

Response to reviewer

We thank the reviewer for a second careful assessment of our manuscript. The comments were again helpful in clarifying the presentation. We have revised the manuscript accordingly and provide point-by-point responses to the comments below.

In addition to the comments reproduced here, the reviewer also provided a number of annotations in the manuscript PDF. These were considered as well, and the corresponding corrections and clarifications have been incorporated into the revised manuscript.

Comment:

The text says or implies a few times that the approximation that $|h| \ll H_0$ isn't a good one, but . . . I may well be mistaken but . . . if H_0 is a function of y , it's a good approximation. I followed the derivation of (20)–(24) and it seems to me that everything works even if H_0 is a function of y . . . because $\overline{h}$ is an average along a constant- y line.

Response:

If we allow H_0 to vary with y , the depth can indeed be decomposed as $H(x,y)=H_0(y)+h(x,y)$, where $H_0(y)$ is the mean depth along a given y -coordinate. However, within the formulation we use, the approximation $|h| \ll H_0$ is still only moderately well satisfied. For the baseline bathymetry, $\max |h|$ is about 300 m at mid-slope, while H_0 is about 500 m there. The topographic variations are smaller away from the mid-slope. We have revised the text to provide representative values for h when introducing the QG PV-flux interpretation of the simulations (line 477).

Comment:

"QG PV" (again): Last time I said I didn't quite know whether quasi-geostrophy is necessary or not or what QG PV is for the barotropic system.

This time I understand that quasi-geostrophy per se is necessary: We use the relation $v \approx \phi_x$. But, the authors cite Chapter 6 of Salmon (1998), where he starts from the so-called quasi-geostrophic PV.

But, I think the term "QG PV" may be misleading, or, if not misleading, unnecessary, at the very least.

The "original" PV for the shallow-water system is

$$PV = (f + \zeta) / (H + \eta) \dots (A)$$

Naming aside, we need to approximate this to

$$PV \approx (f + \zeta h/H_0) / H_0, \dots (B)$$

where $H = H_0 + h$ and $|h| \ll H_0$ and $|\eta| \ll H_0$, for us to arrive at equation (24).

So, we need

- quasi-geostrophy: $v \approx \phi_y$
- small-amplitude undulation: $|h| \ll H_0$

to arrive at equation (24). So, instead of starting from "QG PV", we can just state the small-amplitude approximation and derive the approximate form (B) above.

So, the question is: Is the use of the term "QG PV" for the approximate barotropic PV really justifiable? How is the small topographic undulation related to geostrophy?

For baro_clinic_ QG systems, if I understand the argument correctly, a small interfacial displacement is a "result" of the geostrophic assumption. For geostrophy to hold, the nonlinear terms in the momentum equations must be small and for that to happen, the isopycnal displacements have to be small in a baroclinic system. For this reason, small displacement is closely tied to geostrophy and for this reason, the approximate $PV \approx (f - \zeta h/H_0) / H_0$ is called "QG PV" for a baroclinic system, where h is the interfacial (thermocline) displacement this time and H_0 is the mean thickness of the thermocline.

Response:

We agree with the reviewer here that this derivation could be clarified. We have now removed the initial reference to the definition of barotropic QGPV and instead started with the derivations, using QG scaling where needed. We then end by pointing out that the resulting expression takes the form of a QGPV flux. As a detail, note that QG requires *both* a small Rossby number (geostrophic velocities at lowest order) and small-amplitude topographic variations.

Comment:

The present paper focuses on the arrestation of Rossby waves (resonance) and the resultant strong nonlinear prograde forcing. But, the gentler, weakly-nonlinear prograde forcing in the 16-day-forcing experiment (Figure 4b) isn't clearly explained. I had to collect scattered pieces of information from the text to synthesize what is happening.

The paragraph of lines 700–703

Nonlinear flow–topography interactions introduce a systematic prograde bias in the circulation response, in the sense that the response is shifted towards prograde values relative to linear estimates. As the circulation strengthens, the retrograde flow reaches a critical velocity beyond which its strength saturates, while prograde flow can continue to increase, leading to a pronounced asymmetry between prograde and retrograde circulation.

suggests that the same basic mechanism is still responsible for the resonant case: Resonance just drastically enhances the same prograde nonlinear forcing. ← This explanation is lacking (or unclear) in the text.

If I'm not mistaken, the discussion on lines 476–483 is the explanation of the basic mechanism, but its connection to the smooth prograde forcing isn't clear. I think we should set up a subsection or something that explicitly explain the mechanism.

Regarding that, I guess this mechanism works only when friction is present because it relies on PV mixing, but I'm not sure. Is the prograde forcing the same without friction? Is the non-acceleration theorem applicable to the present situation?

The paragraph of lines 678–686 seems to be the answer to this question. It would be nice if all these pieces of information are put together in a coherent narrative to explain the whole story of the prograde forcing.

Response:

The reviewer is right that the same basic mechanism underlies the nonlinear deviations in both forcing regimes. We now state this more explicitly in the conclusion, where we clarify that asymmetric topographic form stress is responsible for the nonlinear deviations regardless of forcing period, but that the asymmetry is strongly enhanced when Rossby waves with the same wavelength as the bathymetry are arrested. We also now state explicitly that this mechanism relies on irreversibility. These changes appear in lines 734–738. In addition, we now emphasize earlier in the manuscript that the same mechanism operates in both the short- and long-period cases, both in the signposting around line 358 and in the discussion of the isobath analysis at lines 461–463.

Comment:

Linear part of the PV flux.

Line 720 has "this linear contribution to the PV flux". Looking at equations (20)–(23), I see that the $f h/H_0$ part of the linearized PV is dominant. Or is it?

This point has never been clear to me. For the most part, the text keeps saying "nonlinear" mechanism for the generation of the prograde tendency. But, is the thickness flux the essential mechanism and is the response essentially linear?

This discussion points to another question which I don't know the answer to. Figure A1 presents a solutions with extremely weak forcing without bottom corrugation. Is that correct? What happens if the standard corrugation is included and the ocean is driven by extremely weak winds? If the mechanism that generates the prograde tendency is linear, there should be the same prograde tendency.

If the underlying mechanism is essentially linear, it's good news. The wind stress includes various frequencies, but if the ocean's response is linear, each frequency band gets individual prograde tendency and the total response is just a superposition of the frequency bands.

The text on lines 729–731 suggests potential future studies. If nonlinearity is essential, then the total response to realistic winds might be another interesting subject of a future study.

Response:

We thank the reviewer for bringing up this important point.

First, regarding the bathymetry used in the simulations shown in Fig. A1: these simulations use the baseline bathymetry, including the corrugations. The text at line 249 and the figure caption have been revised to state explicitly that this experiment is a weak-forcing variation of the baseline case, with the forcing reduced to 10%.

As the forcing is reduced, we expect the nonlinear contribution to tend toward zero, although not to vanish exactly. If one looks closely at Fig. A1, there is in fact a slight offset between the 1:1 line and the data. Figure 7, by contrast, shows simulations without corrugations; in that case, the domain-integrated balance is purely linear.

Regarding the underlying mechanism, nonlinearity is key to the asymmetric response, even though this is not immediately obvious from the PV equation alone and even though the expression for topographic form stress appears linear. In particular, advection of relative vorticity modifies the pressure field so that retrograde forcing produces both larger pressure amplitudes and a larger phase offset between pressure and topography. These in turn lead to a stronger topographic form stress during the retrograde phase. We now state this explicitly in lines 283–287 and refer back to it in lines 496–497. Because the underlying

mechanism is nonlinear, the ocean response cannot be regarded as a simple superposition of independently forced states. As the reviewer suggests, the response to realistic winds is indeed an interesting topic for future study, and we now mention this in lines 679-681.

Comment:

The small-scale, fast oscillation in Figures 5a and 5c needs to be discussed, even if its cause is not known. Is this what Figure A2 shows or is it noisier than the main feature Figure A2 shows? (Unfortunately, the animations aren't available to me. I somehow get an "internal server error" when I tried to view the animations.) When I first saw Figures 5a and 5c, I expected that this oscillation was some physically meaningful phenomenon and would be explained later.

Response:

We agree that the high-frequency oscillations in Figs. 5a and 5c should be discussed explicitly. In the revised manuscript, we now note that the rapid oscillations during the retrograde phase coincide with the emergence of transient eddies seen in the supplementary animations and are therefore consistent with eddying behavior. We also note that weaker, small-amplitude high-frequency oscillations are present during strong prograde flow, likely reflecting barotropic instability. The relevant discussion has been added in lines 425-427 and 431-433.

Comment:

Caption to Suppl. animations? I didn't find a caption to the two Suppl. animations. This set of animations needs a standard caption like a standard figure caption or at the very least, a caption that explains the relation of the animations to Figure A2.

Response:

Thank you for pointing out this. We have added a caption to the animations under *Video supplement*.

Comment:

Boldface for vectors: Please make sure that boldface is sufficiently bold in the final version. I was a bit surprised to see "i", which I momentarily mistook for the imaginary unit, in (26),

before I realized that it's the unit vector in the x direction. In this manuscript all boldface letters are a bit too thin to be distinct. It sometimes happens that the standard bold-italic font is too thin and the standard bold-upright font is more distinct. If so, I recommend the latter font for vectors.

Response:

We thank the reviewer for pointing this out.

When typesetting, we have followed the typesetting regulations given in the template from Copernicus Publications, which state that vectors are identified in bold italic font using $\vec{x}$. We therefore keep this notation for consistency with the journal's formatting requirements.

Comment:

Dependency on R ? Initially I was a bit concerned about the lack of explicit discussion on the dependency of the solution on R .

But, I realized that the text on lines 198–202 implicitly says that changing the bottom friction coefficient is equivalent to changing the forcing period. Formally, the time variable t of the basic barotropic shallow-water equations can be scaled and nondimensionalized by (H_0/R) with some mean depth H_0 . So, the dependency of the solution to ω can be converted to the dependency on R through the scaling of the time axis, and so an explicit dependency test on R is not necessary . . . So, perhaps the text on lines 198–202 may be enhanced to more clearly put forward this point.

Response:

This is a helpful point. We now state this scaling argument more explicitly in lines 205-207.

Comment:

I recommend the term "isobath" whenever it makes sense. The authors keep saying "depth contour" but "isobath" is shorter. Also, they sometimes say just "contour" as in caption to Figure 4. In such a case, "isobath" is always unambiguous.

Response:

We have replaced “depth contour” with “isobath” throughout the manuscript.

Comment:

*Lines 65–69 "Fluid columns that are stirred back and forth over uneven topography . . ."—
This is nice explanation of how net horizontal mixing of PV can be accomplished by bottom
friction. So, I recommend explicitly mentioning PV mixing, because usually the Neptune
effect is explained with horizontal PV mixing without going into details of how that mixing is
accomplished.*

Response:

We now mention PV mixing explicitly on line 70.