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Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Evidence of a fluctuation theorem for the input of mechanical power to the ocean at the air-sea interface from satellite data" *by* Achim Wirth and Bertrand Chapron

Achim Wirth and Bertrand Chapron

achim.wirth@legi.cnrs.fr

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[npg, manuscript]copernicus

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amsmath

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.

The purpose of our work is to extend the theory of FTs which is at the heart of non-equilibrium statistical mechanics for the last 30 years to climate science with interacting components. We give examples of immediate applications but to our understanding, a major benefit resides in the development of a new theoretical framework to further our understanding of the fluctuating interaction between different components of the climate system and their predictability as well as its limits to it. In the previous work we have discussed the application of the concepts (Fluctuation dissipation relation, Fluctuation dissipation theorem and the Fluctuation Theorem) to air sea interaction on the basis of idealized bulk formulas. In the present work we discuss the applicability to data from satellite observations.

Both reviewers are concerned with what concrete benefits FTs provide to the understanding of air-sea interaction and climate science. To address this concern we did several changes in the manuscript (see detailed answer below) and also changed the last two paragraphs of the paper which now read:

Finally, we put the theory of FTs in the more general context of climate dynamics. A measurement, especially when coming from satellites always contains some averaging in space and time. A FT, when it applies, will help to relate averages over varying periods and is a powerful tool to guide the up and down-scaling of observational data in time and obtain the statistical information on shorter and longer time scales, which are not explicitly observed. More precisely, when the pdf of the power supply, and therefore also the symmetry function is known form observations for given averaging times the symmetry function can be calculated for shorter and larger averaging times and constrains "half" of the pdf. This is useful in down-scaling and the construction of statistical parameterizations of not directly observed dynamics over shorter time scales. On the other hand, the information can be useful for developing models for the persistence of events over large time-scales not yet observed. A FT can help to decide if the persistence in time of a phenomena is within the likeliness of the statistically stationary dynamics or due to external influences. Furthermore, when data from observations follow (or not) a FT, model data should do likewise. As such, the FT becomes a tool of investigating the fidelity of models.

We conclude by looking at our results from the stand point of dynamical systems. Statistical mechanics of systems in equilibrium are described by the Boltzmann distribution, which is completely determined by the temperature. In non-equilibrium statistical mechanics no such universal distribution is known (see i.e. **?**, **?** and **?**), but some quantities in some processes seem to follow a FT which constraints the pdf and might indicate some universality. The mechanical power-input to the ocean by air-sea interactions, as a forced and dissipative dynamical system, may thus belong to a class of particular non-equilibrium systems exhibiting a FT symmetry property and offer guidance for climate studies.

Furthermore we like to mention that the applications of FTs in climate science are just beginning and other applications will possibly arise. In the present work we base our investigation on previously published theoretical / numerical investigation which show the existence of a FT in power-supply to the ocean in idealized models. We used a 24 years time series at 6h resolution and have just enough data to start seeing FT like behavior. But to our understanding, there is no doubt that climate science is looking towards a rapid increase (in quantity and quality) of available data and the question about the presence of FT like symmetries in the data can be answered more decisively, also for different variables than the mechanical energy input into the ocean. This allows to analyze environmental data based on theories developed in non-equilibrium statistical mechanics for the last 30 years. We are therefore convinced that the reviewers concerns about the immediate benefit of FTs for ocean science concerning prediction and modeling of quantities will disappear automatically with time.

Sincerely,

Achim Wirth, Bertrand Chapron

Anonymous Referee #1

This study investigates empirically whether or not the time integrated input of mechanical power from the atmosphere to the ocean obeys a fluctuation theorem. If this were the case, observations of the very common case where momentum is transferred from the atmosphere to the ocean could be used to infer probabilities for the rare opposite case. The paper is overall well-written and easy to follow, even if the reader is not closely familiar with ocean dynamics or fluctuation theorems. The core idea is sufficiently interesting for publication in this journal and constitutes a natural next step after the first author's previous study of conceptual models (Wirth 2019). The results appear to be somewhat inconclusive but this fact alone should not exclude the paper from publication. I am mainly concerned with the data analysis in section 5 which is not very clearly presented, both in terms of the methodology and the actual discussion and plots.

Specific comments:

p.4 l4-5 "fixed surface area" this is probably not very important but is the surface area actually fixed when the sea state can change over time? If you always consider fixed geographical regions, wouldn't calm conditions lead to a smaller surface area than rough seas?

The roughness of the surface is not considered here. I know changed to:

(the area which spans 10^o in the longitudinal and the latitudinal direction)

p.2 l22 "the focus" please make it clear whose focus you mean (the focus of most current research?)

I now changed to:

Furthermore, the research interest in many natural systems lies mostly in the fluctuations rather than in an average state, [...]

p.2 l33-34 “not only concerned with instantaneous values” if I understand correctly, eq.3 doesn’t refer to instantaneous values at all, right? In that case you should cut “only” here.

Done.

p.3 l30 please make it unambiguous that the limit of large τ relates to both conditions and not just (ii). Also this is the first instance where tau_0 occurs, please explain what this refers to.

It is now changed to:

The Galavotti-Cohen fluctuation theorem (called FT in the sequel for brevity) holds for $\mathcal{P}$, if for averaging times larger than a characteristic time scale of the system ($\tau \gg \tau_0$), two conditions are satisfied: (i) the symmetry function depends linearly on the variable z, and (ii) on τ:

p.5 l27f consider including a map of the world showing these four regions to give non-oceanographers at least some idea where they are located, how large they are and what factors might influence the different dynamics.

I did have a world map in a preliminary version of the paper, but the areas are rather small and not instantaneously visible. The solution is to put at least two maps, one for the North Atlantic and one for the North Pacific, but this takes too much space in my rather short paper and also I do already have many figures. Furthermore it is less the areas than their dynamic regimes which are important, which asks to include some current / wind information, which asks for individual zooms of the areas. Putting this might suggest that a FT can be eye-balled, which is of course not the case. When I give a talk on the subject I point towards the areas on a map, and show films of the current and the wind data considered, which resolves the problem. I therefore ask to

keep as is. It is a personal preference and other choices are clearly possible.

p.5 l27f do you have some idea how sensitive your results are to the specific choice of your domains?

We show that the FT “works” in the re-circulation areas considered and that it does not work in the turbulent extensions of western boundary currents. It is written in the paper that: “During data analysis, we also found that a FT does not apply when islands or coastlines are present (not shown here). Departure from a FT for the power input to the ocean is found where horizontal dynamics dominates over the vertical ocean-atmosphere momentum exchanges.”

Furthermore the analysis is very demanding in computer time which prohibits general investigation.

p.6 l1 what exactly do you mean by “an interval that spans twice the mean value [. . .] from the origin”? 0 +/- 2*mean(E_tau) ? In that case why is zero not at the center of the left parts of Fig.1-4 ?

We now replaced “mean” by variance. Not all the data obtained is shown in the graphs as the the averages over shorter time have a much lager variance. We adapted the range in the figs to have a good compromise showing the wide pdfs of short averaging and the narrow pdfs of the long averaging. Note that a convergence of the symmetry function is obtained for the limit in taking the long averaging times. Figures 1-4: Please add axis labels to both parts of the figures. Then the captions of Fig. 2-4 don’t need to repeat that of Fig.1, “as Fig.1 but for case XY” would be sufficient. Please give the unit of the averaging time as well.

Axises are now labeled. And we added in the legend of the first figure:

The variable τ gives the length of the averaging interval in terms of observations done every 6 hours.

p.6 l11you state that you will verify Eq. 3 in two steps so the reader expects these two to be addressed in order. It is however unclear to me which of the following two paragraphs is supposed to refer to which aspect (see further comments below).

We now added:

That is, we first have to confirm that the lines in the right panels of figs. **??**, **??**, **??** and **??** converge towards straight lines for increasing averaging periods and second we see if the lines superpose when increasing averaging periods.

p.6 l12 you claim that you "determine the slope" but that that slope is never actually shown or discussed directly. Why not fit lines to your curves and show us the estimated slopes (see comment below)? In that way we could also compare whether or not the slope differs between the regions which is hardly possible by comparing curves indifferent plots with different y-axes.

There are already many lines the figures and adding lines makes the figures difficult to see. Furthermore in exps. GSE and KUE the behavior clearly fails to be linear, so lines can not be included. I choose to define the index gamma to investigate linearity. We do not give the value of the slope as we do not have a theory for the slope and how it is related to the dynamics. This is the case in all references on Fluctuation theorems obtained from experiments with turbulent fluids we know of (see e.g. **?**). We have of course tried to find a relation but did not succeed. Please note that it was and is written in the paper: "The contraction rate $\sigma > 0$ see **?**, **?**, **?** and **?**) depends on the problem considered."

I our case it is influenced by the relation of the average wind to wind variability on different time-scales, the small scale turbulence in the boundary layer and the temperature stratification in the atmosphere.

We now added:

We did not manage to determine it from observed quantities.

p.6 l13 you again mention tau_0, can you at least give some rough estimate how long

that time-scale might be, relative to the length of your time series? Could this be inferred from the power-spectrum of the time-series?

The reviewer is right, an estimate of tau_0 should be given, but we do not have enough data to provide such a solid estimate. To consider FTs huge amount of data is necessary, which is often not available yet in environmental sciences. In the present work we base our investigation on previously published theoretical / numerical investigation which show the existence of a FT and we have enough data to start seeing FT like behavior. Please note also that tau_0 strongly depends on the tail of the pdf, the rare negative events. Results indicate that in the cases where we observe a FT the symmetry function converges to a strait line in about 1 year. The power-spectrum gives information about the amplitude of a given frequency, but the phase is equally important to determine the occurrence of the high amplitude events (in the same manner as phase is important to determine coherent structures in turbulence). So the connection between the power-spectrum and tau_0 is subtle.

I now added:

For the extension of the domains within the recirculation area of the subtropical gyre a convergence towards a linear variation with z is observed in less than $t_0 \approx 1$ year.

p.6 18f "This indicates the existence of a large deviation principle" isn't it more important that this convergence is predicted by the FT? What is the relationship between the existence of an LD principle and a FT? Also is this the first or the second part of the verification mentioned above?

The relation of FT and large deviation principal is often asked when I communicate about this work and I wanted to clarify the point here. If the LD exists for all z than the normalized symmetry function converges, but not necessarly to a straight line. So (ii) indicates the existence of a LD (but does not proof it), even if (i) does not hold. So the way it is said in the text is correct. I do not know how to say it correctly in a different and clearer way. If this sentence about LD leads to confusion it can be taken away.

The rest of the paper is completely independent of it. I would prefer to keep it. We now changed :

For the domains within the recirculation area (ASG and PSG) of the subtropical gyre a convergence towards a linear variation with z is observed in less than $t_0 \approx 1$ year. This points towards the existence of a FT, as both points put forward at the beginning of the previous paragraph are observed. For the extensions of the western boundary currents (GSE and KUE), the convergence does not achieve a linear behaviour of the normalised symmetry function. This shows that a FT does not hold, as the first point put forward at the beginning of the previous paragraph is not satisfied.

p.6 l19f "extension of the domains within ...", "extension of the western boundary current" please refer to the different regions by the acronyms you established before and also refer to the figures in which these results are shown.

Done

p.7 l1f I'm not sure why you chose to quantify the linearity of your curves by this specially designed index. If I understand correctly, the scaled symmetry functions corresponding to long averaging times should be linear across the whole range of z-values. Why not simply fit a line via least squares to calculate the overall slope? Use RËĘ2 to get an idea of the goodness of fit and plot the slopes against tau to observe the convergence behavior. I understand that the statistical interpretation in terms of confidence intervals is questionable but I don't see why your index is more appropriate. Unless I misunderstood your definition, there are many non-linear curves for which gamma=1.

Yes, there are non-linear curves for which gamma=1. One known scenario when the FT fails is due to boundary conditions as briefly mentioned in the text. In this case there is a transition in the slope from high to low values, as we observe in our data. Based on this analytically explained scenario I choose gamma the way I did, other choices are clearly possible.

p.8 l7 "extreme events are often key" of course extreme events in general are interesting but your framework doesn't describe just any kind of weather extreme but specifically unusually small (negative) values of atmosphere-ocean momentum transfer. Can you explain a bit more specifically why a rare event wherein the wind in the atmosphere is sped up by the ocean is of interest?

The reviewer is right. We now added:

Extreme negative events lead to strong transfer of energy to small-scale turbulence in the atmospheric and oceanic boundary layers, potentially causing strong mixing in the atmosphere and ocean.

p.8 l9f I like this example, perhaps it would be even more illustrative if you put in actual numbers for tau? Say one month or one year? This, however raises the question how large tau has to be for the FT to hold...

We now added:

The variable τ gives the length of the averaging interval in terms of observations done every 6 hours, that is $\tau = 400$ corresponds to a period of 100 days. A FT represents a tool to obtain the rare negative events from frequent positive events for all averaging times $\tau > \tau_0 \approx 1$ year

p.8 l12 "all averaging times" if I understand correctly, your FT only makes statements about long averaging times, right?

We now added:

$t > t_0 \approx 1$ year

p.9 l3 "exp2 & 4" please refer either to the figures or the abbreviations of the different, regions in a consistent manner, the terms "expN" were never explicitly introduced.

Oups, yes, now corrected.

p.9 l18 “guide the up and down-scaling” can you either give a reference for this claim or explain a little more how the FT could help with that?

We now added:

More precisely, when the pdf of the power supply, and therefore also the symmetry function is known form observations for given averaging times the symmetry function can be calculated for shorter and larger averaging times and therefore constrains “half” of the pdf. This is useful in down-scaling and the construction of statistical parameterizations of not directly observed dynamics over shorter time scales. On the other hand the information can be useful for developing models for the persistence of events over large time-scales not yet observed.

Technical corrections:

p.2 l14: case mismatch between “the importance [...] is, [...] their imprint”, please re-formulate

Done.

p.2 l17-18: the sentence with “can not be understood or modelled” is repeated verbatim, please cut or re-formulate.

Done.

p.2 l32: replace “i.e.” by “e.g.”

A negative event is when the ocean loses energy, so I would like to keep “i.e.” meaning: “that is”.

p.4 l7: replace “is” by “should be”

Done.

p.5 l6f “the production has been performed of ...” confusing sentence, do you mean “a near real-time data set, as well as a 24 year reanalysis, [...], have been produced” ?

Done.

p.5 l15 25 or 24 years ?

Now corrected. The ocean data is 25 years but the overlap with the atmospheric data is only 24 years.

p.5 l20 “6h in time and 1/4^o in space” this is repeated from the previous sentence.

Yes, but the first time it considers the atmospheric data and the second time it is the atmospheric and oceanic data. We put it to emphasize that both are available at the same resolution in time and space. We now write:

[. . .] at the same resolution in space and time.

p.5 l24 “, For” either change to lower case or start a new sentence

Done.

p.5 l30 “from” instead of “form”

Done.

p.7 l5 “these cases” or “this case”

Done.

p.8 l1 “is a currently a hot topic” cut one of the “a”s

Done.

p.8 l9 “slope” instead of “slops”

Done.

p.9 l5-6 replace “to which” by “in which”

Done.

p.9 l14 “growth” instead of “grows” or write “its surface grows quadratically”

Done.

Anonymous Referee #2

This paper aims to provide observational support in favour of the idea that the wind-power input satisfies a fluctuation theorem (FT) in some regions of the ocean. Fts have only appeared recently in the literature and have been useful to justify the physical character of (rare) violations of the second law of thermodynamics. In this paper, it is the wind power input that is treated as the dominantly positive quantity and the analogue of the positive entropy production predicted by the second law, while the negative power input events are seen as the analogue of the rare events seemingly violating the second law. Review of the literature on the subject is pedagogical enough that it can be read and understood with little background on the part of the reader. Overall, the paper is relatively clear and easy to follow, while the analysis appears to be competently done although short on practical details. The main weakness of the paper, however, is that it appears to devote much time explaining why FTs are useful or important in general, without ever really explaining why they are useful or important in the particular case considered by the paper, namely ocean energetics. The negative power input events are presented as ‘extreme’ events, but it is unclear to what extent this is justified. Are these events related to the passing by of low- pressure systems that result in occasional reversal of the winds relative to prevailing conditions? The authors emphasise that extreme events are often ‘key’ for the systems considered (by others), but do not explain why these are key for the system they consider. The paper needs to improve on those aspects as well as on the specific points outlined below before it can be accepted for publication.

Concerning the lack of concrete applications of FTs in air-sea interaction please see my answer to both reviewers in the beginning of this reply

General comments

Title: A more concise title would be: Empirical evidence of a fluctuation theorem for the wind mechanical power input in the ocean. I suggest using empirical because the estimation of the power input does not just involve satellite data. The authors need to explicitly state that the mechanical power input is due to the wind, as surface buoyancy fluxes also contributes to powering the ocean.

We agree and changed the title to:

Empirical evidence of a fluctuation theorem for the wind mechanical power input into the ocean

Aim: Could the authors clarify the precise aims of the paper? Is it intended to contribute to the literature about ocean energetics? If so, the authors should provide some review of the literature about ocean energetics. Is it intended to provide a constraint and metric by which to constrain ocean models? If so, the authors should expand on this some more and explain how one should go about it. Even better would be to repeat the calculations using model outputs where the authors find evidence for a FT to establish whether this would be a useful metric to assess models. As written, it is difficult to understand what issues of interest to the oceanographic community the present results are useful for.

Please see my answer to both reviewers in the beginning of this reply. Performing the same analysis on model data is planned, but this is another paper. Here we want to discuss the existence of FTs in observations. We added in the introduction:

For a general discussion on air-sea interaction we refer to **?**, for ocean energetics to **?** and for wind work to **?**.

More specific comments

1. Abstract, line 3: ‘global satellite observations’ may be more specific . Scatterometer wind observations and surface current derived altimeter data.

Yes, but then there is also drifter data and in-situ measurements. We are afraid being at the same time to specific and not specific enough in the abstract. We prefer writing that the basis are 'global satellite observations' and being more specific in the Data section and most importantly referring to the work were this rather involved products are described in all detail. Other choices are clearly possible.

2. Page 1, lines 15-17: The wind stress also includes a form stress component due to the wind blowing creating negative and positive pressure anomalies on the surface waves

By shear we mean the difference of the wind and the currents near the surface. In the present paper we are not concerned with the details of the air-sea interaction at small scales but suppose that these are parameterized by bulk formulas. That is why we write : [. . .] due to the difference between the atmospheric winds and the ocean currents near the surface in the corresponding planetary boundary layers.“ and not "at the surface". We now added:

In the present work we do not discuss the various physical processes occurring at the air-sea interface which are important for the momentum transfer.

We now replace "shear“ by "shear-stress" in the text.

3. Page 1, lines 20-21: The energy exchange is not conservative and most of the mechanical energy is dissipated. I don't understand what that means. Clearly, momentum is conserved and energy is transferred from the atmosphere to the ocean. Part of it goes into avaialbel potential energy to push down isopycnals or suck up isopycnals. Does it go into heat rapidly? Ultimately, sure. What are you trying to say here?

In air-sea interaction momentum is conserved but not energy (it resembles an inelastic collision of two objects, that stick together after collision). Most of the energy goes into 3D turbulence in the atmospheric and oceanic boundary layers with a direct energy cascade to dissipation into heat a large part goes into wave generation.

4. Page 2, line 5. 'measure' -> 'estimate' or 'evaluate'. The power input is clearly not measured.

Done.

5. Page 2, line 12: 'spacial' -> 'spatial'

Done. (The dictionary says that spacial is ok too)

6. Page 2, lines 16-17: and conversely, turbulent motion depend also on the mean. Does it matter for the arguments developed here?

It does, but here we want to emphasize the closure problem, that is the large scales we are usually interested in, in climate sciences, can not be modeled without some knowledge of the small scales. We now added:

, and vice versa.

7. Page 3, line 7: 'existence of a FT was shown empirically'. 'Shown' sounds like a strong word. Suggested sounds more accurate

Done.

8. Page 3, line 13. 'Satellite measurements' not onl. 'discuss their relevance' it is not clear to me that this has really been achieved satisfactorily. This needs to be improved.

See our answer to both referees in the beginning of this reply

9. Page 4, line 21: I find reference to 'shear' somewhat confusing, since power is best understood as the product of a force times displacement by unit time. Why not refer to the wind stress rather than the shear? Moreover, the wind stress is not just due to the shear, it also includes a form stress part due to the wind blow creating pressure positive and negative pressure anomalies on the upwind and downstream sides of sea surface waves.

Yes, the reviewer is right but by writing "wind stress" we are afraid that the reader thinks that we are using the approach where the force is calculated based on the wind only and not the difference between wind and current. This is detailed in section 3. We now replace “shear“ by “shear-stress” in the text.

10. Line 25. May be indicate the value of Cd used for the calculations.

We now added:

Variations of the drag coefficient are not considered and all the results are independent of a constant C_d.

11. Page 4, linear 29. ‘goestrophic’ - > ‘geostrophic’

Done.

12. Page 4-5, Lines 31-33. What does it mean physically? Is the power converted into available potential energy or is it dissipated into heat? How does this result justify estimating the wind power input proposed by the authors? Are the overall results sensitive to using the surface velocity or 15 m velocity? The calculations seem easy enough to do that the authors should describe both.

The wind injected at the surface goes into waves or is dissipated locally in the Ekman layer (see Zhai et al.), has no direct significance on the ocean dynamics. This why we did not consider it here.

13. Page 6, Lines 19-20: ‘This indicates the existence of a large deviation principle ’What does that mean? What does that imply? Why is this important or useful?

The relation of FT and large deviation principal is often asked when I communicate about this work and I wanted to clarify the point here. If the LD exists for all z than the normalized symmetry function converges, but not necessarily to a strait line. If this sentence about LD leads to confusion it can be taken away. The rest of the paper is completely independent of it. I would prefer to keep it.

14. Page 8. Lines 6-8. Why is this useful?

If a FT holds we have “half of the pdf” in the case of non-equlibrium stat. mechanics where we do not know the pdf this is the only information we have and it is useful. This is now discussed in more detail in the Conclusions (see answer to both reviewers above).

15. Page 8. Lines 7-8. ‘Extreme events are often key for the system [...]’ What does that mean? To what extent are negative wind power input ‘extreme’ and ‘key’ for the understanding of ocean energetics.

They are extreme because they are in the tails of the pdf. In this events, both, the atmosphere and the ocean loose energy, so large amounts of energy go into small-scale turbulence. We now write:

Extreme negative events lead to strong transfer of energy to small-scale turbulence in the atmospheric and oceanic boundary layers, potentially causing strong mixing in the atmosphere and ocean.

16. Page 9. Lines 14-26. These last three paragraphs are particularly vague and abstract and not really related to any issues pertaining to ocean energetics. Is it possible to link these to ocean energetics in some way? This paper does not contribute to the theory of FT, so it is unclear why it should speculate on it.

We consider if FT is applicable to air-sea interaction and find that is does in some cases. These last three paragraphs are key as they show how FTs can be useful and the last paragraph puts the work in a larger context, it does not speculate. So we would like to keep the paragraphs. We rewrote the last three paragraphs (see answer to both reviewers above).