Review of the Manuscript:

Title: Shifting momentum balance and frictional adjustment observed over the inner-shelf during a storm

written by M. Grifoll , A. Aretxabaleta , J. L. Pelegrí , and M. Espino

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General comments

Authors investigated the momentum balance in the Catalan (inner) shelf area of the NW Mediterranean Sea during the passage of storms. They explored this by, for the most part, analyzing current-meter observations at three locations in that shelf area. They offer an explanation about different responses of the shelf sea to the passage of storms. They show a very solid understanding of the balance of terms in the momentum equation in the along-shelf direction and one observes from the references that they are experienced in the circulation issues of that area linked to processes in the lower frequency domain with periods of a few days or longer. The paper reveals some possibly new findings and is also educative. The English is quite solid.

However, their explanation of different responses of the shelf relies on the incapacity of the bottom stress to dissipate high kinetic energy during the second storm, because there was a kinetic energy 'left-over' from the first storm. This is to be reflected on significant rapid oscillations with periods longer than 12 h of local acceleration and advective terms. In their abstract and in the manuscript they also point out that the response of the inner shelf (24 m depth) is 'prevailing frictional one'. One seems hardly to go together with the other: the 'prevailing frictional response’ that is obviously not sufficient to dissipate the ‘rapid’ oscillations of local acceleration and advection terms. If however, one statement goes easily together with the other, then this deserves special attention in Discussion.

What is most certainly missing in the paper is a conception of how the two storms are really similar which is certainly reflected in the balance of terms in the momentum. Authors did not offer a single statement about that. Storms should be observed from the point of their extension, their travelling speeds, horizontal gradients of winds and air-pressure during the storm. Authors did attempt to capture the barotropic pressure gradients through the differences in pressures between the A2 station and the remote tidal gauge 64 km away. However, the reader misses the ‘image’ of both storms that pass over the Catalan shelf. It is true that ‘global atmospheric models’ (e.g. ECMWF) may still present difficulties revealing storms in their ‘true’ time and space scales. Nonetheless, even so one would get an image about the ‘similarity’ of the two storms - especially from the point of view of horizontal gradients of wind (and pressure) which most certainly influence the horizontal gradients of currents, and therefore advection terms which are extracted from the space difference of Eulerian measurements of velocity. This is the first key point missing from the paper: an estimate of horizontal gradients within the storm, i.e. if the first storm was horizontally much more ‘homogeneous’, then horizontal gradients of currents (and their depth average) in forced motion would be weaker, advective terms would be smaller, and ‘the non-sufficient friction’ explanation would not be sufficient…

A second key point missing from an otherwise well written paper is linked to the possibility of generating different long coastal waves by the passage of a storm. It is true that during a storm the current field would follow the wind field (and the air-pressure field), meaning a forced system. To know the time and space extension of this, one needs to have a look at the synoptic meteorological evolution of the storm mentioned earlier. After the cessation of the first storm and before the second storm passes over the shelf area, there are also free long waves on the shelf – there is no word about them in the manuscript. Authors correctly inferred – without any notion of long waves – that if the second storm starts 'early enough' this means that the friction damping of motion raised by the first storm was not sufficient. However, it is the friction dissipation *of free waves* that matters here. Therefore during the second storm, if it arrives 'early enough' (say within 18 h after the first storm), there is a superposition of remaining long free waves from the first storm with the forcing motion of the second storm…

What authors explored is the (in)significance of wind-driven surface waves, which could also be remotely generated (swell), with a period around 8 s on the balance of terms in the equation of motion. Returning to the concept of 'missing long shelf waves', questions arise: Are they trapped (or arrested in an offshore direction) topographic barotropic waves on a sloping bottom, present during and after the storm passage? Is the oscillation of local acceleration and advection terms also linked to inertial, or just-above inertial frequency phenomena? During those storms was the radiation of internal waves from the surface (wind-mixed layer) to larger depths and horizontal distances also present, at least around the A2 station? This latter analysis requires knowledge of the stratification (before and after the storm), and authors did refer to it in a relatively modest sense by using CTD observations on 17th March 2011 for the estimate of thickness of the surface and bottom boundary layer. The spread of internal waves to depth may offer an explanation as to why at one place oscillations of acceleration of depth average currents are 'not visible' (or not pronounced) due to the baroclinic nature of motion (the first baroclinic two layer mode, visible on figure 2 c for the depth-time distribution of cross-shelf velocity), which after integration of currents along the vertical yield modest remaining depth averaged currents, which enter in a vertically averaged equation of motion. This baroclinic nature of inertial motion during the passage of the storm was studied on flat bottom areas by Gill (JPO 1984, vol. 14, 1129-1151) and by Kundu and Thomson (JPO 1985, vol. 15, 1076-1084).

Even if these issues cannot be resolved by pure analysis of currents at three locations, they deserve to be discussed and would make more sense in a context of the exploration of the balance of terms in the along-shelf (and across-shelf) direction. A plausible hypothesis of the existence of some long coastal waves with periods between 12 and 24 h generated during the storms, most likely topographic but also 'flat-bottom long waves', e.g. Kelvin and even Poincaré waves (angular frequency $\omega > f$, the Coriolis parameter), could matter and certainly deserves attention.

A third point is the careless introduction of the advective terms in the (long-shore) equation of motion (1). It is strange that authors did not pay any attention to the dynamics which can be ruled by the sloping bottom of the (inner) shelf (referring mostly to topographic waves). There are no horizontal gradients of the bottom depth in their analysis. This would be true if advective terms were not introduced. Authors derivation of terms in the Appendix is focused on the e-folding friction time scale (done correctly). The derivation of depth average of advective terms over a sloping bottom is not described. When these terms are introduced, however, some advective terms that apparently seem to be of the same order of local acceleration term, are missing in their key equation of motion (1). This might change their conclusions that the advective terms do not play a significant role in the(?) sense of transport, or depth averaged currents.

Therefore, this otherwise solid manuscript needs to be upgraded. It stands 'in between': it is far from being rejected, but, again, it needs to be upgraded.

Specific comments

1. Abstract, lines 10-11 and 15-16, statements like:

   '…apparently *reflecting the incapacity of the bottom stress to dissipate* the high kinetic energy of the system' and

   '…Estimates of the frictional time and Ekman depth confirm the *prevailing frictional response* at 24m.

are apparently somehow in disagreement: if the (bottom) friction is not sufficient to dissipate the kinetic energy, then one can hardly say that the response is prevailingly frictional. If both statements are to hold, then appropriate explanation should be given, but not in the abstract.

2. Page 900, section 'Site location and data'

Later in the text authors mention (an important) tidal gauge station at Blanes, a town which is written microscopically on figure 1. Since the pressure gradient term is also calculated with these sea-surface elevation data, this station certainly deserves attention – these are 'data' right? Authors calculated the pressure gradient terms, which would be impossible to do if those AWAC and RDI instruments were not also equipped with a pressure sensor. The 'noise level' of those sensors should be written here and not mentioned later in the text. Authors have also used CTD measurements on 17th March 2011 and there is no word about them in this section. If authors will improve the manuscript with the shape of the storm field (winds and pressures) then most certainly these synoptic data, together with their elaboration should be entered here.

The paper would be clearer if it would be better structured. This means that this section should also be enlarged with 'methods', meaning mostly methods of data elaboration (low pass filtering with a 12 h filter, time and space differentiation), which, according to authors, now deserves attention in the section of 'results', which makes this section longer and less readable.

Also the description of the numerical model of waves should be here. The horizontal resolution of the wave model should be written here (not on page 905, line 25 under 'Results'), together with the description of the input wind data, which is missing. Since authors mentioned the wave model, this certainly means that they have available wind (and air-pressure) model data over the observed shelf area.

3. Page 901, lines 19-20: 'The cross-shelf flow (Fig. 2c) was less intense than the along-shelf flow…' True, but not so much during the times of peak winds. It is hard to deduce values of currents from plots 2 b and 2 c because the color scale is different (-0.4 to 0.4 m/s, or -0.1 to 0.1 m/s), which should be unified.
4. Page 901, lines 24-27: 'The second wind peak (14 March 15:00UTC) was characterized by an intensification of the southeastward flow, while the cross-shelf flow was also enhanced. During the second peak (14 March), the onshore surface flow was compensated by a return flow near the bottom.' At this point one should be more precise: during the first half of the second storm, when the rise of wind speed (stress) is present, there was an intensification of the cross-shelf flow that was onshore at the surface and offshore near the bottom. An intensification of the southeastward flow over the whole water column (dark blue on figure 2b, seen only when a reader makes a 'huge zoom on figure 2) looks to be reached a little later, when the wind peak of the second storm occurred. It looks like the along-shelf current follows the (along-shelf) wind stress. After the peak of the second storm, however, when the wind stress decreases with time, there is apparently a reversed situation with regard to the cross-flow: the offshore current at the surface and the onshore one near the bottom. At least this is what this referee could see from those plots b and c on figure 2. The 'phase delay' of these (three) processes during the second storm might be important for the explanation of 'what was going on'.

5. Page 902, lines 8-9: ‘the depth-averaged current in the along-shelf direction (Fig. 2d) adjustment observed was toward the southwest with a maximum peak during 12 March 07:00UTC’. Here it seems appropriate to add that this happened two hours after the wind-stress peak of the first storm.
6. Page 902, lines 11-13: ‘The wave conditions measured at A3 were characterized by two significant wave height peaks (Fig. 3f) from the E–SE direction with a wave period of 8 s.’ It seems important to point out that the peaks of significant wave height followed the peaks of (along-shelf) wind stress with an even larger delay than that of the along-shelf currents, most likely meaning that swell waves play a larger role. Nonetheless, if one again makes a ‘huge zoom’ on the figure one sees that the radiation stress of wind waves has values on an order of magnitude lower than other terms in the equation of motion.
7. The introduction of the advective terms needs to be done correctly. The depth average of the two advection terms in the along-shelf momentum equation yields at least an additional term like $-\left(\overline{uv}\right)\partial H/\left(H\partial x\right)$ next to the existing term $\partial\left(\overline{uv}\right)/\partial x$ and a term like $-\left(\overline{v^2}\right)\partial H/\left(H\partial y\right)$ next to the existing term $\partial\left(\overline{v^2}\right)/\partial y$. Both additional terms, not to mention others, hardly seem ‘to vanish’ by the correct usage of the continuity equation. Terms with the gradient of the depth in the across-shelf sense are likely to have at least the same order of magnitude as those that have been explored. Authors would agree that we may reasonably suppose for the order of magnitude v = 0.1 m/s, H = 50 m, $\partial H/\partial y$ = 25/10^3, where the distance between A2 and A3 station is taken as 1 km. This gives $\left(\overline{v^2}\right)\partial H/\left(H\partial y\right)$ = 0.5 10^{-5} m/s^2 which is the order of the local acceleration term

and other important terms in the equation (1). Therefore, the analysis of the insignificance of the advection terms in the manuscript looks incomplete.

8. Page 903, top two paragraphs and the paragraph around line 25: Although authors made it clear that by using the cut-off filter of 12 h little energy remained in frequencies higher than inertial, it is exactly this frequency, and frequencies ‘slightly above it’, that matter for reasoning about what is going on the Catalan shelf during storms. They wrote: ‘…After the wind direction reversal, the acceleration term oscillates indicating a readjustment of the momentum balance (i.e. a relaxation period that lead to the pre-storm during storm conditions).’ It is true that the relaxation time (meaning friction mechanism) matters. Friction, however, cannot produce oscillations of acceleration before, during and after the second storm. Is there really not a period of 18.15 h seen between the first two minima of the along-shore local acceleration between 13 and 14 March 2011 (figure 3 a)? Could it be that the water mass with inertial oscillation passed over station A3? Between the two consecutive minima of the ‘advection’ terms this period also seems to prevail. There also seem to be oscillations with the time intervals between neighboring minima or maxima that have a higher frequency than inertial. This is hard to deduce from images by the reader because of the microscopic and unpleasant nature of the plots.

   Anyway, during the calm periods after storms one would expect the long free waves to move around the shelf. Despite the reasonable number of references put down by the authors, one is missing, i.e. the investigation into the possible ‘arrested topographic wave’ (Csanady, JPO, 1978), which follows from linear theory without any advection. This theory states that long waves with periods longer than the inertial one ($1/f$) are confined to a sloping shelf and are not radiated away. Their amplitude decreases exponentially, with an offshore decaying scale $L_x = [rL_y/(fs)]^{0.5}$ (Pettigrew and Murray, 1986. Baroclinic processes on Continental Shelves, Coastal and Estuarine Sciences 3, 95-108), where L_y is the along-shore length scale determined by the wind field, r is the coefficient of linear friction, which was thoroughly determined by authors, f is the Coriolis parameter and s is the bottom slope which hardly enters into the analysis in this manuscript, which seems to be deficient for processes on a sloping shelf. This expression (a similar one is written by Csanady in JPO 1978 in expression (21), where the along-shelf wave number is used instead of L_y) should be easily explored by measurements and is linked to the cross-shelf modulation of long waves. As Brink pointed out (Brink, 1998. the Sea, vol. 10, Wind-driven currents over the continental shelf, 3-20) on page 16 (6.1 Storm surges) storm surges may have frequencies that are higher than inertial. In this case there is an off-shore radiation of long waves that rules out coastal trapping.

Authors should also explore the cross-shore momentum balance, to see if the offshore acceleration (not examined) is present, or that it is not of much importantance and there are signs of geostrophic balance, which is typical for trapped shelf waves. They may also verify the apparent 'in-phase motion along-shelf of topographic coastal wave, if that one is trapped and its amplitude at A3 station is smaller than that at A2 station ($\omega < f$). None of these views is present in the manuscript.

9. Page 905, line 6: the value of $r = 8.5\times10^{-4}$ m/s seems to be linked to the first storm and the peak of PGFO during it. What about the PGFO peak during the second storm and consequently the 'representative' value of r for the second storm?
10. Page 907, lines 9 and 10: 'The peak in the acceleration term occurs before the wind maximum as a result of the enhanced frictional dissipation and the

increase of the pressure gradient force. Thus the along-shelf current is limited by the intensity of the bottom friction'. Authors did not pay attention to 'instrument clocks' in the section of 'data'. Since it is hard to extract the time delay from figure 3 between the wind peak and the acceleration peak (a few hours?) one needs to be certain that the timing of ADCPs matches the hours of the timing of the wind measurements (all in UTC) and that the clock error of the instruments is much smaller than this delay. Currents are not only limited by the bottom friction – they are actually limited by the wind-setup piling of the sea-surface.

11. Page 907, lines 27-28: 'These fluctuations (during the second peak) likely are a result of the increased energy available in the system, not properly dissipated by the bottom stress.' This is not a good argument. Friction may damp more or less oscillations, but the cause of them is still unclear and should be linked to some known form of motion on sloped shelve areas. Was the dissipation during and after the wind stress peak of the first storm not sufficient to damp the oscillation of the acceleration, while during the second storm it was sufficient? This seems strange.
12. Page 908, lines 7-10: Authors are not convincing in their explanation of the small advective terms during the first storm (first wind stress peak) while during the second peak the non-linearity of the flow results as a lack of dissipation of kinetic energy. One actually needs to justify why terms like $\partial\left(\overline{uv}\right)/\partial x + \partial\left(\overline{v^2}\right)/\partial y$ are much smaller during the first storm and larger during the second storm. If we confine ourselves only to these terms with horizontal gradients of products of depth, averaged velocity components most certainly depend on the extension of the atmospheric structure of both storms that force the coastal shelf sea and also travel over it at a certain speed. The latter matters

(with respect to the speed of long waves) if one would try be to explore the resonance forcing of long waves. Apart from time series of measurements at one meteorological station we do not have a clue about what these storms above the sea-surface looked like.

13. Page 909, top paragraph about the calculation of *r*: This explanation of authors is about the time for frictional adjustment, which in the case of linear friction calculates *r* iteratively from PGFR and from it the frictional time as H/r. The authors offered a method to calculate the frictional time from the time interval between zero and maximum bottom stress. However, this could also be reversed: by knowing the frictional time, one calculates the coefficient of linear friction $r = H/t_{14}$, where t_{14} = 14 hours for the adjustment time of the second storm. This point of view also means that the complicated iterative method given for *r* is a value that is wrong by almost a factor of two…
14. Page 909, lines 24-25: '…dependence of the flow on bottom dissipation at depths of the order of 24 m during a storm, precluding the appearance of inertial fluctuations independently of the coastal constraints'. Maybe it is so. Still, there are large oscillations of the along-shore local acceleration and there were statements that friction was not sufficient to damp these fluctuations.
15. Page 913, line 9: 'The storm had two separate peaks'. No, this concept is wrong. There were *two* consecutive storms and their horizontal gradients of winds and air-pressure could be quite different. Their travelling speed over the shelf might differ significantly as well. All this matters in the explanation of forced motion.

Technical corrections

Page 899, line 14: '…found the prevalent terms that the size of the momentum terms…' → '…found in the prevalent terms that the size of the momentum terms…'
Page 900, line 1: '(from hours to few days)' → ''(from hours to a few days)'?
Page 904, line 11: if the bin size of ADCP instruments is 1 m, then it is hardly possible that the first cell would measure currents at the height of 1 m above the sea bottom. There is a blanking distance, plus the height of the frame on which the ADCP is mounted.
Page 919, figure 1: why do these two figures have to be so small? This is really hard to look at. A zoom on figures shows that they have high-enough resolution. There is the name of the town written (Blanes) with letters that are a height of 1 mm? This name is written between the Balearic Islands and the Catalan coastline, it does not give an idea where Blanes is. All letters, including the names of stations, are simply too small and really unpleasant to a reader (and to this referee).
Pages 920 and 921, figures 2 and 3: while all plots on the figures are again too small and unpleasant, one could see with a huge zoom that while on figure 2 the time scale is labeled in 'units' yy/mm/dd (=year/month/day), the time scale on figure 3 is labeled in 'units' dd/mm/yy (=day/month/year), which is confusing. While figure 2 has the full day of 11th March on the time scale, figure 3 does not. Labels of plots, like **a**, **b**, **c**,… are missing on both figures. These labels are otherwise written in figure captions of both figures, but they are missing on plots. Figure 2 should have the same color bar scale on plots b and c to have a better feeling about how the cross-shelf velocity is smaller than the along-shelf velocity.
Page 922, figure 4: too small, letters are too small to read.

Review criteria

Principal criteria	Excellent (1)	Good (2)	Fair (3)	Poor (4)
Scientific significance: Does the manuscript represent a substantial contribution to scientific progress within the scope of Ocean Science (substantial new concepts, ideas, methods, or data)?			X	
Scientific quality: Are the scientific approach and applied methods valid? Are the results discussed in an appropriate and balanced way (consideration of related work, including appropriate references)?			X	
Presentation quality: Are the scientific results and conclusions presented in a clear, concise, and well-structured way (number and quality of figures/tables, appropriate use of English language)?		X		