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Interactive comment on "Seasonal and synoptic variability of diurnal currents in an upwelling system off northern Chile near 30° S" *by* Mónica Bello et al.

Anonymous Referee #1

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In their manuscript, Monica Bello and co-authors analysed the variability of diurnal currents off northern Chile.

The main problems I had with this work are presented first, followed by a list of other issues.

1. What are the new scientific findings? When reading the text, it seems as if almost every findings replicates or is very similar to previous research findings. You should clearly state in the abstract and in the conclusion the new findings of your study.

2. Source of data It is quite common to properly disclose the data sources. Who collected the data? How do I get access to the data? Given these are older data sets,

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you should summarize all research publications that made use of this particular data set and highlight what is new in your application.

3. Tides While this is about diurnal currents, it is quite remarkable to note that diurnal tides are not mentioned in the text until the last point of the summary. Tides truly need to be discussed as part of the data analysis.

4. IGW and CTWs Your study exclusively uses current and wind data. Despite this, you discuss potential influences by internal gravity waves and coastally trapped waves in the absence of any direct observational evidence of density effects. These parts are way too speculative in my opinion and should be removed from the text, unless they can be directly linked to your data.

5. Your Summary Here you should highlight only the new findings of your scientific investigation (and add recommendations for future research). However, your summary actually refers to some points that you didn't consider in your study at all, such as tides. This includes your statement that "the surface layer is deeper in spring". While you estimated the thickness of the surface Ekman layer, you haven't measured the true depth of the surface mixed layer from CTD data, have you?

6. What creates inertial oscillations? In the 4th point of your summary, you state that inertial oscillations are generated by a sudden decrease in wind intensity. This statement is incorrect. You need a sudden increase (not decrease) in wind intensity. For instance, a decrease in wind intensity has little impact on the dynamics of ocean processes that are geostrophically adjusted.

Here are the other issues in chronological order:

1. In the abstract and elsewhere you state that "the diurnal wind variability is modulated by the synoptic scale circulation". You probably refer to the synoptic wind patterns? Please add the term "atmospheric" before "circulation"

2. Line 66/67: There is a "(Ramos, pers. comm.)". Is this the same Ramos, who is also

co-author? It would be quite unusual to have a personal communication by a co-author included in a paper that is actually co-written by the same person.

3. Line 72/73: “Diurnal wind forcing (. . .) can equalize the diurnal and inertial frequencies”. I don’t understand this sentence. Please rewrite.

4. Line 95. “report” => change to “paper”

5. Line 146: Can you describe the method of “complex demodulation” in more detail.

6. In section 3.2 you refer to semidiurnal currents and you make several statement including the words “could be resonating” and “may be explained”. In my view these statement should be deleted or merged with similar statements made in the introduction.

7. Line 230: You compare the hodographs with “anticyclonic gyres”. This terminology is grossly incorrect.

8. Line 272: You state that “the maximum current intensities agreed with the maxima of the wind”. What do you mean? Average wind speed? Diurnal wind-speed maximum?

9. Line 281: Here you refer to satellite-tracked surface buoys without giving any references. Who did the deployments? More details on this should be given in the methodology section, not in the results section.

10. Line 284: What do you mean by “maximum winds”? Do you mean the strongest synoptic wind events? Or the strongest sea breezes?

11. Line 339: Estimate of the Ekman layer thickness (equation 1) should be included in the methodology section not here. Note that the true mixed layer depth can differ from the Ekman layer depth.

12. Line 447: Why a “4 h shift”? I am totally lost here.

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