

# Comments on “Is condensation-induced atmospheric dynamics a new theory of the origin of the winds?” by Jaramillo et al. (2018)

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## 1 Introduction

Jaramillo et al. (2018) criticized our theory of condensation-induced atmospheric dynamics (CIAD). We value any such interest but, as we show below, Jaramillo et al. (2018)’s main statement, that CIAD modifies the equation of vertical motion such that it violates Newton’s third law, is unsupported. Contrary to their claims, CIAD does not make any “*modification to the vertical momentum budget*” (correct or incorrect) nor to any fundamental equations of hydrodynamics. (Despite claiming their assessment to be “*rigorous*”, Jaramillo et al. (2018) don’t locate the alleged equation in our publications.) Rather, as we summarize below, CIAD constrains the *power of atmospheric circulation* in a manner that is consistent with observations.

More specifically, we find that Jaramillo et al. (2018)’s analysis of the equation of vertical motion is invalid: it confuses the internal and external forces acting on a unit volume of air. The equations of motion for moist air are complicated (Trenberth and Fasullo, 2018), so confusions do occur. For example, the inconsistency between two conflicting equations of motions published within one year in same meteorological journal had persisted unresolved for over 15 years (Ooyama, 2001; Bannon, 2002). That controversy related to misinterpretations of Newton’s third law (see Makarieva et al., 2017b, their Fig. 1), which Jaramillo et al. (2018) perpetuate.

Jaramillo et al. (2018) correctly note that two different expressions for the evaporative force  $f_e$ , a key element of CIAD, occur in our publications. We use this opportunity to clarify these expressions. Finally, we clarify that there is no disagreement between CIAD and consideration of the atmosphere as a heat engine. Contrary to the claims of Jaramillo et al. (2018), these approaches address different problems and are complementary.

## 2 The equation of vertical motion

Jaramillo et al. (2018)'s statement can be summarized as follows. They write the equation of vertical motion as

$$\begin{aligned} F_z &= \left( -\frac{\partial p_d}{\partial z} - g\rho_d + F_{vd} \right) + \left( -\frac{\partial p_v}{\partial z} - g\rho_v + F_{dv} \right) \\ &= -\frac{\partial p}{\partial z} - g\rho + F_{vd} + F_{dv} = \rho a_z, \end{aligned} \quad (1)$$

where  $a_z$  is the vertical acceleration of air,  $g$  is the acceleration of gravity,  $p_d, p_v, p = p_d + p_v$  and  $\rho_d, \rho_v, \rho = \rho_d + \rho_v$  denote, respectively, pressure and density of dry air, water vapor and moist air as a whole. The terms grouped in parentheses are interpreted by Jaramillo et al. (2018) as “*the forces on each component*” – dry air and water vapor. Accordingly, forces  $F_{vd}$  and  $F_{dv}$  are defined as “*respectively the force of the vapor on the dry air and the force of the dry air on the vapor, as mediated by molecular collisions between the two components*”. According to Jaramillo et al. (2018), these “*internal forces*” must cancel due to Newton’s third law,  $F_{dv} = -F_{vd}$ .

Jaramillo et al. (2018) further state that “*if the air parcel is not undergoing vertical acceleration, then  $F_{vd} = f_e$ , as defined by (6)*”. In their notations this means that

$$F_{vd} = f_e \equiv -\frac{\partial p_v}{\partial z} - \frac{p_v}{h_v} = -\frac{\partial p_v}{\partial z} - \rho_v g, \quad (2)$$

since  $h_v \equiv RT/M_v g$ ,  $p_v = N_v RT$  (ideal gas law), and  $\rho_v = M_v N_v$ , where  $R$  is the ideal gas constant,  $T$  is temperature,  $N_v$  and  $M_v$  are molar density and molar mass of water vapor. From this statement, Jaramillo et al. (2018) proceed directly to their conclusion that “*the flaw [in CIAD] is now clear*”: it “*includes  $F_{vd}$  in the vertical motion equation while omitting  $F_{dv}$* ”, which represents “*a clear violation of Newton’s third law*”.

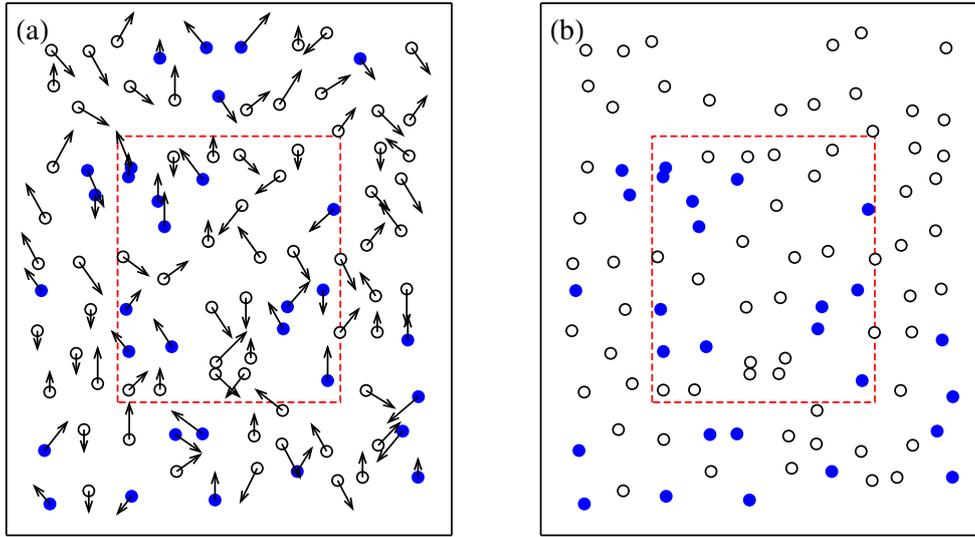
However,  $F_{dv}$  and  $F_{vd}$  cancel and thus cannot be retrieved from Eq. (1). Despite claiming their approach to be “*rigorous*”, Jaramillo et al. (2018) themselves do not explain how their central statement – Eq. (2) – was obtained. We speculate that they might have separated the equation of motion (1) into two “*component*” equations, for water vapor and dry air,

$$\rho_v a_{zv} = -\frac{\partial p_v}{\partial z} - \rho_v g + F_{dv}, \quad (3)$$

$$\rho_d a_{zd} = -\frac{\partial p_d}{\partial z} - \rho_d g + F_{vd}, \quad (4)$$

where  $a_{zv}$  and  $a_{zd}$  are vertical accelerations of water vapor and dry air. Then “*if the air parcel is not undergoing vertical acceleration*”,  $a_{zv} = a_{zd} = 0$  and Eq. (2) follows from Eq. (3) and the assumed  $F_{dv} = -F_{vd}$ .

The problem with this assumed derivation is that Eqs. (3) and (4) are incorrect. It is possible to write separate equations of motion for such components of moist air as gas and condensate including their mutual interaction governed by Newton’s third law, see, e.g., Eq. (23) and Fig. 1 of Makarieva et al. (2017b). This exercise requires, however, a correct identification of the external forces acting on the two components. In the case of Eqs. (3) and (4), while it is true that gravity acts separately on dry air and water vapor and is, respectively,  $\rho_d g$  and  $\rho_v g$ , it is an error to assume that  $\partial p_d / \partial z$ , the partial pressure gradient of dry air, acts exclusively on dry air, while the partial pressure gradient of water vapor,  $\partial p_v / \partial z$ , acts exclusively on water vapor (Fig. 1).



**Figure 1.** Momentum exchange between gas molecules (open circles – dry air, filled circles – water vapor, dashed frame denotes the considered unit volume). (a) The cartoon is a reminder that all types of molecules collide with each other (arrows show the chaotic velocities of molecular motion) and there is no rule that dry air (water vapor) outside the volume only collides with dry air (water vapor) within the volume as implied by Eqs. (3) and (4); (b) The gradient of water vapor is perturbed from the initial equilibrium state (a) by an instantaneous removal of water vapor from the upper forth of the vessel; the gradient of dry air is not perturbed; within the unit volume nothing changes either – in particular, interactions between the molecules remain the same; for clarity, molecular velocities are not shown. In this case, according to Eqs. (3) and (4), only water vapor will accelerate upward to fill the void, while the dry air as a whole will remain motionless. This absurd conclusion results from the incorrect identification of external forces in Eqs. (3) and (4).

Borrowing the words of Jaramillo et al. (2018), “*as mediated by molecular collisions between the two components*”, these forces are not separable. The total pressure gradient  $\partial p/\partial z$  is an external force acting on a unit volume of air. Molecules of all gases adjacent to the volume collide and exchange momentum: dry air and water vapor molecules outside the volume collide with both dry air and water vapor molecules within it. The difference in the rate of these collisions across the volume is what determines the vertical pressure gradient  $\partial p/\partial z$ . Figure 1 illustrates this basic point.

Since external forces in Eqs. (3) and (4) are incorrectly specified by Jaramillo et al. (2018), Eqs. (3) and (4) are also incorrect as equations of motion, i.e. the sum of the forces in the right-hand side of these equations, taken per unit mass, is not equal to accelerations  $a_{zv}$  and  $a_{zd}$ . Therefore,  $F_{vd}$  cannot be retrieved from the condition  $a_{zd} = a_{zv} = 0$  and remains unspecified. The statement of Jaramillo et al. (2018) summarized by Eq. (2) – that the evaporative does force  $f_e$  introduced by Makarieva and Gorshkov (2007) is related to the internal interactions between dry air and water vapor within the considered unit volume of air – remains unsupported. And with  $F_{vd}$  unspecified, the main conclusion of Jaramillo et al. (2018) that CIAD “*includes  $F_{vd}$  in the vertical motion equation while omitting  $F_{dv}$* ” does not have any grounds.

Furthermore, Jaramillo et al. (2018) provide no evidence that CIAD modifies the equation of vertical motion *in any way* (NB: it does not). Jaramillo et al. (2018) did not quote any equation from our publications that would support their statements. Rather, they incorrectly attributed their Eq. (11) to Gorshkov et al. (2012) (again without giving an equation number)<sup>1</sup>.

Everywhere in our works the equation of vertical motion is used in the form, using notations of Jaramillo et al. (2018),  $\rho a_z = -\partial p/\partial z - \rho g$ , see, for example, Eq. (15) of Makarieva and Gorshkov (2007), where  $\rho a_z = -\partial p/\partial z - \rho g = f_e$ , and Eq. (19) of Makarieva et al. (2013c), where  $\rho a_z = -\partial p/\partial z - \rho g = 0$  (hydrostatic equilibrium).

### 3 CIAD and potential energy

One key element of CIAD correctly identified by Jaramillo et al. (2018) is the non-equilibrium vertical distribution of atmospheric water vapor. Due to the condensation that occurs in the rising air and removes water vapor from the gas phase, the negative partial pressure gradient of saturated water vapor is several times larger than the weight of a corresponding amount of moist air. Makarieva and Gorshkov (2007, their Eqs. 15, 16) proposed that the corresponding vertical force, termed “*evaporative force*”  $f_e$ , “*drives the global circulation*”:

$$\begin{aligned} f_e &\equiv -\frac{\partial p_v}{\partial z} - \rho_v g = \frac{p_v}{h_c} \left( \frac{h_v - h_c}{h_v} \right), \\ h_c &\equiv \frac{RT^2}{L\Gamma} \ll h_v \equiv \frac{RT}{M_v g}, \end{aligned} \quad (5)$$

where  $L$  (J mol<sup>-1</sup>) is the latent heat of vaporization and  $\Gamma \equiv -\partial T/\partial z$ . Makarieva and Gorshkov (2007) backed up this proposition with the observation that in diverse circulation patterns, irrespective of their size and geometry (e.g. tornadoes versus Hadley circulation), the pressure difference governing air motion is of the order of 10 hPa – coinciding, in the order of magnitude, with the partial pressure of water vapor at the surface  $z = 0$

$$\Delta p(z) \equiv \int_z^\infty f_e dz \lesssim p_v(z). \quad (6)$$

This remarkable universality of atmospheric motions was noted by Holton (2004, p. 4), but it has not been reflected in theories of atmospheric circulation.

While Makarieva and Gorshkov (2007) proposed  $f_e$  as a dominant driver of circulation, providing a testable quantitative framework was left to subsequent publications. The magnitude of  $\Delta p$  (6) (J m<sup>-3</sup>) was interpreted as the local store of potential energy available for conversion to kinetic energy (Makarieva and Gorshkov, 2009a, b, 2010). A possible analogy is a spring compressed from an equilibrium state with length  $h_v$  to  $h_c < h_v$ ; this spring decompresses in the upward direction until Hooke’s force associated with its deformation (“ $-\partial p_v/\partial z$ ”) becomes balanced by spring’s weight (“ $-\rho_v g$ ”).

<sup>1</sup>We note that if the last equality in Eq. (1) ( $= \rho a_z$ ) is absent, this equation ceases to be the equation of vertical motion. It becomes a definition of a certain vertical force  $F_z$  with an unspecified relation to vertical acceleration. For example, gravity  $-\rho g$  is a *vertical force*, but it is not *the total vertical force*  $F_z$  that determines the vertical air acceleration in (1), i.e.  $-\rho g \neq \rho a_z$ . We suspect that the vertical force  $f_e/\rho$  has been misunderstood by Jaramillo et al. (2018) as equivalent to the vertical acceleration  $a_z$  in the equation of motion.

The magnitude of potential energy depends on how the state with minimum potential energy is defined (Lorenz, 1955). Makarieva and Gorshkov (2007) considered as such a static atmosphere where every  $i$ -th gas with partial pressure  $p_i$  and molar mass  $M_i$  has its own scale height  $h_i \equiv -p_i/(\partial p_i/\partial z) = RT/M_i g$ . However, in the real atmosphere already in the presence of small vertical motions (but in the absence of condensation) the air is well mixed in the vertical: its molar mass  $M$  is independent of altitude and all gases have same scale height  $h_i = h = RT/Mg$ . Accordingly, in later CIAD publications the definition of the evaporative force (also termed the “*evaporative-condensational*” or “*condensational*” force) was modified, with  $h_v$  in (5) replaced by  $h$  (Gorshkov et al., 2012, Eq. (15)):

$$f_e \equiv \frac{p_v}{h_c} - \frac{p_v}{h} = -p \frac{\partial \gamma}{\partial z}, \quad (7)$$

where  $\gamma \equiv p_v/p$ . This distinct formulation, noted by Jaramillo et al. (2018), presumes that the minimum of condensation-related potential energy characterizes air well mixed in the vertical. After condensation disturbs the pressure distribution of moist air by removing water vapor from the gas phase, the air as a whole tends to relax to hydrostatic distribution with the scale height  $h$ ,  $p/h = \rho g$ . By analogy, the state with minimum available potential energy as defined by Lorenz (1955) is not a static isothermal atmosphere, but an atmosphere with an adiabatic vertical lapse rate, which requires some motion to be maintained. Defining  $f_e$  as in (7) likewise presumes that some small motion (not generated by condensation) is required to counteract molecular diffusion and maintain a vertical air distribution with  $M = \text{const}$  and  $h_i = h$ .

The key statement of CIAD is that condensation provides power to atmospheric circulation: the rate at which the kinetic energy of wind is generated is equal to the rate at which the condensation-related potential energy is released. The latter rate is equal to the work per unit time  $\mathbf{v} \cdot \mathbf{f}_e = w f_e$  of the evaporative force, where  $\mathbf{v}$  and  $\mathbf{w}$  are the total and vertical air velocities. It is in this sense that the evaporative force drives winds. Accordingly, the key equation of CIAD is the equality between  $w f_e$  and the local rate of generation (and, in the steady state, dissipation) of kinetic energy. For a hydrostatic atmosphere this equation takes the form

$$w f_e = -\mathbf{u} \cdot \nabla p, \quad (8)$$

where  $\mathbf{u}$  is the horizontal velocity ( $\mathbf{v} = \mathbf{w} + \mathbf{u}$ ), see Eq. (8), Eq. (4), Eq. (17) and Eq. (5) of, respectively, Makarieva and Gorshkov (2009a, b, 2010, 2011), Eq. (16) of Gorshkov et al. (2012), Eq. (37) of Makarieva et al. (2013c). Repeatedly emphasized as the “*key relationship*”, “*main dynamic equation*” of CIAD etc. (see, e.g., Makarieva and Gorshkov, 2009b; Gorshkov et al., 2012), Eq. (8) has escaped notice of Jaramillo et al. (2018).

#### 4 CIAD and dry air

Jaramillo et al. (2018) expressed concerns about our treatment of dry air by noting that CIAD, “*in general, does not address the role of dry air correctly in the mixture, in particular during the process of condensation. In the real world, the vertical expansion of the water vapor column due to the difference between its actual and aerostatic scale heights is frustrated by the dry atmosphere.*”

While Jaramillo et al. (2018) do not provide a quantitative definition of the “frustration”, one can guess that they refer to the phenomenon considered in detail by Gorshkov et al. (2012) and Makarieva et al. (2013c). Makarieva et al. (2013c, p. 1047) wrote: “when water vapor condenses and its distribution is compressed several-fold compared to the hydrostatic distribution, the dry air must be “stretched” compared to its hydrostatic distribution. Only in this case, when the non-equilibrium deficit of vapor in the upper atmosphere is compensated by the non-equilibrium excess of dry air, the moist air as a whole will remain in equilibrium”. See also on p. 1053: “Condensation causes the distribution of vapor  $N_v$  to deviate from the equilibrium distribution. The condition that moist air as a whole nevertheless remains in equilibrium causes dry air  $N_d$  to also deviate from the equilibrium - but in the opposite direction to the vapor”. Gorshkov et al. (2012) emphasized that “condensation changes the vertical distribution of both water vapor and dry air components” and described how (see their Eq. 17).

Jaramillo et al. (2018) neglected these arguments<sup>2</sup> when they state that CIAD is incorrect “by including  $F_{vd}$  in the vertical motion equation while omitting  $F_{dv}$  and not addressing the role of dry air in the mixture. Since these two forces cancel, their net effect on the moist atmosphere as a whole is zero. Thus, they have no effect on geophysical fluid dynamics.” As we already discussed above, the argument about  $F_{vd}$  is invalid and thus irrelevant.

Regarding the role of dry air, using the spring analogy, as the spring decompresses upwards it lifts moist air (which is mostly dry air) to fill the void caused by condensation and to re-establish the hydrostatic equilibrium. Unlike for water vapor, which is replenished by evaporation, there is no source of dry air at the surface. Hence, as soon as the dry air has moved upward, the air pressure at the surface drops. This results in a horizontal pressure gradient to drive horizontal winds with a power  $-\mathbf{u} \cdot \nabla p$  equal to the power  $wf_e$  (8) of potential energy release during condensation – the process that has led to the formation of this pressure gradient. Makarieva et al. (2013c, p. 1047) noted that “the horizontal pressure gradient produced by condensation is a direct consequence of hydrostatic adjustment”.

The effects of these processes on atmospheric circulation were illustrated by showing that Eq. (8) provides a satisfactory quantitative explanation for the observed wind and pressure profiles in hurricanes and tornadoes (Makarieva and Gorshkov, 2009b, 2011; Makarieva et al., 2011). Furthermore, the global integral of Eq. (8) produces an estimate of condensation-driven global circulation that likewise is in a satisfactory agreement with observations (Makarieva et al., 2013c, b, 2017c). Since Eq. (8) presumes that condensation is associated with the vertical temperature drop and vertical air motion, a generalization to this equation was obtained accounting for horizontal temperature gradients (Makarieva and Gorshkov, 2010; Makarieva et al., 2014a).

## 5 CIAD, heat engines and buoyancy

Jaramillo et al. (2018) assert some general claims requiring rebuttal. For example, they state that “MGH [presumably intended as our team more generally?] never made any serious efforts in presenting the existing theory (or theories) on the maintenance of flows in a moist atmosphere. This is clear from MGH’s difficulty in understanding the production of work in thermally direct

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<sup>2</sup>A. M. Makarieva brought these arguments to the attention of Jaramillo et al. (2018) in a personal email communication to D. Raymond, O. Mesa and A. Jaramillo of 01 November 2017.

*circulation, by asserting that the ascending and descending components of work done by the buoyancy force cancel, resulting in a significant decrease in the energy released by atmospheric circulations. We showed using the Kelvin circulation theorem that the latent and sensible heating injected into the circulation at low levels by surface fluxes have a net positive effect on the circulation.*" This is incorrect and misleading.

First, we note that our group has been quite attentive to the *"existing theories on the maintenance of flows in a moist atmosphere"*. Among our recent works, Makarieva et al. (2015) discussed how the concept of surface pressure gradients driven by surface temperature gradients is considerably less robust than commonly thought. Makarieva et al. (2017d) presented *"an analytical approach that relates kinetic energy generation in circulation cells, viewed as heat engines or heat pumps, to surface pressure and temperature gradients"*. Makarieva et al. (2017a) discussed how condensation-induced hurricanes relate to the concept of maximum potential intensity of hurricanes of Emanuel (1986).

Second, Jaramillo et al. (2018) incorrectly attribute the statement that *"the ascending and descending components of work done by the buoyancy force cancel"* to "MGH". This pattern was explained by Goody (2003), who convincingly showed that the *"net positive effect on the circulation"* of surface fluxes can be arbitrarily small. Goody (2003, Fig. 2) demonstrated that when the radiative cooling of the descending air is small, the work done by moist convection can be negative. Jaramillo et al. (2018) ignored these arguments<sup>3</sup> and, based on the work of Pauluis (2011), considered a specific example where the radiative cooling in the upper atmosphere is such that the work of the cycle is positive. However, the lower limit for the efficiency of the buoyancy-driven circulation cannot be, and has not been, obtained from the heat engine considerations. Rather than *"readily explaining"* circulation over forests, Jaramillo et al. (2018) did not present any evidence that their examples are quantitatively relevant to the real atmosphere<sup>4</sup>. In contrast, applying CIAD specifically to the Amazon region, Makarieva et al. (2014b) demonstrated that the theoretical predictions of CIAD agree with observations of surface pressure gradients and velocities.

Finally, Jaramillo et al. (2018) juxtapose CIAD to the consideration of the atmosphere as *"a heat engine that produces mechanical work by transporting energy from warm to cold regions"*. It is a false opposition. Since atmospheric air circulates between the warm surface and the cold upper atmosphere, it is always possible to describe this motion as a thermodynamic cycle, irrespective of what drives it: condensation, buoyancy or something else.

The dynamic and thermodynamic approaches to atmospheric circulation combine in the relationship between pressure gradient and heat input. Indeed, in a steady-state, to receive heat from the ocean surface air must move (otherwise its temperature would rise) and this requires a pressure gradient. Without specifying this pressure gradient, it is not possible to determine the amount of heat received and, hence, the work performed within the thermodynamic cycle.

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<sup>3</sup>The paper of Goody (2003) was not quoted by Jaramillo et al. (2018) despite being brought to their attention by A. M. Makarieva in a personal email communication to A. Jaramillo and O. Mesa of 11 July 2017.

<sup>4</sup>Note also that Eq. (10) of Jaramillo et al. (2018) represents a formal replacement of variables: in  $F_z \equiv -\partial p/\partial z - \rho g$  pressure  $p$  is replaced by a combination of  $\rho_d$ , saturated mixing ratio  $r_v^*$  and temperature  $T$  using the ideal gas law and the equation of moist adiabat. The resulting expression for  $F_z$  does not contain any information regarding the role of condensation in atmospheric dynamics since all these new variables, as  $p$  in the original expression, remain unspecified. Likewise, the definitions of the evaporative force (5), (7) by themselves do not contain any information about condensation being a circulation driver. It is only Eq. (8) that does.

For example, in the thermodynamic cycles considered by Pauluis (2011) (whose ideas Jaramillo et al. (2018) reproduce in considerable detail) – the “steam cycle” (only latent heat consumed from the ocean) and the “mixed cycle” (both latent and sensible heat consumed) – the surface pressure difference is an *external parameter*. This surface pressure difference, which drives surface winds, cannot be retrieved from the heat engine approach. In consequence, Pauluis (2011) could not uniquely relate work output to the moisture supply (condensation rate) within the cycle: the relationship relied on the unknown or *a priori* postulated surface pressure difference. (Specifically, Pauluis (2011) set the surface pressure difference to zero for his “steam cycle” and left it unspecified (determining the sensible heat flux) for his “mixed cycle”). Relating work output to moisture input requires an extra equation – a constraint governing the dynamics of the boundary layer. Equation (8) of CIAD provides such a constraint.

The mere existence of a heat source and a heat sink does not guarantee that work will be performed. There must be a dynamic system which can convert heat into potential energy and work (see discussion by Makarieva et al., 2010). E.g. a spring attached to a piston in a cylinder accumulates potential energy while the gas within the cylinder expands and then pushes the piston back to compress the gas. In this context, the statements of Pauluis (2011, 2015) and colleagues that the water cycle *limits* [i.e. reduces] *the work output of the atmospheric heat engine* [because of irreversible processes associated with phase transitions] can be compared to the statement that since the spring is not ideal and has internal friction, the spring reduces the *maximum* possible output of useful work within the cycle. This is correct, but one should not read this statement as *without the spring (or the water cycle) the work would be larger than it is in its presence*. Rather, it is crucial to note that without a spring there would be no work at all. While the standard theory identifies the pressure gradients caused by differential heating as such a spring, we propose instead that the main dynamic mechanism providing power to the Earth’s circulation is the non-equilibrium vertical distribution of water vapor caused by condensation.

The equations of hydrodynamics allow for a solution where despite the same horizontal differential heating the power of atmospheric circulation on Earth is zero (the atmosphere is in geostrophic balance and no kinetic energy is generated). In current atmospheric models a non-zero power is achieved by fitting the parameters of turbulent friction. CIAD by constraining atmospheric power output, Eq. (8), actually guides the parameterization of turbulence (which in current models is not explicitly related to condensation).

It is from our observation that atmospheric power is well predicted by CIAD that we conclude that the net effect of temperature gradients (differential heating) is small. This implies that on a dry Earth the power of atmospheric circulation would be smaller than now. More importantly, removing major sources of water vapor, e.g. through large-scale deforestation, will influence atmospheric circulation, modify ocean-to-land moisture transport and impact the terrestrial water cycle (Makarieva and Gorshkov, 2007; Makarieva et al., 2013a; Nobre, 2014; Sheil, 2018). Independent observation-based studies testify in favor of a significant impact of vegetation cover on ocean-to-land circulation and moisture import (e.g., Levermann et al., 2009; Chikoore and Jury, 2010; Andrich and Imberger, 2013; Poveda et al., 2014; Herzsuh et al., 2014; Levermann et al., 2016; Boers et al., 2017). These scattered studies currently lack a unifying theoretical framework. One reason is that current circulation models do not appear to support abrupt changes in air circulation following changes in the functioning of vegetation cover (e.g., Boos and Storelvmo, 2016). However, if modeled turbulence could be re-parameterized so as to account for CIAD,

we expect the simulated atmospheric reactions to vegetation removal to be more realistic. We thus welcome discussion and strongly advocate the rigorous and focused (re)appraisal of the implications of forest and land cover change for atmospheric circulation and moisture transport.

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