Authors' response to both Referees regarding
Review of : Lagrangian trajectory modelling for a person lost at sea. . ..
By Licer et al

The authors would first like to thanks both reviewers for taking the time to read and comment on the paper. We believe their comments led to a much improved and clearer revised version of the paper. Before we address their respective comments point-by-point below, we would like to briefly recapitulate major modifications to the paper content here:

- FlowTrack sections of the paper were left-out of the revised version since we agree with the review that OpenDrift-FlowTrack comparisons lacked added value to merit inclusion.
- Independent verification of survivor's trajectory estimate is performed, using WERA HF radar back-propagation simulations from the beaching site. Both trajectories were found to be qualitatively consistent.
- A 10-month verification of the NEMO model was performed against HF radar data in the Gulf of Trieste. This allows us to put numbers on NEMO performance during the drift. Unfortunately this performance was below-average and this is now reflected in the paper where meridional and zonal model biases are stated.
- Current-only and wind-only simulations were performed with "person with surf board" drifter type. Wind-only simulations are now included in the revised version of the paper and analyzed alongside full wind+current simulations.
- Quantitative estimation of performance of each simulation was added to the paper. We base these estimations on stranded and active particle distributions over distances from the beaching side. This allows us to quantify somewhat the performances of each simulation type even in the context of poor NEMO model performance. "Person with surf board" scenario yields also quantitatively the best results.

Our point-by-point responses are below. Reviewer suggestions are typed in **bold-face**, author responses (AR) are normally typed.

Anonymous Referee #1
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Review of : Lagrangian trajectory modelling for a person lost at sea. . ..
By Licer et al

The subject of the paper is relevant, since it evaluates the performance of two Lagrangian models versus a real accident with a person lost at sea during an extreme meteorological event. The paper implementation though lacks in my opinion of several crucial elements. I therefore recommend re-submission, and ask the authors to address the points detailed in the following.

AR: We thank the reviewer sincerely for taking the time to read and comment on the paper!

1) In Section 1, the authors introduce the reconstructed trajectory of the survivor (Fig.1b), that they use to test the two proposed models. I find the trajectory and the associated uncertainty (500 m radius) quite arbitrary. Was the survivor in possession of a GPS, or at least of a watch (time information)? If so, this should be

mentioned, and if not, how did he estimate time and position of the trajectory? In absence of solid information, I think that the evaluation should be based mostly on the only secure data point, which is the beaching point (in space and time) of the survivor, rather than trying to quantitatively match the specific positions evaluated along the trajectory.

AR: We thank the reviewer for this important question – we followed reviewer's instructions closely. No, the survivor was not in posession of a GPS or a watch. Therefore the reviewer is right to point out the arbitrary nature of his estimates. This reviewer's comment has - in conjunction with demands from the second reviewer – led to our independently verification of the survivor's drift trajectory. As described in a new passage in the paper this was done by interpolating in space and time the HF radar currents during the period of the drift, and to then use these measurements to compute Lagrangian *back*-propagation from the *beaching* location as the reviewer suggested. This simulation yields a trajectory which seems largely consistent with survivors estimate. We have explained trajectory verification in a separate section and plotted the results in Figure 4. We hope the reviewer will agree that this substantiation gives some credibility to the survivor's estimate and that we can use it as a valuable qualitative orientation also during the drift, not only at the beaching point.

2) The description of the two Lagrangian models is inconsistent, the conceptual differences between the two are not clearly outlined, and a number of basic information that are then used later on in the paper, are not provided. Specifically, why is the particle equation written for Flow Track (Section 4.2, eq 1) and not for OpenDrift (Section 4.1)? If the basic equation is the same, it should be introduced in Section 4.1 and the differences in parameterizations and considered processes should be discussed up front, for instance in terms of wind drag and lift, Stokes, turbulence etc..
Also the model implementation should be better discussed. How many particles are typically launched in each model? (this is mentioned for Flow Track but not for OpenDrift) How are the results diagnosed? In Section 5, it is mentioned that for OpenDrift a Rescue Area (RA) is computed as a polygon based on particle location, while in Section 5 and 6, it is mentioned that a RA cannot be computed for Flow Track. . . I do not understand why is it so, and I think it should be better explained. Also, all these aspects should be presented up front in Section 4, rather than at the end of the paper.

AR: We thank the reviewer for this comment. We agree that the two Lagrangian models are different in many respects and this is often an issue when comparing different model performances. After both reviewers pointed out this comparison as a weakness of the paper, we decided to leave FlowTrack section out and instead deepen the OpenDrift part.

Now there is only one Lagrangian model in the paper (OpenDrift), but it is presented in more depth and used in more ways. It is used for trajectory verification on HF currents and subsequently for forward-propagation with modelled currents. We hope this lessens the confusion and clarifies the message of the paper.

However, to answer the reviewer's question about RA in FlowTrack: the RA can of course be computed also for FlowTrack which, as reviewer mentions, should indeed have been pointed out in the first version of the paper. However the particles in Flowtrack simulations form a very thin sickle(y) geographic shape so the actual RA is misleadingly small and therefore not very telling. This question however has now been put aside since FlowTrack was omitted in the new version of the paper.

3) The discussion of model evaluation in Section 5 is in my opinion very unclear. The paragraphs at pg12 and 13 for PIW and PPV are simply repeated with the RA values changed. . .What are the grey shading areas shown in Fig. 8-10 and how are they computed? I do not see a clear difference in the three cases. The authors seem to favor the results of OpenDrift PPV because the RA is more reduced (even though it still covers the whole Gulf?), but the RAs are not shown in the figures. In general, as mentioned in point 1) I think that the quantitative assessment should be
based mostly on the beaching point. What is the distribution of beached particlestime and space for the 3 configurations?

AR: We thank the reviewer for the suggestion. We have followed it and we now include the calculation of the distribution of stranded and active (still in the water column) particles and plotted its histogram over distances from the beaching location at beaching time for both drifter types. These plots are now depicted in Figure 10 in the paper. The following analysis was added to the paper:

“Top right panel in Figure 10 indicates that the distribution maximum of "person with surf board" drifters is positioned about 12 km closer to the beaching point than the distribution maximum of "person in water" drifters. The fact however that the distribution maximum of "person with surf board" drifters is also positioned about 12 km from the beaching point indicates that there is a lag in the movement of these drifters. As mentioned above, this is very likely due to the NEMO model surface current estimation during the event. These conclusions are also consistent with spatial particle distributions in Figures 8 and 9: at beaching time, "person in water" particles are dispersed over a much wider area than those of "person with surf board" type. Top right panel of Figure 10 reflects that.

However, and regardless of this lag, when focusing on the accumulation of stranded particles (bottom left panel of Figure 10), we may reach a conclusion that at beaching time about twice as many "person with surf board" drifters stranded within 5 km of the beaching point than those of "person in water" type. The same holds for particles stranding within 10 km radius. Within 20 km radius this ratio triples. All these results quantitatively substantiate earlier qualitative claims of better performance of the "person with surf board" drifter type for this case study.”

Included in the mentioned section are also wind-only simulations results, treated in the same fashion as indicated in the passage above.

4) The conclusions (Section 6) are in my opinion not satisfactory. The authors mention that OpenDrift is more suitable for Search and Rescue applications because it is more operational, i.e. it has a classification for object parametrizations, and it provides Ras. But these points were known from the

beginning, given the model characteristics! What is the added value of the comparison?
I think the authors should discuss in an objective way the results, and indicate strength and weakness of each model with respect to the actual performance. Of course,is also important to point out the shortcoming of Flow Track in terms of operational performance, but indeed that was a given and we did not need this exercise to reach this conclusion. . .

AR: We agree that there was too little added value to the original model comparison and that it does not warrant publication. Flowtrack is now out of the paper.

5) Finally, I would like to make a general comment. From the patterns of currents and wind (Fig.5,7), it looks like the trajectory of the survivor was likely to be strongly influenced by the ocean currents (that facilitate the entrance inside the Gulf,) while the trajectories of both models tend to overestimate the wind influence (that moves them more to the north-west).
Indeed Fig.6 shows that the wind input in the Lagrangian models is approximately double with respect to the currents (please clarify the dimensions of the variables that Figure: are they velocities or are they model inputs somehow normalized?). On the other hand Fig.5 shows that the NEMO current amplitude is underestimated with respect to the HF radar. So, it is possible that improving NEMO results would greatly improve the trajectories of the Lagrangian models. Alternatively, could the HF radar results be used as inputs in the Lagrangian models? I understand that there is a permanent gap in the middle of the Gulf, likely due to the GDOP, but probably gap filling techniques can be used in it in to ensure a more extended coverage. The authors should explore these aspects.

AR: We agree with this comment and thank the reviewer for it. This comment, in conjunction with the first comment, lead to gap-filling of the HF radar data and to the performing of back propagation from the beaching location. Since back-propagation from the beaching location using HF radar currents is consistent with survivor's trajectory at the beginning and at the end, we believe the reviewer is right: improving NEMO results would lead to an improvement of Lagrangian tracking.

As we now point out in the paper, we performed a 10 month verification on HF radar data with NEMO and it seems the model performance during the Scirocco was particularly weak since the biases in (u,v) currents are otherwise much lower. Verification of the NEMO model versus ten months of hourly HF radar currents (not shown in this paper) yields a bias in zonal velocity between 0 and -2.5 cm/s and a bias between +2.5 and -2.5 cm/s for meridional velocity. NEMO model underestimations during the limited period of this case study were however even larger: spatially averaged (over the HF domain) and temporaly averaged (over the period of the drift) NEMO biases amounted to -6.3 cm/s for zonal velocity and a bias of -9.2 cm/s for meridional velocity. NEMO setup therefore exhibited below-average performance during the period of interest. This issue with model performance during storm conditions was explicitly pointed out in the paper and will have to further addressed in a separate investigation.

As far as Fig 6 is concerned, thanks for pointing this out. Plotted quantities are drifts, and therefore Eulerian velocities (in case of currents) or OpenDrift downwind slopes x u10 (in case of winds). Units are m/s. No other normalization is used. All this is now explicitly stated in the Figure caption.

Anonymous Referee #2
Received and published: 8 February 2020
General Comments

In this work the potential trajectory followed by a windsurfer lost at sea in the surroundings of the Gulf of Trieste during a storm that took place in October 2018 is simulated through a Lagrangian approach.
To this end, high resolution oceanic and atmospheric models are used as input for two Lagrangian tools: OpenDrift and FlowTrack. Despite using the same numerical integration scheme (Runge-Kutta 2nd order) they present some differences. Likely the most important is that Open Drift offers pre-calibrated coefficients for downwind and crosswind components more suitable to simulate the drift of a person at sea, specially if the wind drift is dominant. As a result, the final distribution of particles simulated by OpenDrift match better the (tentative) inferred trajectory reconstructed by the authors from an interview with the windsurfer.

My impression is that this paper is based on a compelling idea and that results can be potentially useful at regional and operational level. English is good and one key strength is that model data (especially ocean currents) have very high resolution. However, I feel that this version of the manuscript needs some reorganization, a more accurate description of methods, a more elaborated assessment of the different con tributions of wind/ocean currents/(waves?) terms to the simulated trajectories, and an improved treatment of uncertainties and search and rescue areas, before being considered suitable for publication.

AR: We thank the reviewer sincerely for taking the time to comment on the paper!

In the below lines I elaborate further my above overall impression.

Major Comments
A- It is rather weird (and a bit confusing) to describe wave data from ECWAM model when it is not used later in the Lagrangian simulations. Indeed I think it is more logic to start in Section 2 with the quite general equations of Lagrangian particle tracking (current Section 4), and later describe in Section 3 observations and model data. In this way the fact that you neglect the Stokes drift is clearly stated and there is no need to include a description of the wave model in Section 3. Then in Section 4 you could show the results on the validation of model data with observations, and so on.

AR: We thank the reviewer for the comment. We agree that the approach was confusing. Before we clarify changes to the revised paper any further, we would like to say the following in our defense: it is true that ECWAM was not used in the final simulations with FlowTrack. But we did need ECWAM to compute the surface gravity wave wavelengths and to justify that we may indeed neglect Stokes drift. This is why it was eventually presented. Since FlowTrack is out of the paper, ECWAM is also out of the paper. With OpenDrift this was not an issue since its parametrizations already include wave effects and OpenDrift consequently does not require the wave model at all. We also hope the existing Section order is now working better.

Additionally, OpenDrift is now presented in more depth and additional simulations were made. We will address these changes point by point below.

B- You say that you do not consider the Stokes drift for your simulation because wavelengths are significantly larger than the size of a person/windsurf table. However, as far as I understand in the case of microplastics, wavelengths are proportionally even larger but the Stokes drift has an important role on their distribution (e.g. van den Bremer and Breivik; Onink et al., 2019). Am I wrong? I think that it is likely simpler to say that the Stokes drift is generally a second (or third) order term in the Adriatic Sea in terms of magnitude (e.g. see Fig. 1d within Onink et al., 2019) . Indeed it would be a good exercise to verify this point with an in-situ wave buoy or with your wave model data.

AR: As pointed out above, we now leave sections of FlowTrack, and consequently ECWAM, out of the revised paper. However, we will try to respond to the reviewer's question. Stokes drift is computed as a temporal mean of Lagrangian water particle velocity over its unclosed orbit in a surface gravity wave. It is in principle computed for the water particle moving passively with the wave field, not for material objects in the water. Although this is not a specialty of any of the authors, we would expect that the microplastics can safely be said to move directly with the fluid in the wave field. On the other hand, as far as we understand, there seems to be a difficulty to claim the same for larger material objects. Their orbits in the wave field do not generally follow closely the fluid motion due to a gravity wave. Therefore the temporal mean of the *object's* Lagrangian velocity over one cycle of its orbit, i.e. its "Stokes" drift, may differ substantially from the temporal mean of the surrounding *fluid's* Lagrangian velocity, i.e. water's Stokes drift. The argument was made in the references cited in the original version of the paper (Hackett et al. 2006, Breivik and Allen 2008) that we can only assume that object's orbit coincides to any significant degree with surrounding fluid particles orbits if the length of the material object is close to the surface gravity wave wavelength. In this case, Stokes drift calculation for fluid is a good estimate for the temporal mean of the object's unclosed orbits in the wave field. In other cases the orbits differ too much to make this assumption. This is how we understand the issue.

C- It is not explained which expression you use to simulate turbulent effects (random numbers) at each time step. Is it based on a uniform or a normal distribution? Is it different for each direction? Is the maximum number of 0.02 m/s based on observations or a model assessment? Indeed in coastal regions where strong gradients in ocean currents exist the magnitude of diffusivity can change significantly between relatively close grid cells depending on the ocean features. Please clarify this point.

AR: As noted above, FlowTrack was removed from the paper. Turbulent component was however computed from a normal distribution around this value (for each direction) which was inherited from an older Lagrangian code (unpublished), which based it from empirical velocity data from some specific nice-weather situations. We are aware that a constant turbulent diffusion is unacceptable for velocity fields with significant shear and we are improving FlowTrack to be able to ingest NEMO horizontal eddy viscosities, obtained via Smagorinsky or other schemes which take into account local velocity shear.

D- With respect to the validation of the model data with observations, why do not show a comparison between the ADCP data (Figure 6, violet arrows) and the closest grid cell ocean model velocity? This would help to provide some numbers on the discussed underestimation of modeled currents. Also, do you have any reference in which HF radar velocities have been validated? Add it (them) to Section 2.2.

AR: We thank the reviewer for the suggestion. We have followed it to the point to perform a 10-month verification of the NEMO model against HF radar data. We hope the reviewer will agree that this is a more extensive way to verify NEMO than a single-point ADCP comparison. We do not show plots of the results but we do state numerical values of the errors in the updated revision of the paper: they indicate below-average model performance during the period of the drift and this is pointed out in the paper.

The following passage was added to the paper: "Verification of the NEMO model versus ten months of hourly HF radar currents (not shown in detail in this paper) yields a bias in zonal velocity between 0 and -2.5 cm/s and a bias between +2.5 and -2.5 cm/s for meridional velocity. NEMO model underestimations during the limited period of this case study were however even larger: spatially averaged (over the HF domain) and temporaly averaged (over the period of the drift) NEMO biases amounted to -6.3 cm/s for zonal velocity and a bias of -9.2 cm/s for meridional velocity. NEMO setup therefore exhibited below-average performance during the period of interest. This will have to be further addressed as a separate issue and needs to be kept in mind when interpreting results below.
"

E- Other unclear points are:

- How do you estimate the light red circles in Fig. 8-10?

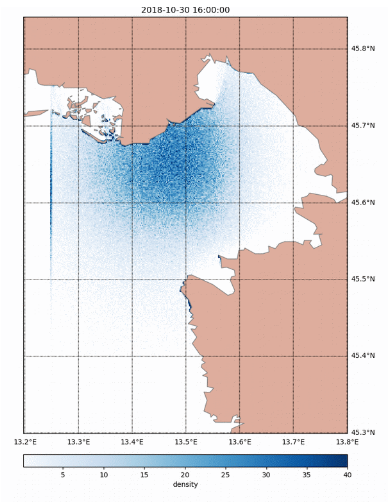
AR: We thank the reviewer for this question. These were initial estimates by the survivor. We now expand on our reply to the other reviewer in this regard. The survivor was not in posession of a GPS or a watch. Therefore both reviewers have justification to point out the arbitrary nature of his estimates. They are indeed arbitrary but we nevertheless took them as a rough estimate – this reviewer's comment has - in conjunction with demands from the second reviewer – led to our attempt to independently verify the survivor's drift trajectory. This was done by interpolating in space and time the HF radar currents during the period of the drift, and to then use these measurements to compute Lagrangian *back*-propagation from the *beaching* location. This simulation yields a trajectory which seems largely consistent with survivors estimate. We have explained trajectory verification in a separate section and plotted the results in Figure 4. We hope the reviewer will agree that this substantiation gives some credibility to the survivor's estimate and that we can use it as a valuable qualitative orientation also during the drift, not only at the beaching point.

- Why particles are initially deployed in a rectangular shape?
AR: The square shape was simply one of the ways particles are seeded in FlowTrack. We comment a bit more below.

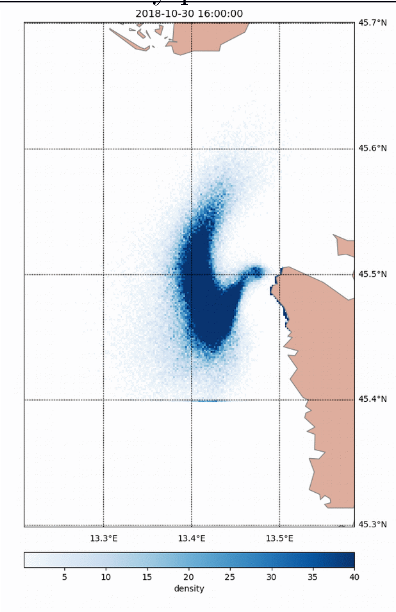
- You deploy initially 480 particles, one for each value of Lp(theta), however in this way the uncertainty introduced by their different initial location is not assessed. What is the impact of the uncertainty in the initial position on the final search and rescue area for each Lp(theta)? Is it significant?
AR: We thank the reviewer for this question. Yes, this is a good point. We haven't tested this sensitivity but it is perhaps worth noting that the model resolution is 1 km and the deployment square size is 1 km. Therefore the dimension of the deployment shape is equal to the model grid size. Therefore we would expect that in this case the uncertainty in the initial position would not have, in itself, significant impact on the SAR areas since the model velocities do not change (or change very little) over the dimension of the release shape. However, note again that FlowTrack passages were removed from the paper.

F – I find that would be interesting to show also the distribution of simulated particles with only wind drift/only ocean currents, and to estimate how large is the dispersion of particles (final area of search and rescue) for both cases. It would show graphically how predominant is the wind drift in the advection of particles and the lag with only ocean current.
AR: We thank the reviewer for the suggestion: we have performed both (wind-only / current only) simulations for "person with surf board" drifter type as the reviewer suggested. We include the results below. Wind-only particle simulations were also included in the revised Figure 9 and in Figure 10 and we comment upon them in the text as well. Given that the situation was wind dominated, as is clear from "wind+current" and "wind-only" simulations, we did not include "current only" simulations in the paper.

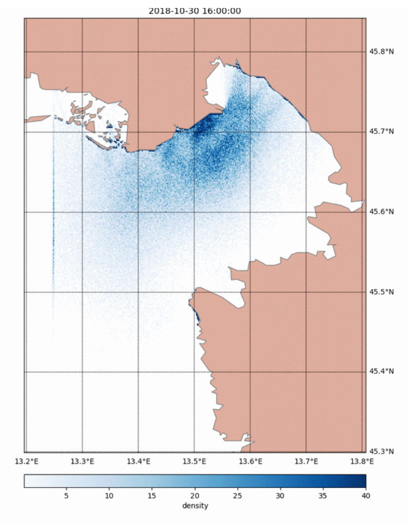
<u>Wind-only particle distribution</u> after last time step:



Current-only particle distribution after last time step:



Wind+current distribution at the last timestep:



These figures indicate that winds are the dominant factor in this drift, but the currents play their role in elongating the spread along the coastal current "jet" direction (seen in left panel of Figure 6) thus moving it closer towards the beaching site.

G- My major criticism to this work is the approach you follow to show your results. Considering that it is aimed to be useful for search and rescue (SAR) tasks and, therefore, time is critical, I think that to show trajectories is far less useful

than to show areas (or contours) of accumulated probability constructed with suitable mathematical functions. In terms of accumulated probability the areas for search and rescue tasks can be more easily prioritized. Your estimated areas after just few hours of simulation look pretty large to be useful for search and rescue tasks. Additionally, the probabilistic approach naturally includes the fact that there are uncertainties everywhere. Even more, probability contours can have a bimodal distribution, while your polygon seems to include all particles inside irrespective of the spatial holes among them. For example, this approach can be found in Abascal et al., (2010). You need to convince me that for SAR tasks your current approach is reasonable enough, otherwise I suggest to redo your Fig. 8-10 with a probabilistic perspective (showing e.g. contours of 50%/70%/90% of accumulated probability estimated from the distribution of your particles), which is relatively easy to implement. In the latter case I suggest to remove the word "trajectory" from the title.

AR: We thank the reviewer for the suggestion: we have rerun and replotted all simulations with OpenDrift to show numerical particle densities [number of particles / m2]. While not showing probability contours but particle number density, this approach is not substantially different and has been commonly used by the authors of OpenDrift themselves (Roehrs et al, 2018; Dugstad et al, 2019). We hope this satisfies the reviewer. We have followed reviewer suggestion and removed the word "trajectory" from the title.

Other Comments
Title. Change "for" by "of".
AR: Done.

Ln 2. Suggest to change "He was drifting" by "He drifted".
AR: Done.

Ln 6. We "modeled".
AR: This is now out of the text.

Ln 52. Unclear how you estimate the +/- 500 m of error.
AR: As noted, this is survivor's subjective estimate. We now state this in the paper explicitly and we add independent verification of survivor's trajectory using back-propagation on HF radar currents.

Ln 65. "By the time he is entering" . . .
AR: Done.

Ln. 218. Remove point after "day"
AR: Done.

Ln. 218. "all directions..."
AR: Done.

Ln. 224. "generates a westward initial current".
AR: Changed to "generates a westward inertial current"