Review Response

I. REVIEWER 1

This manuscript presents an idealized solution for the midlatitude zonal mean heat and momentum equations by assuming the eddy momentum and heat fluxes can be represented by a simple diffusive parameterization. The first-order solution reproduces the basic qualitative structure of the midlatitude tropospheric circulation, with a Ferrel cell located between two oppositely-oriented cells, a westerly jet at the center of the Ferrel cell, poleward eddy heat flux in the lower troposphere and up-gradient eddy momentum flux. The solution also allows for an examination of the effect of the model parameters on the mean flow properties and structure.

We thank the reviewer for the careful reading of the manuscript and the useful comments.

While the solution presented in this manuscript could potentially be of scientific interest to the WCD community, I find the presentation of the manuscript to be overly-complicated, which makes it difficult to evaluate the scientific contribution. The major issues are listed below. I would recommend that the authors resubmit the manuscript, with a different focus and structure. Currently, I am not able to assess whether this manuscript contributes to new insights not addressed by previous studies.

Response:
This research may appear complex due to its mathematical formulations. However, to highlight the core ideas and demonstrate the robustness and generality of the overall approach, we believe it is essential to present the detailed derivation of the main model. The key equation is a simple, linear, fourth-order partial differential equation with boundary conditions. Our goal is to show that this equation originates from the full quasi-geostrophic (QG) dynamics and retains the fundamental characteristics of mid-latitude atmospheric behavior, despite the use of simplified parameterizations and the exclusion of external diabatic forcing.

Changes in the text:
We will substantially rewrite the manuscript such that the paper is more accessible and that the major novel insights are clear.

The abstract and introduction give a very vague impression of what the manuscript is about, while the actual subject is quite simple. The inclusion of topics such as extreme events, eddy life cycle and the response to climate change in the abstract give the wrong impression that the model relates to these topics, while actually the model solves the equations only for the climatological zonal mean flow. Also, it would be preferred to mention that the parameterization for the eddy fluxes in this model is a simple diffusive parameterization with a constant diffusion coefficient and that the model is a beta-plane model. This give the reader the right context for the presented research. The introduction gives a lengthy overview of the general atmospheric circulation (lines 20-85), while not covering the more specific topics of this manuscript: How are the midlatitude circulation properties affected by general parameters such as Earth's rotation rate, static stability, the meridional width of the domain, etc. or by the non-dimensional parameters derived from these parameters?; How can the eddy fluxes be represented using simple diffusive parameterizations while capturing their qualitative properties? While previous studies have dealt with these questions, the introduction does not discuss these papers and does not clarify what are the remaining open questions which this manuscript addresses.

We will update the introduction following your suggestions.

Overly-complicated mathematical derivation. The manuscript contains 54 equations, in addition to a few inline equations and 5 additional equations in the appendix. Also, the number of variables, parameters and signs used is very large, which makes it very difficult to follow the derivation. I find this complication unnecessary, as the model itself is quite simple. The derivation could be shortened by beginning with a clear listing of the model assumption and presenting the beta-plane zonal-mean heat and momentum equations, which are quite standard in the literature. Subsequently, the diffusive approximation can be replaced into the equations and the four boundary conditions can be presented. There is no need to develop the Ekman layer equations, as there is no use of them in the model. This would reduce equations 1-26 to around 8 equations. The derivation of the analytical solution in section 3.1 (equations 35-50) is quite messy and could be organized in a more readable manner. Also, there is some redundancy, where some equations are repeated twice. There are too many signs and subscripts, which I don't think are necessary. The detailed comments below elaborate on this issue.

Response:
We understand that the paper may appear overly complex at first glance. However, the structure of the sections and the level of detail in the derivations have been carefully selected to effectively convey the main ideas.

The simplicity of the main equations is deliberate. Since the quasi-geostrophic (QG) equation is widely accepted as a standard framework for analyzing large-scale atmospheric dynamics in the mid-latitudes, it is essential to demonstrate that our main equation is derived from the QG system. This includes a parameterization of eddy fluxes that is supported by numerical simulations and appropriate boundary conditions (Equations 1–11). In particular, the simplified parameterizations of eddy heat and momentum fluxes must be validated through numerical experiments.

Boundary conditions play a critical role in shaping the overall solution structure. Specifically, the eigenvalue problem is highly sensitive to these conditions, making their appropriate selection essential for realism. For this reason, we provide a detailed discussion of their physical significance.

Changes in the text:
We will increase the accessibility of the paper by moving much of the mathematical material to appendices.

I think that a slightly different parameterization would be more physically consistent. Several previous studies have used diffusive parameterization for the potential vorticity (PV) flux, instead of the heat and momentum fluxes. Using this approximation and a non-dimensional parameter that describes the ratio between the vorticity and stretching terms in the PV equation (the Burger number) would give a parameterization for the heat and momentum fluxes. As PV is the conserved variable in the absence of friction and diabatic heating, it makes more sense to approximate its flux using a diffusive parameterization.

Response:
Fundamentally, a non-accelerated state in the absence of external or diabatic forcing corresponds to a vanishing Eliassen Palm (EP) flux divergence, which is mathematically equivalent to zero potential vorticity (PV) flux. Importantly, in this research, we are not concerned with parameterizing the PV flux—because it is identically zero or constant in the latitudinal domain under these assumptions. Instead, our focus is on addressing a fundamental question in atmospheric dynamics: What is the structure of the atmosphere when the PV flux is zero?

The answer, as shown in this study, is that the system admits a steady, eddy-induced mid-latitude circulation that qualitatively resembles the Ferrel cell. The key challenge, then, lies in the parameterization of the eddy heat and momentum fluxes in terms of the mean state variables. This parameterization is central to capturing the dynamics of the circulation in the absence of external forcing.

Changes in the text:
We will explain in the revised text, why a parameterization of the PV flux is not used.

It is not clear what is the new scientific contribution of this manuscript. Previous studies used diffusive approximations for eddy fluxes, and/or performed comprehensive parameter sweeps investigating the effect of non-dimensional parameters on the properties of the midlatitude circulation, including the Ferrel cell and the eddy-driven jet. Most of these studies are not mentioned in the manuscript. Some are mentioned, but without discussing their outcome. It is not clear what exactly this study adds to the existing knowledge.

The potential for a significant scientific contribution based on the suggested model is not fulfilled. I think that the potential for new insights from this study comes mostly from the relations obtained from the parameter sweep, such as displayed in figure 8b and figure 9. However, these sweeps and their discussion are quite limited in this manuscript, and they are not compared with the results of previous studies. The other results are not really new, and I don't think they add any new insights for the dynamics of the midlatitude circulation.

Response:
Our study provides several significant contributions, both in terms of theoretical outputs and conceptual perspectives. We derive analytic solutions by integrating the momentum, mass, and energy balance equations. Rather than focusing on the relative magnitude of individual terms, these solutions offer a holistic view of how each term contributes to the overall circulation structure. This integration highlights the physical mechanisms that shape the steady-state mid-latitude circulation.

The analytic framework reveals that the dynamic balance between eddy momentum flux and eddy heat flux is central to the non-acceleration state. The eddy momentum flux acts to strengthen the zonal-mean zonal wind, whereas the eddy heat flux works in the opposite direction, weakening it. Crucially, the eddy momentum flux is concentrated in the upper troposphere, while the eddy heat flux is prominent in the lower troposphere. This vertical separation necessitates a coupling mechanism between upper and lower levels—resulting in an indirect circulation that resembles the Ferrel cell. The analytic solutions reveal this explicitly.

Understanding what determines the size of the Ferrel cell is essential for characterizing mid-latitude climate. For instance, the equatorward edge of the Ferrel cell aligns with the poleward boundary of the Hadley cell - a region often associated with subtropical deserts. Equations (52) and (54) provide a theoretical length scale for the Ferrel cell, derived from our analytic solutions. This scale is governed by a structural parameter, γ_1, which incorporates eddy momentum and heat fluxes (M and D), dry static stability (S), and vertical structure (q) - all key components of the baroclinic wave life cycle. Notably, this predicted scale closely matches the Ferrel cell size observed in numerical simulations (Fig. 8b).

The structural parameter γ_1 includes the vertical eigenvalue q, indicating that the vertical structure is intimately linked to the meridional extent of mid-latitude circulations. This has implications beyond Earth—for example, when observing planetary atmospheres via telescopes or satellites, we often only have access to horizontal structures. Our analytic solutions suggest that it may be possible to infer aspects of the vertical structure from these horizontal observations.

While our results align with earlier findings in a qualitative sense, the true contribution is the synthesis: bringing together known components into a coherent, systematic understanding of mid-latitude quasi-geostrophic (QG) dynamics. We believe this represents a significant step forward in explaining the emergence of organized meridional circulations such as the Ferrel cell.

Changes in the text:
In the revised discussion, we will stress even more the value of analytic research, in particular in its ability to integrate differential equations into closed-form solutions, revealing the structural relationships among physical processes in a unified and interpretable framework.

II. SPECIFIC COMMENTS

Lines 10-12: It is not clear from reading the abstract what these sentences are describing. Only after reading the whole manuscript I understood that this is a description of the solution for the zonal-mean climatological flow in the idealized analytical model, and that the solution is a sum of the two listed features.

This is the description of the solutions from the main equation as will be clarified in the revised paper.

Lines 42,45: Instead of "the original theory" I would suggest referring to the Held-Hou theory as the axisymmetric theory.

We will update it in the next version of manuscript.

Lines 51-52: It is not clear in what sense the Ferrel cell is "originating" from the downward motion near the edge of the Hadley cell. Is this sentence suggesting causal relations? The same comment applies to line 288.

Response:
The indirect Ferrel cell emerges as a dynamical adjustment to restore the zonal-mean zonal wind, which is weakened by baroclinic instability and the associated lower-level poleward eddy heat flux. In this sense, the Ferrel cell can be interpreted as a large-scale atmospheric response in the mid-latitudes aimed at maintaining the zonal wind structure.

Within the life cycle of unstable baroclinic waves, the heat flux generated by growing waves in the lower troposphere acts to reduce the zonal-mean zonal wind, thereby weakening the thermal wind balance. As the waves evolve, they propagate toward the flanks of the jet stream and eventually break. This wave breaking leads to the convergence of eddy momentum fluxes, which in turn re-accelerates the zonal-mean zonal wind.

The poleward eddy momentum flux in the upper troposphere is associated with ageostrophic downward motion, which effectively marks the poleward edge of the Hadley cell. Thus, the downward motion induced by upper-level eddy momentum flux convergence serves as the dynamical origin of the indirect Ferrel cell.

Changes in the text:
This will be better described in the revised paper.

Lines 86-93: I think there should be more discussion on previous parameterizations of the heat and momentum fluxes. This could come at the expense of shortening lines 20-85 in the introduction. The following studies explored the properties of the midlatitude circulation using diffusive parameterizations for the eddy fluxes, or using scaling laws based on geostrophic turbulence theory, to name a few (see bibliography below): Held and Larichev (1996) – cited in this manuscript but not in the introduction; Pavan and Held (1996); Lapeyre and Held (2003); Zurita-Gotor (2007); Thompson and Young (2007); Scheider and Walker (2008) - cited in this manuscript but not in the introduction; Jansen and Ferrari (2015); Chen and Plumb (2014). These papers and others should not only be cited, but also there should be given an overview of the current knowledge of how the midlatitude circulation depends on the non-dimensional parameters of the system, and the open questions addressed here should be highlighted.

Response:
Equation (1) is presented in non-dimensional form following Pedlosky (2013). Since this is a standard procedure, we have omitted the detailed derivation. However, as you correctly pointed out, some important steps are missing, which could lead to confusion.

Changes in the text:
We will provide a more detailed derivation in an appendix to the paper, including the references to the papers mentioned.

Line 128: I couldn't understand the definition of Θ_0. Is it really necessary to define the potential temperature using two notations (θ and η)? I couldn't understand why you are using the derivative of Q with respect to θ_0 and what it means.

Response:
Following Pedlosky (2013), Θ_0 represents the leading-order potential temperature, with its scaling determined by the Rossby number. In large-scale atmospheric dynamics, thermodynamic forcing is typically modeled using Newtonian relaxation toward the radiative-convective equilibrium potential temperature.

We explained the rationale for applying Newtonian relaxation in that paragraph. In this context, the thermal forcing in the heat equation corresponds to the local radiative-convective equilibrium, denoted by Q. The equilibrium potential temperature, Θ_E, is defined as the value for which $Q(\Theta_E) = 0$. Although Θ_0 differs from Θ_E due to large-scale dynamics, the difference $\eta_0 = \Theta_0 - \Theta_E$ is asymptotically small compared to Θ_E itself. This is consistent with the dominant seasonal cycle of temperature observed globally. Typically, Θ_E is on the order of $300K$, while η_0 is less than $10K$.

Given this, it is a good approximation to apply a Taylor-series expansion of $Q(\Theta, \dots)$ around $\Theta = \Theta_E$. The leading-order term in this expansion corresponds to the Newtonian relaxation term, which serves as the thermal forcing in large-scale atmospheric models.

Changes in the text:
We will describe this reasoning in more detail in the revised paper.

Jumping back and forth between neglecting and not-neglecting the non-conservative terms makes it very confusing. I suggest the authors to plan the order of the equations so the transition between neglecting to not neglecting these terms would be done only once.

We tried to make sure that the omission of non-conservative terms is logically and mathematically done but will improve this in the revision.

The description of the eddy life cycle is repeated too many times in the manuscript. It is enough to describe it once (for example in lines 161-166), and say that the parameterization used here tries to capture the integrated effect of eddies over many life cycles.

In the revision, redundant parts will be shortened or erased.

In some places the authors mention "previous studies" without citing them explicitly (lines 199-200, 373).

We will include proper citations.

In several places it is argued that the adiabatic heating and cooling by the vertical motion is induced by eddy momentum flux (lines 215-217, 592-593, 633, 637, 647). However, eddy momentum flux is only one of the factors controlling the vertical motion of the overturning circulation, all other terms in the zonal-mean zonal momentum and heat equations also play a role (see various studies which solved the Kuo-Eliassen equation).

Response:
Our main equation is analogous to the Kuo-Eliassen equation, incorporating parameterized eddy fluxes and excluding external forcing. The Kuo-Eliassen equation is derived from a combination of the zonal momentum and thermodynamic (heat) equations. According to this framework, the components of meridional circulation are thermally and mechanically interconnected, making it difficult to isolate one process as the driver of another. However, by considering the transient life cycle of unstable baroclinic waves —from their generation to eventual breaking—we find that adiabatic warming or cooling is induced to maintain thermal wind balance, particularly following the intensification of upper-level zonal-mean zonal wind during wave breaking. These interpretations are grounded in the time evolution of baroclinic wave dynamics.

Changes in the text:
We will include this interpretation in the revised paper.

Figure 2 – what is the range of latitudes and pressure levels used to calculate the dots in panels a and b?

In panels (c) and (d), yellow dots are visible, surrounded by black dots. The values of D and M shown in panels (a) and (b) are calculated using data from the black dots. These areas are selected because they are the primary regions where eddy momentum and heat fluxes are generated.

Section 2.2 leads to equation (11) in an overly complicated way. I suggest that the steps to get to this equation would follow this order: 1. The zonal momentum equation is in steady state and expresses the balance between the Coriolis force and eddy momentum flux convergence (EMFC). 2. The heat equation is in steady state and expresses a balance between adiabatic heating by vertical motion and eddy heat flux convergence (EHFC). 3. Continuity connects the Coriolis force and the adiabatic term, so that together with the momentum and heat equations leads to a relation between EMFC and EHFC. 4. Using diffusive approximations allows to express the EMFC and EHFC in terms of zonal wind and temperature. 5. Use thermal wind balance to turn the equation into an equation with only one variable (temperature).

Thank you for your suggestions. In the revised version, we will take these steps into account to improve the derivations.

Figure 3 – I don't think this figure is necessary. The boundary conditions should be listed clearly in the text, not in the figure.

We will include the boundary conditions in the text and remove the figure.

Equation 13: It is strange that u_0 was regarded as the zonal wind and then it turns out to be one of the components of the total zonal wind (u_T). Specifically, the eddy momentum flux parameterization should be related to the total zonal wind and not just one of its components. If u_s has a meridional shear, it would affect the eddy momentum flux as well.

You are right. The zonal-mean zonal wind u_0 should be expressed as the vertical integral of the meridional temperature gradient, with the addition of u_s. We will update this accordingly in the revised manuscript.

Lines 297-299: The use of the word "geostrophic" in this paragraph is confusing. Both u_0 and u_s are referred to as "geostrophic", so why is it mentioned interchangeably that they are geostrophic as if the other term isn't?

Yes, this was confusing. What we meant was that the zonal wind—not the geostrophic wind—is zero at the surface. We will update this in the revised paper.

Equations 14-22: I don't see the added value of deriving the Ekman layer equations here. It is standard to approximate the friction at the boundary layer by a linear drag parameterization, which leads to equation (22) and makes this derivation unnecessary.

Response:
This section is intended to demonstrate that the typical boundary condition presented in Equation (20) is equivalent to the zonal momentum balance described in Equation (24). In deriving the main equation, we do not explicitly include the surface friction term. Instead, the surface boundary condition applied to the vertical velocity implicitly accounts for the influence of boundary layer friction. To illustrate the role of surface friction in the midlatitudes, we refer to Equation (6). Our goal is to show that the surface boundary condition is effectively equivalent to incorporating surface friction as represented by Equation (6).

Changes in the text:
We will move this section to an appendix and only use the result in the main text.

Equation 23 repeats equation 6, but without the friction term. The connection should be clarified. Why is the friction term neglected and not neglected interchangeably?

Response:
As explained above, the purpose of this section is to clarify that the surface boundary condition derived from the Ekman solution is equivalent to incorporating surface friction in the momentum equation. The main equation is derived without an explicit friction term, which is why we use Equation (23). However, applying the boundary condition in Equation (20) without explicitly including surface friction is effectively the same as including surface friction in the zonal momentum equation. This is the key message of this section.

Changes in the text:
The connection is clarified in the revised paper.

Line 359: Is this a definition of a new parameter c(z)? I don't see what you need this notation for if it's not used later.

Equation (31) is derived from the TEM equations in the absence of external thermal or mechanical forcing. Since the meridional (y) derivative of the EP flux convergence is zero, the convergence of the EP flux depends only on the vertical coordinate z, as stated in line 359. This will be explained in the revised text.

Equation 33: Why repeat equation 11 again?

As explained earlier, we presented two approaches to deriving the main equation. Equations (1–11) illustrate the conventional method based on quasi-geostrophic (QG) theory. The second approach begins with the transformed Eulerian mean (TEM) formalism, which has the advantage of linking the main equation to the Eliassen–Palm (EP) or potential vorticity (PV) flux. This section demonstrates that the main equation includes a zero PV flux solution, which corresponds to the non-acceleration theorem.

I think the discussion of the non-acceleration theorem should be given once, and in the first place where the equations are presented without the non-conservative terms. It is only presented in line 366, but it should be explained earlier.

Response:
The most appropriate place to introduce the non-acceleration theorem is after demonstrating that the main equation includes a vanishing Eliassen-Palm (EP) flux. The key question at this point is how the mean flow is structured in the presence of zero EP flux. Since this directly relates to the central idea of the non-acceleration state, it is the ideal moment to discuss the non-acceleration theorem.

Changes in the text:
We place the explanation shortly after presenting the main equation, and then explain in detail when the TEM formalism is suggested.

Section 3.1: This section should begin with a text motivating the analytical solution and explaining where this is going and why. Instead, the analytical solution is presented here without any explanation, justification or description of the model assumptions.

Thank you for the suggestion. We will include a summary or a description of the motivation behind constructing the analytic solutions.

Equation (35): One term is missing, with the derivative of Q with respect to z.

If you examine the form of the solutions, you'll notice the presence of the factor $\exp(z/2H)$ multiplied by $Q(z)R(y)$. This factor effectively eliminates the derivative with respect to z, which is a typical technique used in solving the quasi-geostrophic (QG) equations via separation of variables. No changes in text.

Equations 39-50: This part of the manuscript is super-difficult to follow, and I don't think it should be. I think the number of variables and parameters could be reduced, to make the derivation more compact. Some of the equations could be moved to an appendix. The reader should be able to follow easily the path to the main equations used later, most specifically equation (43). All the definitions of the parameters in this equation should be easily found in the text.

Response:
We followed a standard procedure to obtain the steady-state solutions using separation of variables. This method is commonly applied when solving the Laplace equation in a rectangular domain.
Changes in the text:
We will more easily explain the solution procedure used here.

The use of the functions $f(q)$, $g(q)$ and $h(q)$ in the text and in figure 4 is very confusing. If $f(q) = g(q)$, why do you need two notations for the same function? It is not clear at first from reading the text and the caption of figure 4 what these functions represent in the solution. Is it the meridional or vertical component of the solution? Is it the solution for temperature or zonal wind? Is it just the first-order component of the total solution in equation (43) or is it the total solution? Why do you choose to show only the first mode in the sum in equation (43)?

This was a typographical error, which will be corrected in the updated version. Equation (40) represents an eigenfunction of the problem, associated with the eigenvalue q. Equation (41) presents the functional forms of the relevant variables and eddy fluxes. By applying the surface boundary condition (Equation 20) to the expressions in Equation (41), we arrive at Equation (42), a nonlinear algebraic equation. As shown in Figure 4, this equation admits an infinite number of solutions for q. Each eigenvalue q corresponds to a distinct eigenfunction, and therefore, the general solution can be expressed as a sum over all eigenfunctions—this is given by Equation (43).

The choice of parameter values for presenting examples for the solution is not motivated (lines 411).

Response:
Here ϵ is the Rossby number, which typically has a value around 0.1 in the atmosphere. The Ekman number is selected such that the friction coefficient, given by $\sqrt{E_v}/2\epsilon$, remains an order-one constant. S denotes the non-dimensional dry static stability, and we adopt a standard value of 1.0. Similarly, H represents the non-dimensional density height, also set to 1.0. The parameter D is the eddy heat diffusivity, chosen based on estimates from Figure 2. Notably, the value of D aligns closely with that used in prior research on eddy memory (Moon et al., 2021).

Changes in the text:
This explanation will be included in the updated version.

Equations 45-50: What does the subscript "1" represent?

Response:
In principle, the solution should consist of an infinite sum of eigenfunctions. However, we lack detailed information about $f(z)$, which represents the influence transmitted from the tropics to the mid-latitudes. Observationally, the average vertical structure of the large-scale atmosphere in the mid-latitudes exhibits a single maximum in the interior. For instance, the eddy heat flux typically peaks around $700mb$, while the eddy momentum flux reaches its maximum near the top of the atmosphere. Given this structure, the first eigenvalue ($q1$), which is slightly less than $\pi/2$ as shown in Figure 4, and its corresponding eigenfunction, most closely resemble the observed atmospheric profile. For the sake of simplicity, we choose to focus solely on this first eigenvalue and eigenfunction, denoted by the subscript 1. The overall structure of the eigenfunction 1 is shown in the figure 5.

Changes in the text:
This explanation will be added in the revised paper.

The discussion in lines 441-489, including the description of figure 5, relates to the solution for η_0 (the total temperature profile). However, according to equation 43, η_0 is a sum of an infinite number of modes, while the solution discussed here is only the first mode in this sum (is that correct?). Are you assuming that the first mode is larger or more important than the other modes, or are you simply choosing it as one example? This should be clarified before the beginning of the discussion. Also in section 3.2 the first mode is treated as if it is the general solution.

Response:
As discussed above, the structure of the first eigenfunction closely resembles that of the real atmosphere, as it exhibits a single maximum in the interior. Additionally, the eddy heat flux reaches its maximum just above the surface, while the eddy momentum flux peaks near the top of the atmosphere—features that are consistent with the vertical structure of the first eigenfunction. Although the general solution is a sum of infinitely many eigenfunctions with coefficients determined by the function $f(z)$, we focus solely on the first eigenfunction due to its close similarity to observed atmospheric profiles.

Changes in the text:
This clarification will be added to the revised paper.

Figures 4 and 5: it would help the reader if the caption would mention what each parameter represents. Also, the choice of parameter values (line 475) should be explained. Are these parameter values realistic for the atmosphere? Are they based on the GCM simulation?

Response:
Suggestion followed.

Changes in the text:
We will include detailed information in the captions of Figures 4 and 5. The selection of parameter values is based on a combination of numerical simulation data analysis and typical atmospheric values. For instance, D and M are chosen based on the analysis presented in Figure 2, while H and S are set close to 1.0, reflecting standard non-dimensional values used in atmospheric studies. These values are slightly adjusted to center the main structure within the domain. As anticipated, the solutions are constructed on a beta-plane, and therefore, they do not represent the full complexity of the real atmosphere.

Lines 500-502: This is repeating what was written in the previous section.

Response:
This sentence is meant to underscore the role of the sine function in shaping the structure of the idealized Ferrel cell. The entire circulation—including v_1, w_1, and u_0, as well as the eddy heat and momentum fluxes—is fundamentally governed by the sine dependence of the anomalous potential temperature. We emphasize this point here to highlight the central role of this functional form. Ultimately, this structure can be interpreted as an explicit expression of the zero in the Eliassen-Palm (EP) flux convergence, or equivalently, the potential vorticity (PV) flux. Although this idea is mentioned again later, the repetition serves a different purpose.

Changes in the text:
In the updated version, we will refine these sentences to clarify this distinction.

Section 3.3: Previous studies examined how changing the parameters of the flow equations changes the number of jets (or equivalently, the jet width). The conclusions of these studies should be mentioned here and compared with the current results. To name a few: Panetta (1993); Pavan and Held (1996); Esler (2008); Lee (2005); Chemke and Kaspi (2015). Overall, the manuscript does not relate to much of the previous literature, and it is not clear what new contribution arises from these results.

Response:
Thank you for your suggestions.
Changes in the text:

We will aim to reference relevant studies in this section. As you can see, the goal of this research is to introduce an analytical approach to studying large-scale atmospheric dynamics. It serves as a pilot study that lays the groundwork for future investigations. The parameters D and M are related to findings from various previous numerical simulations and reanalysis data. For instance, the activity of baroclinic instability in the mid-latitudes can vary depending on the planetary rotation rate or thermal forcing at high latitudes. However, providing a solid and quantitative connection to past numerical experiments requires further modeling or observational studies. At this stage, it may be premature to draw definitive links with previous research, and we acknowledge that a more comprehensive comparison should be addressed in future work.

Lines 536-537: This sentence is not clear. What do you mean by “multiple vertical layers”?

Response:
Previously, we focused on the first eigenvalue, as the structure of the corresponding eigenfunction closely resembles that of Earth’s atmosphere. However, other planets, such as Jupiter, exhibit distinctly different vertical structures. For example, Jupiter features multiple zonal jets, unlike Earth. Based on the results presented in this study, we may infer that the vertical structure of Jupiter’s atmosphere likely includes multiple maxima in the interior. According to the steady-state solutions, the vertical mode is intrinsically linked to the meridional structure. In this context, ”multiple vertical layers” refers to vertical profiles of relevant variables that exhibit multiple local maxima.
Changes in the text:
This clarification will be added to the revised paper.

Line 546 and equation 53: Why is q_1 the smallest positive solution? Again, it is very difficult to follow the equations and the notations.

Response:
As mentioned earlier, the q_1 is the first eigenvalue determined from the equation (20). The term “first” indicates that the absolute value of q_1 is the smallest among all possible eigenvalues.

Changes in the text:
The meaning of q_1 will be clarified better in the revised paper.

Lines 551-557: These lines repeat things that were written in previous sections.

We will remove the repetitive parts in the revised manuscript.

Caption of figure 8: I don’t see any black dots in panel a. Also, how are L_1 and the size of the Ferrel cell calculated?

Response:
L_1 is calculated using Equation (53). The eddy fluxes, as well as the horizontal and vertical shear of the zonal-mean zonal wind, are evaluated near the center of the jet stream. The size of the Ferrel cell is determined using the mass stream function, with its edge identified by the sign change of the stream function.

Changes in the text:
Figure 8a will be updated properly and the calculation of L_1 and the Ferrel cell size will be clarified.

Line 568: Is the meridional shear calculated around the center of the jet? Isn’t it zero there?

Response:
The meridional shear is approximated by $\Delta U/\Delta y$, where Δy represents the distance from the center of the jet to the edge of the Ferrel cell. This length scale used to estimate the meridional shear is therefore comparable to the size of the Ferrel cell.

Changes in the text:
This clarification will be added to the revised paper.

Lines 577-582: The subject discussed here disserves more careful attention. What determines the relation between the eddy heat and momentum fluxes? The authors should refer to previous studies addressing this question. My understanding is that this ratio is related to the Burger number $(NH/fL)^2$ – see AMS glossary for example. This non-dimensional number gives the ratio between the stretching term and the vorticity term in the QG PV. I think that what the authors call "the structural number" could be expressed in terms of the Burger number. This would help to relate it to previous studies.

We are strongly tempted to draw a connection between the structure number and the Burger number. As noted, the Burger number characterizes the ratio between vertical and horizontal motions. However, the relationship between the magnitude of the vorticity term and the degree of wave breaking that generates the eddy momentum flux remains unclear. Although this is an important and interesting question, it is outside the scope of the present paper.

Line 594: What is "external heat flux"?

We will change it to the local radiative-convective processes. "External heat flux" seems to be confusing to define the radiative-convective equilibrium.

Line 608: Did you mean "non-acceleration" instead of "non-zero acceleration"?

Yes, you are correct. We will change it in the updated manuscript.

Section 4: Again, it is not clear what is the new contribution of this study. This should be clarified in this section.

We explained earlier about the new contribution of this study. It will be updated to emphasize the major contributions in the updated manuscript.

Lines 624-625: How does this research highlight that?

Response:
This represents the central point of our research. The solution is expressed in terms of trigonometric functions, modulated by the structure number. The magnitude of the structure number determines both the size and position of the idealized Ferrel cell. Consequently, understanding how the life cycle of baroclinic waves influences the structure number is key to determining the characteristics of mid-latitude indirect circulation. This study highlights that connection by presenting explicit analytical solutions.

Changes in the text:
This clarification will be added to the revised pape

Lines 627-630: This paragraph doesn't add any information. I would suggest to remove it.

We will remove it in the revised manuscript.

The appendix is not referred to in the text. I suggest removing it.

We will remove it in the revised paper.