearly free wave, and dispersion of IGW demands $\delta n \to 0$ as $t \to \infty$. So the only way for that is $R(r,t) \to 0$ as $t \to \infty$. So adjustment tends to remove imbalance. This is what we can infer from a constrain or a diagnostic equation, without solving the closed governing equations.

Furthermore, the paper is poorly written and full of typos.

Reply: These can be improved (probably with the help of native English speaker).

For all these reasons, it must be rejected and I am reluctant to waste my time to write a detailed report to send to the authors or post online because it will counterproductive.

Reply: My only suggestion to editor is to consider comments of all reviewers of both the present paper and Zhao et al (2011).