Answer to reviewer 4:

Dear Reviewer,

We are grateful for your corrections and comments as they have increased the quality of the paper. Please find our detailed answers and corrections to your 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.

Sincerely,

Achim Wirth, Bertrand Chapron

Reviewer 4:

In full disclosure, I didn't know of the existence of the fluctuation theorem before reading this paper, so please forgive me if some of the comments below may appear naive or out of place.

I am not opposed to the research presented in this manuscript, but I don't understand who it is addressing. If it is addressed to the GFD community, as the motivations and discussion suggest, more efforts should be made to explain why the FT is important and why should one care about it for the power input into the ocean. Yes, we expect some negative values of power input into the ocean over a certain averaging in time and space, and their probability is linked with those of positive values, but so what? Why are these rare negative events particularly relevant for the dynamics of the ocean? In this manuscript, the discussion of the relevance of FT for ocean dynamics is often hand-wavy or redirected to other articles. From an outsider's point of view, it looks like the method is blindly applied to the power input without explaining what oceanic knowledge is gained from it. Turbulence has an imprint on many oceanic quantities, why focusing on power input specifically?

For a statistical quantity all we can know is the pdf and time correlations. In most applications we only know them very roughly because of lack of data. But for some FTs seem to apply, suggesting some underlying symmetry. This is explained in the paper at several places. We understand the criticism of the reviewer, previous reviewers have made comments in this direction and we have added two paragraphs to the discussion section. Making statements beyond this seems to be not justifiable at the present time. Fluctuation theorems have, in the last 20 years revolutionised molecular dynamics and non-equilibrium statistical mechanics and are just about to be applied to fields outside, to fields where fluctuations are non-thermal (turbulence). FTs are recently looked at in wind-tunnel turbulence (https://arxiv.org/abs/2104.03136v1) and other applications (cited in the paper). My focus is on air-sea interaction for which I have recently published a series of theoretical papers (see refs. in the paper) and the present paper is a link of the theory to observations. If we want to understand the turbulent dynamics of the ocean we have to start with the forcing, which is to my understanding (one of) the weak point(s) in ocean modelling. This includes knowledge about the variability of the forcing. But most important: huge amounts of data are available at the ocean surface. This huge amount is barely enough to discuss the existence of FTs.

We now added to the discussion section:

Recently there is an increasing interest in the variability of atmosphere and ocean dynamics and in the exchange of the two, also at high frequency and their contribution toward the lower-frequency air-sea momentum and energy fluxes (e.g., Zhai et al. (2012), Wirth (2021)). It was found that higher frequency wind forcing increases the mixed layer depth (Zhou et al. (2018)). There is evidence that long term variability of the atmosphere ocean system as the Madden-Julian Oscillation and El Niño-Southern Oscillation, are influenced by higher frequency wind forcing (e.g., Bernie et al. (2007), Terray et al. (2012)). FTs when they apply give a connection between events averaged over different lengths of time and can help to evaluate the impact of not explicitly resolved scales. When they do not apply they might help to identify a specific, non stochastic, mechanism responsible.

Some more specific points are detailed below:

On page 6, the authors argue that the pdfs become more centered around unity as tau increases. I don't see this. I would argue that they all converge somewhere between 0 and 1, why is this?

The reviewer is right, looking at the figures this is not clear. When averaged over all data the pdf is a δ-function at 1, by definition. The convergence is at a slow rate, the variance decreases $\sim \tau^{-1/2}$ and data is presented at constant time increments

$\Delta\tau = 125$ for increasing τ. If one looks at graphs of $\tau = 125$, 250, 500, and 1000 days (constant ratio) it is obvious that "the pdfs become more centered around unity as tau increases". We now added:

With increasing averaging period, the pdfs become more centred around unity, which is the average value, see eq. (**??**). This can be verified by comparing the graphs for $\tau = 125$, 250, 500, and 1000 days. It is a consequence of the central limit theorem and occurrences of negative values become less likely for larger τ.

Why is the z-axis not always the same for left and right panels, and why isn't the z range not the same for the curves in each panel? For example, in Figure 1, p(z,tau) 1250 has values between -1 and 1 (pink curve in the left panel). Yet, S(z,tau)/tau has values only until 0.5, shouldn't S be defined in the range [0 1]?

In the right panel the values for positive and negative values of the pdfs at z are compared, so only points can be calculated if data is available for both $\pm z$. With increasing averaging time the values for $z < 0$ become less likely and so the analysis can no longer be performed. So if there are only values until 0.5 of S for larger values of τ this means that for $z < -0.5$ we do not have enough data to calculate S.

Page 9 lines 14-16, the authors argue that improbable events are key for the ocean without explaining why. A very simplified consensus in the literature is that most of the mechanical energy input into the ocean comes from the wind while most of the energy dissipation comes from interaction with topography. Are the authors arguing that a non-negligible part of this dissipation occurs via the interaction with the atmosphere itself?

Yes, surface friction can also deplete energy. This has recently been shown to be important at small scales (in space). Here we only consider temporal behaviour, as to the best of our knowledge no FT exists for fields (spatially extended domains). We now added:

The rare events when the ocean looses energy have recently been the focus of dedicated research (see e.g., Zhai et al. (2012), Wirth (2021)).

Top of page 10, "This suggests that an increased energy cascade, in the extension of boundary currents ..." I don't understand this argument. First, why should we expect an energy cascade in the extension of boundary currents? Is it a direct or inverse cascade? A baroclinically unstable jet will create eddies in the vicinity of the deformation scale, and these can be recycled in the jet itself or advected away. The resulting energy flux in such a small box around the jet is non-trivial and interactions are most likely dominated by non-local transfers. Second, what is the link between the energy cascade and the likelihood of a negative power input event?

The criticism of the reviewer is correct. We have seen a similar behaviour when a friction is added and a direct energy cascade acts as an eddy friction, this is however speculation. We are now more cautious in our argumentation and changed the sentence to:

This suggests that other processes than air-sea interaction dominate in the extension of the boundary currents leading to a departure from a FT symmetry.

Page 10, paragraph about "FTs in the more general context of climate dynamics". This paragraph is important if the manuscript is addressed to the GFD community, yet it is very superficial and speculative. The authors should make an effort of developing these points in a more precise manner and add examples with relevant literature.

FTs are just starting to be used in domains outside of molecular physics. We definitely think that there is a strong potential in interpretation of environmental data, but we do not want to oversell, so we give hints and suggestions of future applications. The suggestions given are true, but we do not know if they are useful in specific applications and we do not want to hypothesise on these points.

We thank the reviewer for the comments and corrections that have helped to increase the quality of the paper.

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