Second review of "The characteristics of tides and their effects on the general circulation of the Mediterranean Sea" by McDonagh et al.

Context

The manuscript "The characteristics of tides and their effects on the general circulation of the Mediterranean Sea" by McDonagh et al. presents a study investigating the effect of tides on the Mediterranean Sea circulation. To my knowledge, this subject has not yet been extensively investigated. Apart from Sannino et al. 2014, which provided a first analysis of the tidal influence on the large-scale Mediterranean circulation, previous studies have only focused on specific areas such as the Alboran Sea (Sanchez-Garrido et al., 2013) or the Sicily Strait (Oddo et al., 2023; Gasparini et al., 2004). As such, the present manuscript proposes a valuable contribution to reinforce and deepen the current knowledge on the tidal influence on the Mediterranean Sea.

In this study, the authors diagnose the effect of tides from a pair of 5-year-long tidal and non-tidal experiments based on very similar numerical configurations. The first and second sections of the manuscript investigate the influence of tides on the Mediterranean Sea dynamics through the prism of sea level and kinetic energy spectra. The first section first investigates the whole Mediterranean Sea and then focuses on the Adriatic Sea and the Gulf of Gabes. The second section focuses on three specific locations: the Strait of Gibraltar, the Tyrrhenian Sea, and the Cretan Sea. They reveal that tides impact high-frequency (> 1 day^{-1}) dynamics through the propagation of the main tidal harmonics at work within the Mediterranean Basin and their interaction with various basin-scale modes. Then, the authors relate the tidally enhanced dynamics and mixing to the deepening of the mixed layer depth in the tidal simulation. The final sections discuss the impact of tidal transformation of Atlantic water masses at the Strait of Gibraltar on the thermohaline properties of the Mediterranean Sea.

Second review: summary

For the first round of review, I requested additional materials from the authors to strengthen the highlighted results. I am pleased to acknowledge the authors' efforts in modifying the manuscript. Their work has enriched the study with valuable information. However, these additional materials also raise critical questions about the main results, making them more fragile. To be specific :

In section 3, the frequencies mentioned in the text are now presented in Figures 2, 3, and 4, improving the clarity of the analysis. However, this also brings to light several discrepancies between the results highlighted in the text and the content of the figures. There are also inconsistencies between the investigated simulations and the literature, which is nevertheless used to interpret the physical meaning of the results.

In section 5, the two new panels of Figure 10 show that mixed layer differences between the simulations mainly result from mixed layer deepening in winter. In the literature, tidal deepening of winter mixed layer, associated with deep and intermediate convection, has been discussed in several studies (PhD of Gonzalez et al., 2023; Sannino et al., 2015; Naranjo et al., 2014), in particular over the Gulf of Lion. So far, it has always been related to the densification of Atlantic water masses by tidal mixing at the Strait of Gibraltar. This remote effect of tides is out of the scope of the present study, which investigates the influence of tidal dynamics in the Mediterranean Sea. Thus, to focus on mixed layer depth variations at the Gulf of Lion, the authors must show explicit evidence that the observed signal is at least partly related to local tidal dynamics. In the current version of the manuscript, the authors only specify that mixed layer changes over the Gulf of Lion may not be related to local tidal mixing.

Besides the above points, I still have issues with section 4, in which the authors chose not to include additional diagnostics that reviewer #2 and myself recommended. In the current version of the manuscript, the limitation of the horizontal kinetic energy analysis to three discrete points (in the Strait of Gibraltar, the Cretan Sea, and the Tyrrhenian Sea) makes the results hardly generalizable to the rest of the Mediterranean. The vertical currents analysis is even more problematic to me as it only relies on a one-month temporal window to investigate the impact of tidal dynamics on the sea vertical currents. On such a short temporal window, and focusing only on three discrete points in space, it seems impossible for me to properly disentangle the influence of tide from the internal variability of the ocean circulation, which is not mentioned in the analysis.

For these reasons, I cannot recommend the manuscript for publication in its actual form. The manuscript's body should be comprehensively revised to address the points above. Because the associated modifications will require new sets of diagnostics for sections 4 and 5 and an in-depth modification of section 3, I feel it is better to start a new review process with an updated manuscript. Therefore, I am arguing for a rejection of the current manuscript. I am very sorry for the inconvenience that goes along. Below is a detailed list of the issues I have noted and some recommendations. I sincerely hope the authors can find the time to make the necessary modifications to publish this valuable study.

Detailed comments

General:

The authors have chosen to keep section 6 in the body of the manuscript, arguing: «This section expands on the work of, e.g., Naranjo et al., 2014, which confirmed the higher salinity and lower temperature of the inflowing water from the Gibraltar Strait. Further to this, we find that the changes in the salinity and temperature are consistent in the upper layer of the entire Mediterranean Sea. ». I respect their perspective, but I hold a different view on this matter. In my opinion, section 6 should not be included in the body of the manuscript because :

1. This study is meant to investigate the influence of tides on the Mediterranean Sea dynamics and the associated impact on its hydrography. However, this section only discusses the effect of Atlantic water mass transformation at the Strait of Gibraltar, which is unrelated to local tidal modulations of the Mediterranean circulation.

2. The consistency of the temperature and salinity changes over the Mediterranean Sea has already been investigated by Sannino et al., 2015 and Harzalla et al. 2014.

However, I think this section provides valuable information in a « model validation » section or as supplementary material.

Introduction

General remarks:
The clarity of the introduction has much improved. I thank the authors for their time in making these modifications. Below are some minor remarks, mainly to improve the references.

Detailed remarks:

- L. 20: "The presence of internal tides generated in the Gibraltar Strait is discussed in Morozov et al. (2002) and Vlasenko et al. (2009)." => You should add the recent paper of Roustan et al., 2024, and Hilt et al., 2020.
- L.25: "For the Mediterranean Sea, several authors have depicted the importance of tidal motion in the Gibraltar Strait (Armi and Farmer, 1985; Candela et al., 1990; Harzallah et al., 2014)." => You should also cite Sannino et al., 2015 and Naranjo et al., 2014.
- L35: "Moreover, 35 Ambar and Howe (1979) found that tides increase the variability of outfowing salinity." => This reference is pretty old. It would be best to back it up with a more recent study.
- L55: "To our knowledge, the effects of tides on the circulation of the entire Mediterranean Sea has not been extensively investigated." => Sannino et al., 2014 did investigate this (see section 4).

Data and methods

General remarks:
Thank you for taking the time to modify this section. I have nothing more to say about it.

Sea level energy spectra

General remarks:
I thank the authors for taking the time to include my suggestions. Because of the issues mentioned above (more details below), I think this section should be entirely rewritten to provide a more consistent physical interpretation of the frequencies enhanced by tides. This may also be an opportunity to give a critical view of the literature, in which several aspects investigated in this study have not been updated in a long time.

Detailed remarks :

<u>L140 – 145: Broad 12h energy peak</u>
"We argue that the broad 12h energy peak in Figure 2 in the tidal run is composed of the amplified 11.4h Mediterranean Sea basin mode energy (Schwab and Rao, 1983), the first Adriatic mode at 10.7h, known to be enhanced by tides (Medvedev et al., 2020; Schwab and Rao, 1983), and the 12h Adriatic/Aegean seas mode (Lozano and Candela, 1995)." => Now that the 11.4h and 10.7h frequencies are explicitly shown in Figure 2, we can see that they are on the upper end of the broad 12h peak and, thus, can only explain the upper part of the peak. In addition, the mentioned frequencies are not specifically enhanced in the non-tidal simulation, and no significant peak is visible in the tidal simulation. In my opinion:

- You must revise the 12h broad peak physical interpretation, as it does not appear related to the mentioned frequencies.
- The 11.4h and 10.7h energy modes should not be interpreted as tidally enhanced, as no clear signal is visible in the tidal and non-tidal experiments. Note that no signal is visible in the Adriatic Sea for the 10.7h Adriatic mode (Figure 3. a). In this regard, explaining why the simulations do not represent these basin mode frequencies would be valuable.

<u>L145: 8h energy peak</u>
"The peak at 8h also aligns with the Western Mediterranean basin mode of 8.4h discussed in Schwab and Rao (1983), and the Gulf of Gabes mode at 8.2h (Lozano and Candela, 1995). Schwab and Rao (1983) also noted a fourth Mediterranean mode at 7.4h and the third Adriatic mode at 6.7h."=> The 8.4h and 8.2h are distinct from the 8h peak, so they should not be used to explain it. Also, in Figure 3b, the 8.2h frequency does not seem specifically intensified over the Gulf of Gabes, so its physical interpretation should be revised (see following comments). The 7.4h and 6.7h peaks are not intensified; if they are mentioned, you should explain why they are not present in the simulations.

<u>L150: Adriatic Sea</u>
"In the Adriatic Sea (Fig. 3a), the sea level energy peaks at the frequencies of the barotropic modes of the Adriatic Sea at 11.4 hours, 6.7 hours (Schwab and Rao, 1983) and 12 hours (Lozano and Candela, 1995) are all enhanced by tides. The peaks are also visible in the model without tides, but with lower energy, thus confirming that these peaks are due to amplification of existing modes forced by winds and atmospheric pressure." => The 6.7h peak is not particularly high in the non-tidal simulation and is not intensified in the tidal simulation. You should display the 11.4h frequency in Figure 3. a. In addition, it does not seem to be significantly intensified, neither in the non-tidal simulation nor in the tidal one.

L150: Gulf of Gabes

"The barotropic oscillation at 8.2h is in the Gulf of Gabes, according to Lozano and Candela (1995), but the third mode of the Mediterranean Sea (Schwab and Rao, 1983), and the nonlinear effects of tides in the central Mediterranean Sea according to Palma et al. (2020) are at frequencies close to this (8.4h and 8.0h respectively), and may also have an impact on Figure 4c. Since many of the calculations of barotropic oscillations in the Mediterranean Sea were made several decades ago, there is a need for an updated confirmation of the frequencies of barotropic oscillations using state-of-the-art methods." => Thanks for this paragraph, which clarifies the discrepancies between the literature and the manuscript. Since you highlight these discrepancies, the 8.2h frequency should not be associated only with the Gulf Gabes in these simulations.

Kinetic energy spectra

General remarks:

For the reasons mentioned above, I suggest entirely rewriting this section. The updated section could first investigate the kinetic energy at the Mediterranean scale (as in the first section) and then focus on specific frequency bands impacted by tides, providing 2D maps of their influence. Regarding the vertical velocities, I understand that 3D hourly outputs are complex to handle, but one month of data is insufficient to conclude on the effects of tides.

Mixed layer depth and water mass formation

General remarks:

I am sorry that investigating the mixed layer depth seasonal cycle did not make the separation of local and remote effects of tides easier. I think this section is interesting, but further work is needed, as it is essential to properly isolate the local influence of tidal dynamics on the mixed layer depth. In this regard, another simulation, which "sees" the same Atlantic water masses as the non-tidal one at the entrance of the Mediterranean, but still includes Mediterranean tides, could be used. Diagnosing the impact of the modulated stratification due to the Atlantic water masses transformation at the Strait of Gibraltar on the mixed layer depth variation to deduce the local effect of tides as a residual could be another solution (see Sannino et al., 2009, section 4.3).

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