Response to Referee #1

Surface Kinetic Energy Distributions in the North and Equatorial Atlantic
Derived from Surface Drifter Observations and High-Resolution
Numerical Models with Tidal Forcing
now:
Surface Horizontal Kinetic Energy Sensitivity to Numerical Parameters
in Tidal-Resolving North and Equatorial Atlantic Simulations
Laxenaire et al., egusphere-2025-6355

This paper presents recent findings regarding surface horizontal kinetic energy (SHKE) levels in a series of high-resolution, realistic numerical simulations of the North and Equatorial Atlantic Oceans. These results are then compared with surface drifter data. This comparison uses simulated Lagrangian time series in the numerical simulations to provide an accurate comparison, as described by Zhang et al. (2024). The paper then explores the impact of various numerical parameters, including resolution, the presence of drag for internal waves, bathymetry resolution, and wind forcing frequency. The paper is clearly written and logically organised, providing results of interest to the ocean modelling community. However, I have a few remarks regarding the objectives of the paper – which I think could be clarified – and possible redundancies with previously published results, considering that the present paper is somewhat long.

We thank Referee #1 for the careful reading and the constructive comments, which have led us to sharpen the main message of the paper, to shorten several passages that overlapped with earlier work, and to strengthen the mechanistic discussion in Section 5. The reviewer's comments are reproduced in bold below and associated responses follow in normal text.

Indeed, the main message of the paper is unclear to me. From the title, it sounds like it is about characterising the surface kinetics, which is actually discussed in Section 3. However, this has essentially been reported previously using very similar datasets and diagnostics in the referenced papers (Yu et al., 2019; Arbic et al., 2022; Zhang et al., 2024). Similarly, section 4.3 provides a lengthy comparison of Lagrangian estimates and Lagrangian vs. Eulerian spectra of SHKE, which seems redundant with previously published results. Section 5 then provides a discussion of the different numerical parameters, which corresponds best with the description provided in the abstract. This is probably the most interesting and novel part of the study. However, it is difficult to link these two sections (3 and 4 on the one hand, and 5 on the other). This is exemplified by the introductory paragraph at the beginning of Section 5, the logical path of which I could not understand (previous results -> motivations for next analysis). Therefore, I suggest that the authors provide clarifications on these points and perhaps take the opportunity to shorten the paper by using appendices for the detailed diagnostics that are not entirely novel, while retaining only the key messages relevant to the paper.

We agree that the backbone of the paper needed to be made more explicit, and we have restructured the manuscript around two main contributions. The two contributions are: (i) a controlled, single-parameter-at-a-time sensitivity analysis of the SHKE distribution in a suite of tide-resolving, eddy-resolving to submesoscale-permitting HYCOM simulations (Section 5), which is the main novelty of the study, and (ii) the use of numerical drifters to assess the Lagrangian distortion in our domain to allow to use surface drifters as reference (Section 4). Section 3 is the common baseline on which both contributions rest and Section 4 provides the observation-to-model bridge between Lagrangian drifter statistics and Eulerian model fields that makes the Eulerian sensitivity analysis of Section 5 interpretable against the drifter reference. In other words, Sections 3 and 4 are not parallel descriptions of already published results but the stepping stones required to read Section 5 in a drifter-referenced sense.

We have decided against moving Sections 3 and 4 to appendices because the by-band values introduced in Section 3 (Table 2) and the Lagrangian distortion quantified in Section 4.2 are cited paragraph by paragraph throughout Section 5 and in the Discussion of Section 6, and splitting them into appendices would force the reader back and forth between the main text and the appendices on every page. We have, however, moved the absolute SHKE values (previously Table 2 in the main text) to the supplementary material (Table S1), in line with the remark of Reviewer #2.

We acknowledge that the reviewer's overarching concern about the clarity and the length of the manuscript was shared by Reviewer #2. We have therefore undertaken a substantial restructuring effort, presenting our results more clearly and discussing them in greater depth, and we hope that the revised version now adequately addresses these concerns.

Finally, I think the authors could provide explanations for their results in numerous places to go beyond a mere description of their diagnostics. For instance, around lines 285–290, why is there such a bias? Likewise, I could not get the added value of the discussion in section 5.1, which discusses the differences between including only the M2 tidal component vs. all tidal components: what are the outcomes of this experiment, either in terms of model behaviour or ocean dynamics? At the end of section 5.2, why do you think a higher resolution reduces the diurnal HKE in nearshore domains? At line 493, why does semidiurnal energy increase in a closed sea with a higher wind frequency?

We agree with this general comment and have revised all subsections in Section 5 to expand the mechanistic interpretation. A paragraph-by-paragraph response follows.

For l. 285–290 of the initial submission, the bias discussed is the frequency-independent increase of SHKE in OP Seed 1/2° relative to OP Seed Drifters, which is visible outside of the $\sim 30°$N band. This offset most likely reflects the fact that the two seeding strategies sample different spatial populations: OP Seed 1/2° weights the whole domain uniformly, whereas OP Seed Drifters is weighted toward the regions where undrogued drifters accumulate, i.e., the North Atlantic subtropical gyre. Around 30°N, the two sampling densities become equivalent because the undrogued drifter density is maximal there, and the offset vanishes. Following the suggestion of Reviewer #2, this OP Seed 1/2° versus OP Seed Drifters comparison has been moved to Supplementary Section S4 and Figure S2, and the take-home message, that the choice of seeding strategy has a small and spatially incoherent impact on the SHKE, is the one kept in the main text. OP Seed Drifters is now the Lagrangian reference used throughout Section 4.

For Section 5.1 (M_2-only versus eight constituents), we have added a closing paragraph that explicitly decomposes the diurnal peak into its tidal and non-tidal (stated as important by Arbic (2022)) contributions. The NEATL12-M_2 run is essentially identical to NEATL12-T offshore and poleward of the diurnal critical latitude ($\sim 30°$N), which demonstrates that the diurnal energy of NEATL12-T in this part of the domain is not tidally forced. On the shelves and equatorward of 30°N, by contrast, expanding the forcing to the eight largest constituents raises the diurnal SHKE by a factor of up to three, which demonstrates that the diurnal signal there is predominantly tidal. This experiment helps us to interpret the bipolar diurnal mismatch with the drifters in the Discussion of Section 6: the equatorward excess is of tidal origin and can be addressed by a wave drag, whereas the poleward deficit is non-tidal and requires, for example, a different wind forcing.

For the end of Section 5.2 (nearshore diurnal reduction at higher horizontal resolution), we have added a mechanistic paragraph. Our interpretation is that at 1/50° the shelf-break topography is sufficiently resolved for barotropic-to-baroclinic conversion of the diurnal tide to become more effective, so that a fraction of the shelf barotropic diurnal SHKE is converted into internal tides and radiated seaward, where it contributes to the offshore diurnal band rather than being retained on the shelf. This is consistent with the predominantly barotropic nature of the shelf tidal signal and with the seaward radiation pathway documented, for example, by Niwa and Hibiya (2011), Buijsman et al. (2020) and Xu et al. (2022).

For the semidiurnal amplification in semi-enclosed basins with hourly wind forcing, we have added a short mechanistic paragraph to Section 5.6. The wind itself carries diurnal and semidiurnal cycles that hourly forcing resolves but 6-hourly forcing aliases (Dai and Deser, 1999; Savazzi et al., 2022; Dai, 2023). The semidiurnal share of the wind variance is small (15–25% over the ocean according to Dai and Deser (1999)), so the wind-forced semidiurnal response can only emerge clearly where the background astronomical semidiurnal tide is itself weak, which is precisely the case in the semi-enclosed basins where the amplification is observed. Consistently, no such amplification appears in regions known for resonant astronomical semidiurnal tides, such as the North Sea, the English Channel and Irish Sea, Hudson Bay, and the Bay of Fundy–Gulf of Maine system, where the strong astronomical signal masks the wind-forced response.

Minor comments

Very few typos, thank you.

We thank the reviewer for this remark. The manuscript has been proofread again and a few remaining typographical errors have been corrected.

l. 45–46: I don't understand what "significant vertical structure" means here.

We have rephrased this sentence. What we mean is that Arbic et al. (2022) found significant differences between near-surface (Eulerian surface layer) and 15 m SHKE in HYCOM and in the MITgcm LLC4320 in all frequency bands except the semidiurnal one, which indicates a strong vertical shear of the current in the first meters of the water column. The sentence has been rewritten to state this explicitly.

l. 142 onward: is this an offline advection of the numerical particles? What is the temporal scheme used, and what is the time resolution of the surface data? These details are lacking in this section.

This is indeed an offline advection. We have added the missing details: the numerical particles are advected with a fourth-order Runge-Kutta scheme, using hourly 2D advecting velocity field (a combination of ocean current and wind-wave effects) outputs of NEATL50-T-HB-HF as the forcing field for 60 days, without stochastic diffusion, following Chassignet et al. (2021) and **?**.

First paragraph of Section 4: I was surprised that you used undrogued drifters. Undrogued drifters are more sensitive to wind effects, which can alter the resulting signal. Could the authors just mention whether it can be expected to have an impact or not?

We use undrogued drifters because the SHKE we analyze is defined at the ocean surface, and undrogued drifters sample the surface velocity more directly than drogued drifters, which are centered at 15 m depth (Niiler and Paduan, 1995; Lumpkin and Pazos, 2007). This is, for example, why Arbic et al. (2022) compare surface and 15-m currents using undrogued and drogued drifters as reference, respectively. Wind slippage does, indeed, introduce a known artifact, and we now make this point explicit in the first paragraph of Section 4 with the following caveat: undrogued drifters carry a larger wind-slip error than drogued drifters, and this slip has not yet been comprehensively disentangled from genuine surface oceanic variability across frequencies (see Arbic et al. (2022) for a detailed discussion).

l. 262–264: I do not see the patterns described by the authors in the corresponding figure. In particular, low-frequency SHKE is said to be higher in the centre of the North Atlantic in the model, contrary to the observations, but there is a big blue patch there in the right panel. Could you please clarify?

We thank the reviewer for pointing this out. The sentence as written was ambiguous, and the pattern we meant to describe is in fact the opposite of what the reviewer read. In the low-frequency band, the normalized ratio map shows that the model carries more SHKE than the observations in the western boundary currents (Gulf Stream and Gulf of Mexico, reddish patches), whereas the observations carry more SHKE than the model in the basin interior (blueish patches, including the central North Atlantic mentioned by the reviewer). The sentence has been rewritten as: “At low frequencies, the modeled SHKE exceeds the observed one in the western boundary currents (Gulf Stream and Gulf of Mexico), whereas the observed SHKE exceeds the modeled one over most of the basin interior, including the central North Atlantic.”

l. 270: Once → One.

Corrected.

l. 336–339: sentence is repeated.

The duplicated sentence has been removed.

l. 554: insufficient horizontal resolution can enhance energy in the SD band by preventing scattering into the wave continuum (not promoting).

Corrected. The Discussion of Section 6 now reads “inhibiting scattering into the wave continuum”, consistent with Section 5.2 and with Yu et al. (2019).

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Response to Referee #2

Surface Kinetic Energy Distributions in the North and Equatorial Atlantic
Derived from Surface Drifter Observations and High-Resolution
Numerical Models with Tidal Forcing
now:
Surface Horizontal Kinetic Energy Sensitivity to Numerical Parameters
in Tidal-Resolving North and Equatorial Atlantic Simulations
Laxenaire et al., egusphere-2025-6355

This study provides a comprehensive analysis of surface horizontal kinetic energy (KE) levels using high-resolution numerical simulations of the North and Equatorial Atlantic Oceans. They compared the results of numerical drifter experiments with surface drifter observations and further KE estimates in the Lagrangian versus Eulerian framework. The most distinguished part of this manuscript from previous published studies, is the sensitivity tests to explore the impacts of the forcings (tidal & winds) and model parameters on KE estimates by numerical simulations. Overall, this paper is well written and clearly organized, and the findings are of significant interest, offering valuable insights to the modelling community.

I highly recommend this paper for publication in Geoscientific Model Development after addressing the following major concerns about the objectives, results and reasons for the discrepancies between observations, different framework, and various numerical simulations. And some minor corrections are also required to be done.

We thank Referee 2 for the careful reading of the manuscript and for the constructive comments. Like Referee 1, Referee 2 indicates that our findings are of significant interest but that the manuscript did not bring them forward as clearly as it could, both in the way the objectives and results are presented and in the discussion of the discrepancies between datasets. We have therefore undertaken a substantial restructuring effort, presenting our results more clearly and discussing them in greater depth, and we hope that the revised version now adequately addresses the reviewer's concerns. The result is a substantially modified manuscript that we hope now addresses the reviewer's remarks. The reviewer's comments are reproduced in bold below and our responses follow in normal text.

Major comments

1. Title alignment. The title does not convey the motivation and key findings of this paper. In both the abstract and most of the results section, the authors focus more on the numerical Lagrangian versus Eulerian analysis and on the sensitivity of the Eulerian simulation to the choices of parameters and forcings. The comparison between surface drifter observations and numerical simulations does not seem to be a central topic of this study. The authors should carefully reconsider the title.

We agree and have revised the title to "Surface Horizontal Kinetic Energy Sensitivity to Numerical Parameters in Tidal-Resolving North and Equatorial Atlantic Simulations". The new title makes it explicit that the sensitivity to numerical parameters is the core of the paper, and no longer suggests that the drifter comparison is the main topic. We have also adjusted the abstract to give the sensitivity-to-parameters message in the first sentence rather than the drifter comparison.

2. Manuscript structure and visualisation. The manuscript is long and repetitive. It would be easier to read if the authors combined the zonally averaged rotary spectra with the KE maps for each subsection, so that the texts and figures are next to each

other. The current figures are somewhat repetitive. The authors could consider using more diverse plots to present their results (e.g. Figure 5 in Arbic et al., 2022; Figure 4 and Figure 5d–f in Zhang et al., 2024).

First, we agree that the manuscript, and in particular the sensitivity-test sections, is repetitive both in text format and in figure layout. The mirroring of the sensitivity subsections across twin experiments is deliberate, as the goal is to provide directly comparable diagnostics for each parameter change. To make the paper easier to read, however, we have extended, at the end of each twin-experiment subsection, the paragraph that discusses the global effect of the parameter on each frequency band. We have also followed the reviewer's advice and combined the zonally averaged rotary spectra with the surface horizontal kinetic energy (SHKE) maps into a single figure per twin experiment, so that the full diagnostic for one sensitivity test now sits on a single page. Repeating the same layout across the figures of the twin experiments is also a deliberate choice that allows the reader to easily compare panels from one experiment to the next.

Second, regarding the diverse plots used by Arbic et al. (2022) and Zhang et al. (2024), these studies compared far fewer datasets than ours, which allowed them to dedicate one figure to each frequency band. Applied to our seven simulations together with the Lagrangian and drifter references, the same approach would substantially multiply the number of figures, which would make an already long paper even longer. We have preferred to introduce a new figure (Figure 11 in the revised manuscript - see below), which provides a cross-experiment visualisation in which each change between twin experiments is represented by a single panel showing the relative change in SHKE in each frequency band and in each subdomain. Figure 11 is now the backbone of the discussion in Section 6.

3. Drifter experiment design. Why did the authors choose observations from undrogued drifters? Why does the OP 1/2° experiment (Figure 1b) have less coverage in the northern part of the domain (say, 50°N and further north) than the undrogued drifter distribution (Figure 1c)? The distribution of trajectories in Figures 1b and 1c looks very different; in this case, does the comparison in Section 4.1 (Figures 3–4) make sense? Why not use OP Seed Drifters to compare with drifter observations directly? Section 4.2 and its figures may not be necessary or could be moved to the supplementary material.

These are three related questions and we address them in turn.

First, undrogued drifters are used because the SHKE we analyse is defined at the ocean surface, and undrogued drifters have been described as sampling surface velocities more accurately than drogued ones (Niiler and Paduan, 1995; Lumpkin and Pazos, 2007). This is, for example, why Arbic et al. (2022) compare surface and 15-m currents using undrogued and drogued drifters as reference, respectively. We have added a sentence stating this reasoning explicitly in the first paragraph of Section 2.1.

Second, the OP Seed 1/2° experiment does not extend beyond 65°N and below 15°S on purpose. These latitudinal restrictions were imposed to avoid proximity to the model open boundaries (located near 28°S and 80°N at the Fram Strait); the northern limit of 65°N also corresponds approximately to the Greenland-Scotland Ridge, which forms a natural boundary of the North Atlantic basin. We have added a sentence in Section 2.2 to make this choice and its justification explicit.

Third, we thank the reviewer for this suggestion, which we have adopted. In the revised paper, OP Seed Drifters is now the Lagrangian reference in both subsections of Section 4: it is compared to the undrogued drifters in Section 4.1 and to the Eulerian NEATL50-T-HB-HF field in Section 4.2. The comparison in Section 4.1 is therefore built on the same spatial distribution as

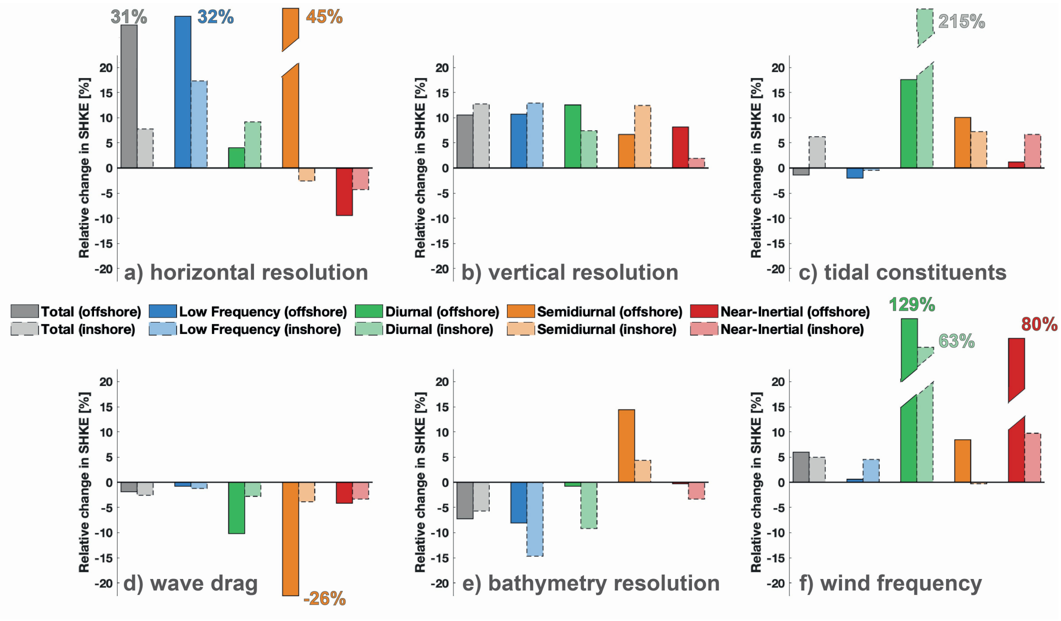

Figure 11: Sensitivity of the domain-averaged surface horizontal kinetic energy (SHKE) to six model configuration choices, separately for the offshore (waters deeper than 500 m; filled bars, solid edges) and continental shelf (waters shallower than 500 m; lighter bars, dashed edges) subdomains of the NEATL region. Each panel shows the relative change in SHKE, $(\mathrm{SHKE}_{\mathrm{mod}} - \mathrm{SHKE}_{\mathrm{ctrl}})/\mathrm{SHKE}_{\mathrm{ctrl}}$ (in %), induced by a single parameter modification, decomposed into five components: total SHKE (grey), low-frequency ($>$ -0.5 cpd and $<$ 0.5 cpd, blue), diurnal ($\pm$[0.9, 1.1] cpd, green), semidiurnal ($\pm$[1.9, 2.1] cpd, orange), and near-inertial ($\pm$[0.9, 1.1]f cpd restricted poleward of $\pm 5°$ latitude, red). The six parameter pairs (control to modified) are: (a) horizontal resolution, NEATL12-T to NEATL50-T; (b) vertical resolution, NEATL12-T to NEATL12-T-HVR; (c) number of tidal constituents, NEATL12-M_2 to NEATL12-T; (d) inclusion of internal wave drag, NEATL50-T to NEATL50-T-WD; (e) bathymetry resolution, NEATL50-T to NEATL50-T-HB; (f) atmospheric forcing temporal frequency, NEATL50-T-HB to NEATL50-T-HB-HF. The vertical axis is clipped at $\pm 20\%$ for readability; values outside this range are reported numerically above (positive) or below (negative) the corresponding bar.

the observations, and residual differences cannot be attributed to sampling density. The impact of OceanParcels sampling density itself (former Section 4.2 in the initial submission) is quantified separately in Supplementary Section S4, which compares OP Seed Drifters to OP Seed 1/2°.

4. Depth of discussion. Most of the results are a basic description of plots and comparisons. More discussion about the underlying reasons or mechanisms would be valuable. The Summary is also too long and repeats information from previous sections. It would be a good idea to make this section brief, leaving only key take-away messages, and to add a short paragraph at the end of the Summary with implications for using drifter observations and numerical Lagrangian versus Eulerian analysis of KE, and directions for future studies.

We agree and we have revised both the body sections and the summary/discussion section.

In the body of the paper, we have expanded the mechanistic discussion in Sections 5.1 (e.g., tidal vs non-tidal diurnal), 5.2 (e.g., near-inertial transfer to depth), 5.3 (e.g., role of the vertical grid in resolving horizontal flows), 5.4 (e.g., selective action of wave drag equatorward of critical latitudes), 5.5 (e.g., transfer from low-frequency to internal-tide generation over better-resolved topography), and 5.6 (e.g., resolved diurnal and semidiurnal wind cycles). Each subsection now closes with a paragraph that attempts to explain the diagnosed signal rather than only describing it.

Section 6 has been restructured into a discussion of per-band parameter sensitivity, built around Figure 11. Rather than restating each pair of experiment band by band as in the initial submission, the discussion now focuses on each frequency band in turn: it first recalls the model-drifter difference in that band after accounting for the Lagrangian distortion quantified in Section 4.2, then discusses the two or three parameters that most strongly affect the SHKE distribution there, and, where applicable, indicates how these parameters could be adjusted to reduce the model-drifter gap.

Minor comments

Table 2 and Table 3 show overlapping information. Keep only one in the main text; the other could be removed or placed in the supplementary material.

We have merged both tables into a single one (now Table 2 in the revised manuscript), which retains the absolute SHKE for the total signal together with the fractional SHKE per band. The full set of absolute SHKE values per band (previously Table 2 in the initial submission) has been moved to the supplementary material as Table S1. The main-text discussion is now organised in fractional terms, which is the quantity that carries the cross-dataset comparison, and the absolute values are available in Table S1 for reference. Note that, in Section 6, absolute values are provided again in a new form in Figure 11.

l. 243: this sentence refers to Yu et al. (2019)? Then Yu et al. 2019 should be put into brackets.

The citation has been corrected to the parenthesised form (Yu et al., 2019).

l. 266–267: repeating information “The bipolar pattern in the diurnal band, with more KE poleward of 30°N in the observations but equatorward in the model,” has already been said at the beginning of l. 265. The authors should try to be concise when describing the results.

This duplicated description has been removed. The critical latitude is now introduced once, in the paragraph where Figure 3 is first discussed, and simply referenced without redescription in the later paragraphs of the section.

l. 270: “Once can . . . ” → “One can . . . ”?

Corrected, thank you.

There might be some other typos and minor errors throughout the text; the authors should carefully check and correct them.

The manuscript has been proofread again and we hope that the remaining typographical errors have been corrected.

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