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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Quantifying the global atmospheric power budget" *by* A. M. Makarieva et al.

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Here we reply to Comments 1 and 2 of Referee 1, Comments 3, 4 and 6 of Referee 2, Comment 3 of Referee 3 and Comments 4, 5 and 6 of Referee 4 addressing the challenge of obtaining reliable estimates of the distinct terms in the atmospheric power budget. Specifically, Referee 1 suggested that we should address the uncertainties surrounding the atmospheric power estimates, which we do in the revised text.

Following the recommendations of Referees 1, 2 and 3 we have extended our analysis in Section 5. We now analyze the 3-hourly MERRA dataset for the entire period 1979-2015. To illustrate the impact of temporal resolution on our results, we additionally analyze daily and monthly mean MERRA data for the same period. Furthermore, we assess NCAR/NCEP daily and monthly data for the last thirty five years.

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Discussion paper

Comment 1 of Referee 1 [doi:10.5194/acp-2016-203-RC1]:

" 1. The evaluation of the gravitational power of precipitation (GPP) as presented in Appendix A, which is used to verify the GPP estimated from the MERRA data, contains a significant source of uncertainties as it depends so much on different input parameters as listed in Appendix A. Likewise, the GPP estimated from MERRA also depends strongly on the data resolution, the number of vertical levels, or the numerical approximations. Before trying to explain the discrepancies between GPP obtained from GPCP data and the GPP obtained from the MERRA data, the authors should at least quantify the errors in all of your numbers. While the authors claim that the uncertainty of your estimated GPP from GPCP is 30%, there is no guarantee that the difference between the two GPP estimations will be statistical significance. Afterall, 30% of 1 W m^{-2} is 0.3, and so it could be anything from 0.7-1.3 W m^{-2}, which may be comparable to the GPP computed from the MERRA data;"

We agree with the above points and have included a discussion of uncertainties in a separate subsection in the revised Section 5. Specifically, we make two notes regarding our conclusion that W_P in MERRA is underestimated. First, as illustrated by the derivation of Eqs. (20)-(22) (see the footnote[1]), W_P *must* depend on data resolution. Indeed, W_P derives from the vertical air velocity and thus describes rainfall associated with air motions at the considered scale.

Meanwhile the theoretical estimate of W_P is based on the total observed rainfall and

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[1]

$$W = -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \mathbf{v}\cdot\nabla p d\mathcal{V} \equiv W_K + W_P, \quad (20)$$

$$W_K \equiv -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} (\mathbf{u}\cdot\nabla p) d\mathcal{V} + W_c \approx -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \mathbf{u}\cdot\nabla p d\mathcal{V}, \;\; W_c \equiv -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \rho_c(\mathbf{w}\cdot\mathbf{g}) d\mathcal{V}, \quad (21)$$

$$W_P \equiv -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} \rho\mathbf{w}\cdot\mathbf{g} d\mathcal{V} = -\frac{1}{\mathcal{S}}\int_{\mathcal{V}} gz\dot{\rho} d\mathcal{V} = Pg\mathcal{H}_P, \;\; P \equiv -\frac{1}{\mathcal{S}}\int_{z>0} \dot{\rho} d\mathcal{V}. \quad (22)$$

thus assesses cumulative gravitational power of precipitation at all scales. If W_P derived from MERRA coincided with theoretical W_P, that would mean that no rainfall is associated with the air motions at a scale finer than 100 km and six hours. Since the scale of convection is of the order of a few kilometers or less, apparently some rain must remain unresolved by the larger-scale motions. Therefore, the fact that W_P in MERRA is lower than its independent theoretical estimate does not indicate inconsistencies in the database.

Second, the theoretical estimate in Appendix A (now B) illustrates how the various parameters entering the value of W_P impact its magnitude. The bottomline however is provided by the TRMM-derived estimate of Pauluis and Dias (2012), which is 1.5 W m^{-2} for the area between 30° N and 30° S. So, global W_P cannot be lower than 0.75 W m^{-2}. If it is 0.75 W m^{-2}, this means that there is no precipitation at all in the extratropics. However, since extratropical precipitation is significant (2.2 mm day^{-1} versus 3.1 mm day^{-1} in the tropics, see Fig. 5 in our manuscript), it will contribute to the global value of W_P. Even we assume that all extratropical rainfall precipitates from $\mathcal{H}_P = 1$ km (which is clearly an underestimate), global W_P will constitute 0.87 W m^{-2}. Therefore, the uncertainty of the lower limit of our estimate $W_P = 1$ W m^{-2} is about 10%.

We note that while formally the analyzed MERRA data have a 3-hourly resolution, they represent an analysis of 6-hourly data with the intermediate values provided for assessing partial derivatives over time of the corresponding variables. To illustrate the impact of temporal resolution on the atmospheric power budget we compared W, W_P and W_K calculated from 6-hourly, daily and monthly mean MERRA data. These results are shown in Fig. 1 attached to this response and present in the revised Section 5.

With temporal resolution changing from 1 month to 6 hours W, W_K and W_P rise, respectively, from 1.02, 0.33 and 0.69 W m^{-2} to 3.27, 2.46 and 0.81 W m^{-2}. This supports our conclusion that with growing resolution of the available observations the kinetic power W_K will increase (presumably until the resolution of the smaller-scale convective motions is reached). Assuming a power law for the scaling of W_K with temporal resolution r

$$\frac{W_K(r_1)}{W_K(r_2)} = \left(\frac{r_1}{r_2}\right)^k, \quad k = \frac{\log[(W_K(r_1)/W_K(r_2)]}{\log[r_1/r_2]}, \tag{c1}$$

where r is temporal resolution in hours, from $W_K(24) = 1.78$ W m^{-2} (daily) and $W_K(6)$=2.46 W m^{-2} (six hours) from Eq. (c1) we find $k = -0.23$. Using this value and $W_K(6)$=2.46 W m^{-2} we find $W_K(1)$=3.7 W m^{-2}, i.e. kinetic power of convective air motions having temporal scale of 1 hour should be about 4 W m^{-2}. This is consistent with the theoretical estimate for condensation-induced air circulation.

Comment 2 of Referee 1 [doi:10.5194/acp-2016-203-RC1]: *"2. Estimations of the total atmospheric power W and W_K are subject to similar uncertainties as mentioned in my comment # 1 above. At resolution of 1.25 degree and 42 vertical levels, any global estimation of the total integrated energy and kinetic energy contains large variation, let alone the difference between two. Have the authors tried the NCEP reanalysis or ECMWF dataset at different resolutions to see how sensitive your estimations are? As long as we don't have reliable estimation of W, W_K, and GPP, explanation for the difference would provide little scientific value."*

We agree with the above comments and extended our analysis to include the NCAR/NCEP daily data for the same period 1979-2015. This yielded instructive results.

As we note in our manuscript (p. 8) and emphasize in the revision, kinetic power W_K is derived from observations of wind velocities and should be associated with much less uncertainty than the vertical velocity. This is confirmed by comparison of W_K across the MERRA and NCAR/NCEP databases, Fig. 2. The profiles of W_K are close at most latitudes and the global mean values are also similar: 1.75 W m^{-2} for NCAR/NCEP and 1.79 W m^{-2} for MERRA[2].

[2] We note that the spatial resolution of a particular re-analysis is not necessarily the same as the spatial resolution

The situation is different for total power W, which depends on the vertical velocity. The global value of W appears as a near-zero sum of large terms of different signs that describe the ascending and descending air motions. This is the reason for its high uncertainty: in order to yield a global W of the same accuracy as W_K, these vertical air flows must be deduced from the continuity equation with an accuracy exceeding that of the horizontal air flows (that define W_K) by two orders of magnitude. However, this cannot be readily achieved, since the only source of information about the vertical air flow is the continuity equation and the observations of the horizontal air flow. As a result of this high uncertainty, W appears inconsistent across the databases.

In Fig. 2b we show the dependence of the columnar mean Ω (Eq. 23 in our manuscript)

$$W = \langle \Omega \rangle, \quad \Omega \equiv -\frac{1}{\mathcal{S}} \int_{\mathcal{V}} \omega d\mathcal{V}, \quad \omega \equiv \frac{dp}{dt} \equiv \frac{\partial p}{\partial t} + \mathbf{v} \cdot \nabla p, \tag{c2}$$

on latitude in NCAR/NCEP versus MERRA database. One can see that, similar to W_K in Fig. 2a, the differences between the derived zonal distributions are relatively small. However, as far as the local magnitudes of Ω exceed its global mean value by about two orders of magnitude, it turns out that these small local differences translate into profound differences in the global atmospheric power W. Our analysis suggests that global W estimated from Eq. (20) in the NCAR/NCEP daily data is *negative* and constitutes -6.06 W m^{-2} versus 2.45 W m^{-2} in MERRA. Unless there is some technical error involved (which is always possible but appears unlikely since our estimates of W_K are consistent across the databases and since taking the integral of pressure velocity over volume is straightforward), the obtained results suggest that the global estimate of W and, hence, W_P in a given dataset is significantly impacted by the particular procedures involved to calculate pressure velocity ω from the continuity equation.

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of the experimental data the re-analysis presents. While using numerical modelling it is possible to rescale the observed data to a finer resolution, the results will not necessarily reflect the processes in the real atmosphere. The similarity between daily data in MERRA (spatial resolution 1.25x1.25 degrees) and NCAR/NCEP (2.5x2.5 degrees) may thus reflect the fact that the raw experimental data can have an average resolution coarser than in either dataset.

Specifically, as also pointed out by Referee 2 [doi:10.5194/acp-2016-203-RC2, Comment 2], in the MERRA database pressure velocity is calculated involving information on the local water cycle in such a manner that the vertically integrated continuity equation has a zero source/sink. This procedure takes some information about local precipitation into account (see Eq. 15 in our manuscript) and, as a result, can yield a reasonable value for total atmosperic power, for W_P and for other terms depending on $\dot{\rho}$. This procedure should also be responsible for the fact that the MERRA-derived W_P has a relatively minor dependence on temporal resolution compared to W_K. Indeed, with transition from monthly to 6-hourly resolution W_P barely increases by 30%, Fig. 1c, while W_K rises almost eight-fold. This is because the long-term mean local rainfall rate does not depend on temporal resolution being a cumulative representation of precipitation events at all scales.

To our knowledge, atmospheric power has not been systematically assessed in re-analyses in the straightforward way outlined by Eq. (c2) – i.e. as the integral of pressure velocity over atmospheric volume. Thus we cannot compare our NCAR/NCEP results with any published estimate. Rather, atmospheric power was commonly assessed as the total dissipation rate in the atmospheric energy cycle, i.e. as work per unit time of the turbulent friction force (see, e.g., Eq. (A3) of Boer and Lambert (2008)). In particular, Boer and Lambert (2008), when comparing atmospheric power across the re-analyses and global circulation models, quoted a figure of 2 W m^{-2} for the 6-hourly NCAR/NCEP data (see Table 3 of Boer and Lambert (2008)). Our results for the daily NCAR/NCEP data for W_K is 1.75 W m^{-2}, which is consistent with the above estimate taking into account the dependence of W_K on temporal resolution as shown in Fig. 1b. Therefore, the estimates reported by Boer and Lambert (2008) do not represent total atmospheric power, which thus remains unstudied across the models and re-analyses datasets.

Our comparison of W between NCAR/NCEP and MERRA highlights the high uncertainty in the calculation of vertical velocities. The estimates of total atmospheric power

W and the gravitational power of precipitation W_P made from re-analyses according to our Eqs. (20)-(22) should be used to constrain the calculation of vertical velocities in re-analyses thus improving their consistency in representing the atmospheric energetics.

In particular, while the MERRA database, which does account for precipitation when calculating ω, produces a reasonable estimate of total atmospheric power W and gravitational power W_P, Fig. 1c shows that this W_P has a pronounced seasonal cycle that appears unreasonable. In July (when the global temperature is at its maximum, see Fig. 6b in our manuscript), global W_P is nearly twice lower than it is in January. This seasonal variation does not correlate with the seasonal global rainfall (see Fig. 1b in our manuscript) and may be an artefact of the procedures involved to calculate pressure velocity in the MERRA database. Our results call for a systematic study of the atmospheric power budget across the re-analyses and also across global circulation models on the basis of Eqs. (20)-(22).

Comment 3 of Referee 3 [doi:10.5194/acp-2016-203-RC3]:

"3. The paper is poorly constructed. It is mainly three separate studies. Sections 2-4 attempt a theoretical discussion of the issues that mostly reprise previous work. It is unnecessarily confusing. Section 5 is the main 'new' result. The computation done are fairly routine, and the result in line with what we know. The inability of the authors to produce a consistent figure for Wp is distressing and should be better addressed in the revision. Section 6 is a lengthy disgression which is mostly a repeat of the authors previous work."

We believe that the revised paper is now much clearer and presents a coherent theme. We underline that the entire literature on this subject is somewhat confusing and it is the need to identify and examine the inconsistencies in other studies that leads to difficulties. Our revision is attentive to these difficulties (e.g. the different formulations of W).

We note that published approaches to W_P suffer important inconsistencies. Pauluis et al. (2000) estimated, on theoretical grounds, that tropical W_P (between 30N and 30S) should be between 2 and 4 W m^{-2}. Pauluis and Dias (2012) analyzed TRMM data to conclude that tropical W_P is, rather, 1.8 W m^{-2}. Makarieva et al. (2013) likewise on theoretical grounds, suggested that Pauluis et al. (2000) overestimated tropical W_P by around one-hundred percent. Their results led Pauluis and Dias to revise their calculations and publish a revised TRMM-based estimate of 1.5 W m^{-2} for the tropics as a corrigendum to their 2012 work. On the other hand, Makarieva et al. (2013) suggested that global W_P should be around 0.8 W m^{-2}; in our present work we show that the true value is around 1 W m^{-2} and we address the associated uncertainties.

As we discussed in our reply to Comment 2 of Referee 1, we show in the revision that the inconsistency in the estimates of W_P as well as of total power W is an inherent property of the re-analyses. We disagree that this is already known, since we find no estimates of global W_P from re-analyses or otherwise. This is indeed surprising given recent emphasis on the thermodynamic aspects of the water cycle (see, e.g., Pauluis, 2015). We hope that our revised work brings greater clarity to this matter.

In particular, our results suggest that if Laliberté et al. (2015) used NCAR/NCEP rather than MERRA data for their analysis, they would have obtained a negative value for total atmospheric power (and, hence, W_P). Key to their result is the procedure of zeroing pressure velocity at the surface between the modelling steps; unfortunately, its details were not reported. If a different procedure were used, the results would be different as well.

Comment 3 of Referee 2 [doi:10.5194/acp-2016-203-RC2]: *3. Computing the work from MERRA data. As mentioned before, the MERRA product has many vertically integrated budget variables that allow one to quantify each one of the term in the energy equation. For this review, I've looked at the kinetic energy generation 1980-1985 and the yearly average gives 3.40-3.48 W/m^2 for the integral of $\omega\alpha$ and 3.6-3.8 W/m^2 when including the kinetic energy generation from the analysis step. The kinetic en-*

ergy generation is balanced by damping from the numerical dissipation, the dynamical remapping and the physically parametrized frictional dissipation. This means that the estimates provided in section 5.1 are substantial underestimates.

We agree with the referee that our estimates of W are substantial underestimates of the real atmospheric power; indeed, it is our major point. We do not consider the kinetic energy generation from the analysis step. (Neither did Laliberté et al. (2015)). We explicitly address how the global atmospheric power can be estimated using the reanalysis pressure and velocity at their face value. In our revised manuscript we present an analysis of the entire period 1979-2015. We note that for W_K our results practically coincide with those of Huang and McElroy (2015), who reported $W_K = 2.46$ W m^{-2} for 1979-2010. Our calculations for the same period give $W_K = 2.45$ W m^{-2}.

Our annual estimates of W for 1980-1985 range from 3.20 to 3.28 W m^{-2}. This is 6% smaller than the referee's. The discrepancy may stem from two sources. First, the dataset with the vertically integrated $\omega\alpha$ derives from the 1-hourly surface dataset (presumably MAT1NXINT [tavg1_2d_int_Nx]), while our estimate derives from the 3-hourly dataset (MAI3CPASM [inst3_3d_asm_Cp]). As we have shown that W increases with finer temporal resolution, see Fig. 1a, this may explain the 6% discrepancy. Second, the discrepancy may stem from a difference in the boundary condition for ω at the surface.

It is not explicitly indicated in the dataset how the integration was performed. We have explicitly stated what boundary condition we are using to make our analysis tractable and comparable to other studies. Furthermore, we investigated the impact of the surface boundary condition on our analysis for each variable and showed that the associated uncertainty is about 6%.

Specifically, for W and W_K we estimated the value of ω and $\mathbf{u} \cdot \nabla p$ at the surface in two ways (see Appendix B in our manuscript). One is to assume that air velocity at the surface is zero, $\mathbf{v} = 0$, another is to linearly extrapolate ω and $\mathbf{u} \cdot \nabla p$ from the nearest pressure level to the surface. Our increased attention to the boundary layer is justified by the fact that horizontal velocity experiences significant non-uniform changes along the vertical. In the limit of an infinitely precise vertical resolution the two approaches should give the same value. In the real atmosphere they produce different results.

Specifically, the extrapolated W_K (W_{K1} in our manuscript) turns out to be higher than W_K calculated assuming $\mathbf{v} = 0$. This has to do with the vertical profile of W_K shown in Fig. 3. Kinetic energy generation grows with increasing pressure in the lower atmosphere. Extrapolation of this dependence to the surface yields a positive surface value for kinetic energy generation. Thus, W_K obtained from this extrapolation is higher than when we assume that $\mathbf{v} = 0$, such that no kinetic energy is generated at $z = 0$

In contrast, the estimate of total power W is smaller when extrapolated than when assuming zero velocity at the surface. This has to do with a different distribution of pressure velocity over pressure levels, Fig. 3. Here the lowest layer between 975 hPa and the surface makes a large negative contribution to the total W. This is because the air predominantly descends in the regions of higher surface pressure. Therefore with one and the same ω at 975 hPa, the layer where the air descends and surface pressure is about, say, 1020 hPa is thicker than where the air ascends and surface pressure is about 1000 hPa. Since W is proportional to $-\omega$, in the result the net contribution of the lower layer to global W is negative.

The difference between the two estimates for W and for W_K is about 10%. The difference between W_P values obtained by the two means is greater. W_P obtained by interpolation is considerably smaller than W_P obtained assuming that zero velocity at the surface. This suggests that our conclusion about W_P being underestimated in MERRA is robust.

Comment 4 of Referee 2 [doi:10.5194/acp-2016-203-RC2]: *4. In section 5.1, I do not see the use for W_1. And why not use $\omega_s = \partial_t p_s + \mathbf{v}_s \cdot \nabla_H p_s$, with ∇_H being the horizontal gradient? The p_s and $\mathbf{v}_s$ are both available and this is the right expression. Maybe that* *could fix their underestimate of W.*

The use of W_1 is discussed in our reply to Comment 3 of Referee 2. W_1 and W_{K1} were used to investigate the uncertainty associated with insufficient data resolution in the boundary layer.

One cannot use $\omega_s = \partial_t p_s + \mathbf{v}_s \cdot \nabla_H p_s$, because the horizontal gradient of surface pressure $\nabla_H p_s$ only exists if the surface is horizontal (i.e. has invariant geopotential height). Since the geopotential height of the real surface varies, surface pressure is much more affected by this variability in the vertical plane than by any effects in the horizontal plane, which prohibits the use of p_s for a reliable determination of ω_s.

Moreover, since the term $\mathbf{v}_s \cdot \nabla_H p_s$ is present in the surface values of both ω and $\mathbf{u} \cdot \nabla p$, even if this term were added, this would not change the difference between the global W and W_K.

To simplify the presentation, in the revised text we everywhere use W_2 and W_{K2} and discuss W_1 and W_{K1} only in the section devoted to the uncertainties.

Comment 6 of Referee 2 [doi:10.5194/acp-2016-203-RC2]: *6. Finally, I'm not sure the following sentence is logically true: "The fact that W_{Kc} is likewise higher than our MERRA-derived kinetic power, testifies in favor of the theoretical estimate". All it means is that W_{Kc} is potentially a right upper bound. The only way to check whether it is the right upper bound would be to either verify if it holds on other Earth-like planets or using simulations with increasing resolution and seeing that it describes the scaling. As I said before, the last two sections of this manuscript are really too speculative in their current form and they are dragging down the original results described in sections 5.*

In our manuscript we explained what we mean by *testifying in favor of the theoretical estimate*. The phrase quoted by the referee is immediately followed by an explicit clarification (p. 16): "To explain this point in greater detail: Eq. (15) and Eq. (22), which estimate, respectively, the mass integral of dh/dt and the gravitational power of precipitation W_P, are not dependent on the assumption that air circulation on Earth is condensation-driven. These equations describe how the corresponding variables can be estimated from observations. Both variables are approximately proportional to the volume integral of net condensation rate in the atmospheric interior $-\int_{z>0} \dot{\rho} d\mathcal{V}$. We notice that both variables estimated from the MERRA database are by 50-70% smaller than when estimated independently from the observed global precipitation P. We attribute this to the insufficient spatial resolution of the air motions associated with condensation. Now, we predict that if atmospheric circulation is condensation-driven, kinetic power generation is also proportional to P, as described by Eq. (25). Since we already know that not all condensation is resolved in the MERRA dataset, we can expect that kinetic energy generation estimated from MERRA using Eq. (21) will be smaller by a comparable magnitude than its theoretical estimate (25). ***This is what we find. If kinetic power was unrelated to precipitation, we could not expect that its value would be smaller than the precipitation-based theoretical estimate (25). If, on the other hand, our theoretical estimate turned out to be smaller than the MERRA-derived estimate,*** $W_{Kc} < W_K$***, this would testify against condensation-induced dynamics.***"

In the revised manuscript we show that kinetic power W_K does grow with finer resolution, Fig. 1, and that the theoretically predicted 4 W m^{-2} is the plausible limit observed at convective scale.

Comment 2 of Referee 4 [doi:10.5194/acp-2016-203-RC4]:

2. The water cycle is generally regarded as making the atmospheric heat engine less efficient as the result of part of the solar forcing being expanded in lifting water vapour against the gravity field, part of which is then removed through precipitation, leaving only the residual to power the atmospheric circulation, an idea proposed by Pauluis and reprised in Laliberte et al. (2015). It seems that this should be discussed.

The referee's account of the work of Laliberté et al. (2015) appears to be a misunderstanding. There are three relevant quantities: the power of a Carnot cycle W_C, the kinetic atmospheric power W_K and the total atmospheric power W. The focus of Pauluis et al. (2000) was indeed to show that W_K is lower than W because, using the referee's words, solar power is "lifting water vapour against the gravity field, part of which is then removed through precipitation, leaving only the residual to power the atmospheric circulation". However, Pauluis (2011) advanced a different statement: that total power W is lower than Carnot power W_C because of the irreversible processes like water vapor diffusion. Laliberté et al. (2015) were likewise concerned about why W is smaller than W_C and did not assess the gravitational power of precipitation.

This misunderstanding might have stemmed from the comment of Pauluis (2015) on the work of Laliberté et al. (2015), where the two statements, $W_K < W$ and $W < W_C$, became mixed. To provide some context, an ideal atmospheric Carnot cycle consuming heat flux $F = 100$ W m^{-2} at surface temperature $T_{in} = 300$ K and releasing heat at $T_{out} = T_{in} - \Delta T$ with $\Delta T_C = 30$ K, would generate kinetic energy at a rate of $W_C = F(\Delta T_C / T_{in}) = 10$ W m^{-2}. Laliberté *et al.* (2015) estimated total atmospheric power W at around 4 W m^{-2}. Comparing their result with W_C, Pauluis (2015) noted that "estimates for the rate of kinetic energy production by atmospheric motions are about half this figure". Here confusion has apparently arisen between total atmospheric power W and kinetic power W_K (because Laliberté et al. (2015) assessed only W but not W_K, the latter being about 2.5 W m^{-2}, i.e. a quarter rather than half of W_C). Indeed, Pauluis (2015) continued that "the difference is very likely due to Earth's hydrological cycle, which reduces the production of kinetic energy in two ways", one of which is the gravitational power of precipitation W_P and the other is the irreversible diffusion processes. However, from our Eqs. (20)-(22), W_P reduces W_K compared to W but it does not reduce W compared to W_C, since $W_K + W_P = W < W_C$.

Comment 6 of Referee 4 [doi:10.5194/acp-2016-203-RC4]: *6. On a last note, I have a hard time accepting that the term $u \cdot \nabla p$ is something observable, given that the only* *way to estimate this term can only be done by means of a numerical model; likewise for the internal condensation/precipitation terms.*

In the meteorological literature it is common to refer to the re-analyses data as to *observations* using which models outputs could be verified – see, for example, the study of Boer and Lambert (2008) devoted to the atmospheric energy cycle. This is because the re-analyses aim to systematize available observations of air pressure, velocity, temperature, humidity etc. in a coherent form. Air pressure and velocity are the basic observational parameters recorded. Likewise, precipitation is directly measured at the surface as well as assessed in the tropical atmosphere with use of satellites (the TRMM mission).

Since vertical velocities are small compared to horizontal velocities, they cannot be derived directly from observations. It is in this sense that the term $\mathbf{u} \cdot \nabla p$ is observable with a good accuracy, while the term $\rho w g$ responsible for the gravitational power of precipitation is not. This latter term can only be derived from observations using additional assumptions. Because of this difference, we estimate W_K with less uncertainty than W_P.

Comment 5 of Referee 4 [doi:10.5194/acp-2016-203-RC4]: *5. Section 4. I don't really understand why this decomposition is useful. Indeed, a well known consequence of making the hydrostatic approximation is to filter out the contribution of the vertical velocity to the kinetic energy. As a result, the evolution equation for the kinetic energy becomes*

$$\rho \frac{D}{Dt}\frac{u^2}{2} + u \cdot \nabla p = \rho F \cdot u \tag{c3}$$

so that in equilibrium

$$\int_V u \cdot \nabla p dV = Friction, \tag{c4}$$

which shows that only what the authors call the kinetic energy power (the conversion between kinetic energy and available potential energy) becomes relevant to understand *how the atmospheric circulation is powered. As is also well known, even without the hydrostatic approximation, the budget of gravitational potential energy is zero*

$$\int_V \rho g w dV = 0 \tag{c5}$$

where ρw is the total mass flux, and hence decoupled from the kinetic energy budget. One may if one so desires to separate the total mass flux into gaseous and liquid components, and restrict attention to the former, for which the GPE budget becomes

$$\left.\frac{d(GPE)}{dt}\right|_{gas} = \underbrace{\int_V \rho g w dV}_{-SW_P} + GAS\ DESTRUCTION = 0, \tag{c6}$$

where ρw is now the gaseous mass flux only, GAS DESTRUCTION means GPE sink due to destruction of water vapour mass by condensation, but that does not make it less decoupled from the horizontal kinetic energy budget, where the underlined term is what the authors call the power of precipitation*, whatever that means. Physically, this term represents primarily a conversion with internal energy, and is not directly related to the kinetic energy of the system, making its usefulness for clarifying the atmospheric power budget dubious. Moreover, it is also well known that for a hydrostatic fluid, it is the total potential energy of the system (i.e., the enthalpy) that matters, given that large variations in gravitational potential energy are compensated by large variations in internal energy, with no impact on kinetic energy. The focus on gravitational potential energy, therefore, is at odds with the common wisdom that GPE is not useful to consider on its own. The claim that GPE variations are somehow connected with kinetic energy production is odd, given that the hydrostatic approximation is unconnected to the vertical velocity field.*

The decomposition of total atmospheric power W into the kinetic power of winds W_K and the gravitational power of precipitation W_P is useful in several ways. First, as we

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discussed above in response to Comment 6 of Referee 4, W_P and W_K in re-analyses are characterized by substantially different uncertainties, so it is useful to keep a separate record for them. Second, W_P can be estimated independently from wind velocities using observed precipitation; this information can be used to constrain vertical velocities. Third, since thermodynamics constrains total power W and not kinetic power W_K or W_P separately, it is necessary to clearly differentiate between W, W_K and W_P from a theoretical viewpoint. Distinguishing these components can help avoid confusions when comparing results from different studies (see also above our reply to Comment 2 of Referee 4). For example, given the modern concern about renewable energy resources it is necessary to understand that the so-called "wind power" (Marvel et al., 2013) as well as the river hydropower (which is part of W_P) are not the total power of the atmosphere.

We also note that in the presence of condensate the vertical distribution of gaseous air is not hydrostatic; the condensate loading term describes the generation of kinetic energy of the vertical air motions and is not zero. Furthermore, the integral of the right-hand part of the referee's equation (c3) is not zero in the presence of phase transitions, so Eq. (c4) does not hold. This is discussed in detail in the revised section 3 (see doi:10.5194/acp-2016-203-AC4, Eq. 29 on p. C10).

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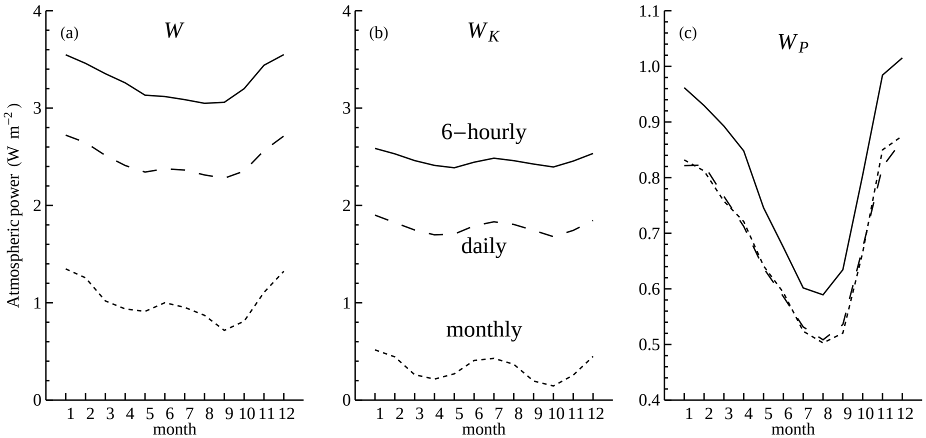

Figure 1: Long-term mean atmospheric power in MERRA as dependent on temporal resolution: 6-hourly (solid curves), daily (dashed curves) and monthly (dotted curves). (a) total power W (20), (b) kinetic power W_K (21), (c) gravitational power of precipitation $W_P = W - W_K$.

Fig. 1.

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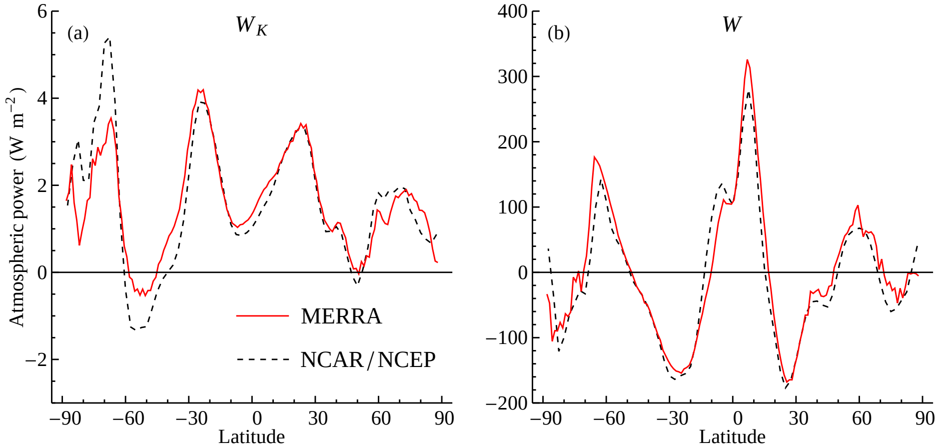

Figure 2: Long-term mean zonally averaged atmospheric power (daily data for 1981-2015) in the MERRA versus NCAR/NCEP re-analysis as dependent on latitude (red solid curve: MERRA, black dashed curve: NCAR/NCEP). (a) kinetic power W_K (21), (b) total power W (20).

Fig. 2.

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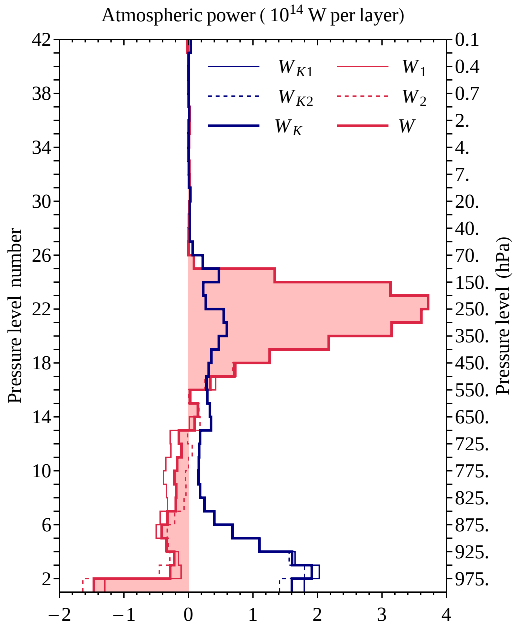

Figure 3: Atmospheric power within the 41 pressure layers enclosed by the 42 pressure levels in the MERRA dataset. See Appendix B for the full list of pressure levels. The lowest bar of the histograms corresponds to the layer with pressure less than 975 hPa. Sum of the histogram values over all layers gives the global values of the atmospheric power.

Fig. 3.

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