Reply to Jaramillo et al. 2019’s comments in the Journal of the Atmospheric Sciences
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Jaramillo et al. (2018) (hereafter JMR1) asserted that our theory, henceforth known as CIAD (condensation-induced atmospheric dynamics), modifies the equation of vertical motion in a manner that violates Newton’s third law. Jaramillo et al. (2019) (hereafter JMR2) have subsequently conceded that this is not the case. This would have resolved the original misunderstanding, had it not been for new claims in JMR2 that necessitate further correction.

1. A ”potential” modification. JMR2 state that while CIAD does not modify the equation of vertical motion (indeed they were unable to locate any such modified equation), they nevertheless believe that CIAD should have done so. They state that adding $f_e$ to the equation of vertical motion ”follows logically” from Gorshkov et al. (2012)’s statement that ”the power of latent heat release which is $\xi$ times larger than the power of dynamic condensation-induced circulation”. This ”suggests” to JMR2 that $f_e$ must be added to the list of forces in the equation of vertical motion. JMR2 characterise this self-written proposal as ”a strong contradiction to their claim that CIAD does not modify the vertical momentum budget.”

This proposition is incorrect. A comparison of the rates of heat release and kinetic energy generation does not ”suggest” any modification of the equa-

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tions of motion. For example, in a close parallel to Gorshkov et al. (2012), Emanuel et al. (1994), following Charney and Eliassen (1964), emphasized that in hurricanes "the latent heat energy released is two orders of magnitude greater than the amount needed to maintain the kinetic energy against frictional dissipation". At the same time, Emanuel et al. (1994) warned against drawing any dynamic conclusions from such a comparison explaining that "the implication that heating per se leads to production of kinetic energy" is "an important misconception in the atmospheric science". We note that JMR2 have based their incorrect claim on this misconception.

2. $F_{vd} = f_e$. A second claim from which JMR2 retreat is that in hydrostatic equilibrium the evaporative force $f_e$ is equal to an "internal force" $F_{vd}$ exerted by vapor on dry air. Makarieva et al. (2019) (hereafter CIAD19) pointed out that $f_e = F_{vd}$ can only be obtained by misinterpreting the equations of motion – in particular, by believing incorrectly that the partial pressure gradient of water vapor (dry air) acts exclusively on water vapor (dry air).

JMR2 made no attempt to defend their $f_e = F_{vd}$ (this statement was not referred to again). Instead, JMR2 added an identical zero $F_{dv} + F_{vd} = 0$ to the equation of vertical motion (with neither $F_{dv}$ nor $F_{vd}$ specified). This procedure lacks a specified physical meaning but convinced JMR2 that $f_e$ "being defined as the force that the water vapor exerts on the air is covered in the definition of $F_{vd}$" and that $f_e$ is an "internal force". Note that one can add any expression simultaneously with the plus and minus sign and claim in a similar manner that $f_e$ is covered in the definition of whatever has been added. Without proper explanations and justifications such ill defined inferences serve only to confuse.

3. $f_e$ in the equation of vertical motion

A new claim by JMR2 is that CIAD19 "presents a strong contradiction proposing $f_e$ as a new force that exerts power, but at the same time, it is not present in the momentum budget". This statement is incorrect, since the fact that CIAD does not add $f_e$ to the vertical momentum budget does not mean that it is not already present there. Indeed, it is. Given that $\rho = p/(gh)$, where $h \equiv RT/(Mg)$, $\rho$ and $p$ is air density and pressure, $T$ is temperature, $M$ is air molar mass, we can write the equation of vertical motion as

$$\rho \frac{Dw}{Dt} = -\frac{\partial p}{\partial z} - \rho g = -\frac{\partial p_e}{\partial z} - \frac{p_e}{h} - \frac{\partial p_d}{\partial z} - \frac{p_d}{h} \equiv f_e + f_d. \quad (1)$$

Here $f_e \equiv -\partial p_e/\partial z - p_e/h$ the evaporative force as defined by Eq. (9) of CIAD19 and $f_d \equiv -\partial p_d/\partial z - p_d/h$. So again JMR2 make incorrect claims that may confuse rather than clarify the merits of CIAD.

4. Is there a rationale in JMR’s statements?

We have been in correspondence with JMR for an extended period (see
footnotes in our detailed comments in arxiv [http://arxiv.org/abs/1809.01874], so we have formulated a vision of how their claim could have been formulated more coherently (and we had long ago addressed that claim too). Looking at Eq. (1) one can ask, if work \( w_f \) per unit time is what provides positive power to atmospheric circulation, why does \( w_f \) deriving from the dry air distribution not provide an equal opposite in magnitude power such that the net impact of these two forces is zero? This question does not require new terms or confusing claims.

The answer lies in noting the implicit misconception of JMR1 and JMR2 that atmospheric power relates to all the work of all the forces in the momentum budget. The definition of atmospheric wind power \( K \) (which neither JMR1 nor JMR2 discuss) is \( K = -\int v \cdot \nabla p dV \), i.e. the integral over the atmospheric volume of the scalar product of wind velocity \( v \) and pressure gradient. The right-hand part of the equation of vertical motion contains, besides the vertical pressure gradient, also the gravity force; but it does not contain the horizontal pressure gradient. Whatever happens in the right-hand side of this equation, whether the forces sum up to zero or not, does not impact the atmospheric power budget. In other words, it does not follow from the fact that \( w(f_e + f_d) = 0 \) that \( K = 0 \). (For interested readers, in the Appendix we discuss a similar situation from the literature when work of the buoyancy force, despite this force is like \( f_e \) exactly compensated by other forces in the right-hand part of the equation of vertical motion, nevertheless determines the available potential energy for a hydrostatic atmosphere with adiabatic motions.)

As we have discussed elsewhere, see, e.g. Gorshkov et al. (2012, their Eq. 17) and Makarieva et al. (2013, p. 1047), the deviation of the dry air distribution from hydrostatic equilibrium is a consequence of the fact that the atmosphere as a whole is in hydrostatic equilibrium, \( f_d + f_e = 0 \), such that all power generated by the work per unit time of the evaporative force \( f_e \) is manifested in the horizontal plane. Acknowledgment, and appreciation, of this physical context would avoid the misconceptions seen in JMR1 and JMR2.

5. What is new in JMR2: ”parameterization”

JMR2 provide a new argument. They do not dispute that the main dynamic equation of CIAD,

\[
\frac{\partial p}{\partial x} = \mathcal{C}_{CIAD},
\]

where

\[
\mathcal{C}_{CIAD} \equiv -w_f \equiv w \left( \frac{\partial p^*_v}{\partial z} - \frac{p_v^* \partial p}{p \partial z} \right)
\]

is consistent with observations in different atmospheric contexts.

But now they argue that this result, Eq. (2), does not require a theory but follows from the continuity equation and ”a parameterization” of condensa-
tion rate $C$ of the type
\[ C_{CIAD} = \alpha C_z^*, \] (4)
where $\alpha \lesssim 1$ and $C_z^*$ is given by Eq. 14 of JMR2 evaluated for the saturated vapor pressure.

This claim is incorrect. As $p_v/p \ll 1$ the continuity equation does not possess a sufficient accuracy to determine $u \partial p/\partial x$ (see below). Thus, while the CIAD equation is consistent with both the continuity equation\(^1\) and the above approximate relationship, it cannot be derived from them.

Indeed, with $\alpha \approx 1$ the continuity Equation (17) of JMR2
\[ u \frac{\partial p}{\partial x} = \frac{p_d}{p_v} (1 - \alpha) C_z^* \] (5)
contains a product of a large factor $p_d/p_v \sim 10^2$ and an unknown small factor $1 - \alpha \ll 1$. Thus, assuming that a certain (a priori unknown) $\alpha \approx 1$ matches the observations, a mere 10% reduction of $\alpha$, while still obeying $\alpha \approx 1$, would lead to an order of magnitude overestimate of $u \partial p/\partial x$. Conversely, any $p_d/p < \alpha < 1$ will produce unrealistically low (down to zero) values of $u \partial p/\partial x$. Thus, the CIAD equation\(^2\) does not follow from the parameterization\(^4\) contrary to the statement of JMR2.

6. Conclusions
Despite their misunderstandings and criticisms of CIAD, JMR2 don’t dispute that CIAD’s ”main dynamic equation” is applicable to a wide range of atmospheric phenomena and ”describes wind and pressure profiles as consistent with phenomena like hurricanes and tornadoes, the circulation in the Amazon rain forest, or even an estimation of the global circulation power that appears to be consistent with observations.” Indeed, JMR2 now consider this equation ”interesting” and they have even attempted to propose their own (incorrect) explanation for why it matches the observations. (We note that if JMR now wanted to argue that the main CIAD equation is invalid, they would have to invalidate their own justification for it.) Before CIAD no universal constraint on atmospheric power was known, this alone makes CIAD worthy of constructive attention whatever views one may hold on its other merits. We are happy with this outcome and foresee many additional constructive developments in the future. We thank JMR1 and JMR2 for their efforts and for having drawn extra attention to our work and allowing us an opportunity to address misconceptions and clarify interpretations.

7. Appendix ”Work of buoyancy force in a hydrostatic atmosphere”
\(^1\)Note that while in both JMR2 and CIAD works the turbulent diffusion terms are formally neglected in the continuity equation, in fact turbulent diffusion is implicitly present in the condition $\partial p_v/\partial x = 0$, i.e. turbulent diffusion is what ensures constant relative humidity at the surface on an isothermal plane.
Lorenz (1955, 1978) proposed his formulations for available potential energy to drive winds that involve the differences in temperature and density between local air with pressure $p$ and density $\rho$ and a hydrostatic reference environment with pressure $\bar{p} = \bar{p}(z)$ and density $\bar{\rho} = \bar{\rho}(z)$ with

$$\frac{\partial \bar{p}}{\partial z} + \bar{\rho} g = 0. \tag{6}$$

Adding Eq. (6) to Eq. (1) and using the definition of the buoyancy force $f_b \equiv (\bar{\rho} - \rho)g$, we find that the equation of vertical motion (1) becomes

$$\rho \frac{Dw}{Dt} = f_b + \frac{\partial \bar{p}}{\partial z} - \frac{\partial p}{\partial z}. \tag{7}$$

Lorenz’ formulations were made for a hydrostatic atmosphere (dry or moist). Later it was shown that Lorenz’ moist available energy can be reformulated in terms of convective available potential energy, which is equal to the work of the buoyancy force on the rising air parcels (e.g., Emanuel, 1994, pp. 179-185).

However, precisely as $f_e$ (or indeed any other vertical force) is, by definition of hydrostatic equilibrium, compensated by the sum of all the other vertical forces, $f_e + f_d = 0$ in Eq. (1), the buoyancy force $f_b$ in a hydrostatic atmosphere is also compensated by the other forces, i.e. by the last two terms in Eq. (7). Nevertheless, despite this fact, the potential energy available for atmospheric motions is not zero. Under the approximations Lorenz made, it is equal to the work of the (compensated) buoyancy force. This is because in a hydrostatic atmosphere all processes in the vertical are compensated, but related in their magnitude to the wind power generated in the horizontal plane. For the same reason, Emanuel (1986) was able to formulate a description of buoyantly neutral hydrostatic hurricanes that was closely related to, and produced similar quantitative results as, the previous descriptions based on consideration of the storm’s positive buoyancy.

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