Answers to both reviewers:

Dear Reviewers,

We are grateful to both reviewers for their corrections and comments as they have increased the quality of the paper. Please find our detailed answers and corrections to both reviewers comments (reproduced in black) below, written in blue. The corrections performed to the manuscript are given in red and an updated version of the manuscript with the corrections highlighted in red is provided.

Reviewer 1:

The authors have satisfactorily addressed all points raised during peer review. The "Results" section in particular is easier to understand now and the "Discussion" gives readers a better idea of the potential benefits of an FT. I have only a few very minor remarks that could be dealt with before publication.

p.2 l.3 missing space between "the" and "shear"

Done

Section 4 If you do not wish to include a map of the domains, I will not insist on it. I just wanted to mention that you could theoretically also add a video (as mentioned in your reply) as a supplement if you thought that would help readers understand your ideas better. It is probably not necessary though.

I will make a video of this research work

p.6 l.3 "twice the variance" do you really mean variance or standard deviation? Variance would be a little bit weird as it has units of z squared so this kind of range only makes sense for unit-less variables like yours.

Done

p.6 l.6 "exp. 1" better to use "ASG" as this is the only remaining use of "exp. X"

Done

p.6 l.24 "less than t_0" this should be "τ_0", right?

Done

p.9 l. 20 remove the "." after "year"

Done

Thank you!

Reviewer 2:

Review of

Empirical evidence of a fluctuation theorem for the wind mechanical power input into the ocean by Wirth and Chapron

Summary and recommendation: The revised version of this paper only differs in minor ways from the previous one and has not really improved. The more I read the paper, the more I find it confusing. Although it is easy to follow, I think that this paper does not sufficiently pay attention to details, does not sufficiently justify or explain its methodology, and does not critically discuss its results and their robustness enough. The following provides a list of what needs to be improved to meet the scientific standard required for publication. All the issues listed should be addressable relatively easily. All is required is that the authors try to put themselves in the shoes of a regular oceanographer.

Major Issues

– **Definition of the wind stress**: The first main issue that I find problematic is the authors' claim that the wind power input is dominated by the shear stress component of the wind stress without any form of justification to back up this claim.

To my understanding, I answered this concern in my last reply to this reviewer and made changes in the text to make it more clear. In our work the smallest horizontal scale is several tenths of kilometers. Parameterizations of the meachanical power input to the ocean are based on the difference between the atmospheric winds and the ocean currents near the surface at large horizontal scales. In this case there is shear-stress at the interface. How the power is actually transmitted at the molecular scale or at the scale of the waves (meter to a few tenths of meters) is another question. At the wave-scale the predominant process is definitely the form stress. Including text which discusses the processes at scales which are not considered in the paper will only lead to confusion. We therefore refere the reader to published work dedicated to this subject.

I also find it problematic that the authors never mention or acknowledge the existence of the form stress due to atmospheric pressures fluctuations on surface waves and swell, Grachev et al. (2003) https://doi.org/10.1175/1520-0485(2003)033%3C240 for instance. As far as I am aware, the 'wave' stress is not in general negligible, as it can cause the wind stress direction to be different from that of the wind and drive an atmospheric jet in the regions of light wind, see Hanley and Belcher (2010). Of course, the authors have the right to retain only the shear stress in their calculations if they want to, but they should explicitly acknowledge that this is an approximation that could potentially invalidate their results.

It was and is written in the paper: "In the present work we are not concerned with the details of the exchange in the respective boundary layers (see e.g. **?**) but suppose that it is well represented through bulk formulas of air-sea interaction (**?**)." The first work presents a detailed discussion on the processes at the wave-scale and below, while the second discusses the details of bulk formulas and gives the important references on the subject.

Moreover, I also checked the paper by Fairall et al. (2006) cited by the authors as the basis for the wind stress calculations and found that the authors' approach seems to be different. Indeed, in Fairall et al., the wind stress is calculated as $\tau = \rho_a C_d S(U_{10} - U_o)$, where S is the average atmospheric wind, not the instantaneous wind relative to ocean surface currents.

Fairall et al. write (page 572, lines: 39 –43): "[...] S is the mean wind speed (relative to the ocean surface), which is composed of a mean vector part (U and V components) and a gustiness part (U g) to account for subgrid-scale variability:" The averaging referes to the average over the grid-scale as opposed to unresolved subgrid-scale quantities. The bulk formulas give the products of subgrid-scale quantities, averaged over the grid-scale, based on quantities averaged over the grid scale. Every observation includes some averaing in time and space, so does ours, performed every 6h and averaged over a square exending $1/2^o$ in both horizontal directions, our grid-scale. And yes, it is relative to ocean surface currents as imposed by the Galilean invariance of Newton's laws.

Moreover, it is not clear from the paper what Uo the authors are using in their calculation of the wind stress. The formula should be used with the total surface velocity including both ageostrophic and geostrophic component, but the way the paper is written suggests that the authors may have used the 15 meters velocity instead. It is essential that the authors clarify this point.

It was and is written in the paper (end of the "Power Input" section): "In the present work, largely building on 15-m drogued drifter velocities (**?**), we use for $\mathbf{u_o}$ the estimation of the current velocity at 15m depth." Please note that in a stationary Ekman layer the surface velocity is at 45^o (const. viscosity) to the shear, while the Ekman transport is at 90^o. So in this case power is provided to the surface flow but not to the Ekman transport. Straightforward calculations show that that the difference is dissipated by viscosity. Taking the surface velocity would mean including the power-input that is dissipated in the Ekman spiral upon injection. This can be avoided (reduced) by taking the velocity below the Ekman spiral. So we use the ageostrophic component minus the Ekman velocity at the surface and the geostrophic component. For basic calculation on the Ekman-dynamics please see: https://hal-bioemco.ccsd.cnrs.fr/INPG/cel-01134110

We now added:

By taking the velocity at 15m rather than at the surface we exclude the power that is promptly dissipated by viscosity in the Ekman-spiral, but include the power that is supplied to the ageostorpic and geostropic dynamics of the core of the mixed layer.

– **Definition of the wind power input**: The second main issue that I find problematic is the authors' definition of the wind power input. As far as I know, the wind power input is by definition the product of the total wind stress by the total surface oceanic velocity, which can be decomposed as the sum of a geostrophic plus ageostrophic part. I think that the authors are right that a large fraction of the wind power input goes into mixing the mixed layer and ultimately dissipated into heat, but my understanding is that this is related to the ageostrophic work.

As described above the ageostrophic motion can be split into Ekman transport and the part of the sub-mesoscale motion that is not in geostrophic balance. Taking the velocity at 15m excludes the input that is promptly dissipated in the Ekman layer.

Indeed, Roquet et al. (2011) https://doi.org/10.1175/JPO-D-11-024.1 provides physical arguments for why the work against the geostrophic component should be the one driving the large-scale circulation. Based on previous work, I would therefore expect that the right way to compute the wind power input would be by using the surface geostrophic component of the GlobCurrent product. What is the justification of using the 15 meters current? Moreover, the GlobCurrent product description says that both the Ekman and geostrophic components, as well as their sum, is available at 15m. Which one do the authors use? This absolutely needs to be clarified. Moreover, it is essential that the computations are repeated by using the surface geostrophic velocity and the computation of the wind stress actually proposed by Fairall et al. (2006).

When the geostrophic current is used the power injected to the geostrophic circulation is calculated. This, however, excludes the power supplied to the submeso-scale motion that deviates from geostrophic equilibrium, which is today the center of much research. This is stated by Wunsch (1998 JPO 28, 2332), who writes about taking only the geostrophic part: "Equation (1) fails if there are extensive regions where the dynamics differ from simple geostrophy plus an Ekman layer." Using the 15m velocity prevents (part of) this failure. Furthermore the supply to the geostrophic circulation is important when the global or basin-wide circulation is considered. We look at the power-supply at rather fine resolution to a local region that spans only 10^o in both horizontal direction. Furthermore, note that following Roquet et al. (2011), not all of the wind work to the geostrophic circulation is injected locally to the geostrophic circulation but transported laterally by Ekman dynamics and the beaviour at basin boundaries becomes important. Therefore: the local injection to the geostrophic circulation is not injected to the local geostrophic circulation. This makes a regional analysis as we perform it questionable (to our understanding) when applied to the geostrophic flow.

- **Negative power input**: The physical meaning of the negative power input events is unclear. If the computations were done for the full wind stress and the ocean surface velocity, they would correspond to instances where momentum is transferred from the ocean to the atmosphere. These events have received some attention in the literature, e.g., Hanley and Belcher (2010) https://doi.org/10.1175/2010JPO4377.1 but these involve the role of swell in regions of light winds. What do these negative events mean here? The authors claim that strong negative events would cause mixing and turbulence (without providing any reference to back up their claim) However, the authors chose the 15 m precisely to avoid this situation, to focus exclusively on a quantity that drives the large-scale circulation, not the turbulence. Doesn't that suggest that their whole approach is inconsistent?

  Negative events, the ocean loosing energy, are not necesarrily associated with the atmosphere receiving mechanical (gridscale) energy. Note that when wind and current are in oposing directions, both media are slowed down, loose mechanical (grid scale) energy. This resemble the inelastic collision of particles, we mentioned in the previous reply to this reviewer. When the horizontal circulation in both media at large scales (our grid scale) looses energy the energy has to go to smaler scale (sub-grid scale) turbulence. I am not aware that this has been studie in the literature and can provide no reference. I do not see any inconsistency in our approach.

Specific comments

1. Page 1, Line 1: Abstract 'The ocean dynamics is predominantly driven by the shear-stress between [...]' I don't think that this is true. As stated in my previous review, the form stress due to pressure fluctuations on surfaces well and swell often represents a significant component of the wind stress, which can even modify the direction of the wind stress relative to that of the wind. In any case, why is it important for the argument that only the shear stress be retained in the calculation rather than the full wind stress? The authors need to explain why they don't want to include the wave stress in their calculation.

At the scale of the wave, the transfert of momentum is done by the form-stress, but for this we need (on average) a difference between the ocean current and the wind near the surface. This is what I wanted to explain in my answer to the first report and also above in this answer.

2. Page 1, Line 15: '[. . .] which is described by the fluxes of mechanical power' Is that the right expression? What is the expression for such fluxes?

   The sentence is now changed to: "In the present work the exchange of momentum is considered. More precisely, we investigate the flux of mechanical power into the ocean mixed-layer at the ocean surface."

3. Page 1, Line 17: 'In the present work we do not discuss the various physical processes occurring at the air sea interface which are important' It concerns me that the authors don't feel it is needed to discuss the physical processes relevant to their argument. The authors need to acknowledge the different contributions making up the wind stress and explain and justify why they neglect the wave stress

   The sentence is now changed to: Various physical processes occurring at the air-sea interface on a large range of scales in space and time are important for the momentum transfer.

   The form-stress on the waves is of course the major part of the total stress at the wave-scale, but it is taken into account in the bulk formulas. This is explained in detail in the works that we cite in the paper. We did not atempt a review of the important physical processes at the air-sea interface at our sub-grid scale in the paper, as it would considerably increase its length and refere to published work.

   Please note the citation from Fairall (p 584, 2. colomn line 15-22) "In the wind speed range 0–20 m s^{-1} the major remaining surface physics issue is the influence of surface waves on the fluxes. With present techniques, a huge number of observations will be required to obtain definitive results because of the addition of one or two independent variables. High-quality, routine measurements of wave properties is an important technical challenge, so we must hope for a breakthrough in theory or modeling." In our work we applied *Occam's razor* and did not consider the variation of the drag coefficient with the shear. Other choices are clearly possible.

4. Page 1, Line 22: 'The energy exchange is not conservative and most of the mechanical energy is dissipated' I still do not understand what the authors want to say, and what is the point they are trying to make. The answer does not help.

   We say that in air-sea interaction mechanical energy is not conserved.

5. Page 2, Line 24: 'Furthermore, the research interest [. . .]' The examples of whether and climate seems ill chosen, since there is as much interest in the fluctuations as in the average. Climate is by definition an averaged quantity, and estimating the state of the atmosphere at any point in time is crucial for initialising weather forecasts.

   The full citation in our paper reads: "Furthermore, the research interest in many natural systems lies mostly in the fluctuations rather than in an average state, weather and climate dynamics are examples where we focus on the fluctuations of the same system on different time scales. For the weather the time scale of interest is from roughly an hour to a week, for the climate the focus is from tenths to thousands of years and beyond." We did never say that the state of the weather and the climate is not important. Weather forcast is concerned with the evolution of the state of the atmosphere. Climate is an average over a certain time interval. If climate were and average over all the data available there would be no climate change ? I typed on google "climate variability" and got 10^9 results, "climate fluctuations" gives 7×10^8 results. We did not invent its importance. We now changed the sentence to: Furthermore, the research interest in many natural systems lies also in the fluctuations not only in an average state,

6. Section Power input. I still don't understand what the authors are doing. The expression differs from that of Fairall et al. (1996), for which the term within the square root only involves the atmosphere wind, not the wind relative to ocean surface currents, see major issue above.

   Fairall et al. write (page 572, lines: 39 –43): "[...] S is the mean wind speed (relative to the ocean surface), which is composed of a mean vector part (U and V components) and a gustiness part (U g) to account for subgrid-scale variability:" . To my understanding relative to the ocean surface means: $u_{10} - u_o^S$, this is also what follows from the Galilean invariance of Newton's laws.

7. Section 4 – Data. I checked the GlobCurrent product description, and it seems to me that this section does not represent an accurate description. In particular, the GlobCurrent website states that different velocities are provided at either: 1) significant wave height, 2) z=0, 3) z=15m. Moreover, both the Ekman and geostrophic velocity are separately available at z = 0 and z = 15m, which is not acknowledged. The authors need to do a better job at explaining what GlobCurrent actually provides, and which exact quantity they are using, whether it is the geostrophic current only or not.

   We give a reference to the GlobCurrent product description and to the major publications in which it is discussed in detail.

8. Section 6 – Discussion. 'We obtain clear evidence that a FT applies to data within the recirculation' I disagree that this has been scientifically established, because the authors did not test the sensitivity of their conclusions to the various assumptions made. In particular, the authors need to test whether their results still hold if they use the surface geostrophic velocity and the correct expression for the wind stress.

   Our expressions of the wind-stress is correct, as we show above. Concerning the geostrophic velocityplease see discussion above. We now deleted "clear".

9. Page 9: 'Extreme negative events lead to strong transfer of energy to small scale turbulence in the atmospheric and oceanic boundary layers, [...]' What is the evidence backing up such a claim? Is this a fact or a speculation? What do they mean by extreme negative events? Doesn't a negative event correspond to transfer of momentum from the oceans to the atmosphere?

   Strongly negative events, the ocean loosing energy, are not associated with the atmosphere receiving energy. Note that when wind and current are in oposing direction, both media are slowed down, loose energy. So a strong loss of energy of the ocean asks for a strong wind against the ocean velocity. When the circulation in both media looses energy the enegy has to go to smaler, subgrid, scale turbulence. I am not aware that this has been studie in the literature and can provide no reference. I do not see any inconsistency in our approach.

   Please note that "Extreme negative events" had already been changed in the manuscript to " Improbable negative events " following the editors request.