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Geoscientific Model Development Discussions

Interactive comment on "MEDSLIK-II, a Lagrangian marine oil spill model for short-term forecasting – Part 2: Numerical simulations and validations" *by* M. De Dominicis et al.

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The Authors thank Referee 3 for the useful comments and detailed analysis that will help to improve the description of the model in the present paper and to address the further developments of the model.

As in the scopes of the GMD journal the paper wants to be a comprehensive descriptions of a numerical model, in order to give an accessible description to a wide community of scientists to allow the reproducibility and implementation of the model by different users. In this paper we describe for the first time new concepts and mathematical formulations such as: 1) how to reconstruct an oil concentration field from the

oil particles adv/diff and transformation processes (oil tracer grid, etc.); 2) the different oil spill state variables, i.e., the oil slick and oil particles variables; 3) all the possible current corrections to be used even with sophisticated oceanographic models now recently available (operational oceanography analyses and forecasts).

The papers describing oil spill models up to now have, in our opinion, offered only partial explanations and they are mainly in gray literature, due to the fact that oil spill models are usually commercial tools and the complete description with aim of reproducibility is unintended.

We have added a phrase in the introduction to make clear from the beginning the novelty of Part 1 of our paper, as described above.

Specific comments:

In the following we list the Referee comments and our answers (numbered from 1 to 24):

Introduction: Minor note: the GNOME model is not restricted to a point source. There is more up to date and detailed documentation available on the NOAA/ORR web site, and as a NOAA technical report (recently released): http://response.restoration.noaa.gov/oil-and-chemical-spills/oilspills/resources/gnome-technical-docs.html A number of existing and operational models do indeed model 3-d processes: Sintef's OSCAR, ASA's OilMAP/SIMAP, and to a lessor extent, NOAA's GNOME. And I'm not sure why this was pointed out, as this paper describes very little 3-d modeling in any case.

(1) The updated GNOME model technical report will be cited instead of the old. As usual and as explained before, if 3-D transformation processes are modelled they would be in gray literature papers, i.e., not peer reviewed. If the reviewer points out to technical reports documenting this, we will cite them.

The authors make a point about the attention given to the "tracer grid", but it appears *to be a simple "count the particles in the grid cells" approach.*

(2) It might be trivial that the final outcome was to calculate oil concentration just counting up particles into a grid a cell, but it is fundamental to explain how to reconstruct an oil concentration field. Often the oil spill model results are given only in terms of particle positions.

The authors claim "an innovative treatment of the surface velocity currents.." It appears to be a simple 1st order Euler method, driven by linear interpolation of a circulation model – nothing innovative there. They do add a Wind correction, which is universally done, though often described differently, and stokes drift, also not unique, and questionable in its application in this case. See more detailed notes below.

(3) We thank the reviewer for pointing out that in our text it is not clear that the wind correction is optional, as shown in Part II of the paper. The novelty stems out exactly from this point: 1) even with sophisticated models we may need to make corrections to account for model errors; 2) we should decide each case what to do, as demonstrated in Part II. We have now replaced the phrase at line 10 of page 1969 with the following explanation: "The wind drift term as reported in Eq. (47) is not needed when using surface currents coming from an oceanographic model that resolves the upper ocean layer dynamics. In such cases, adding the wind drift term could worsen the results, as shown in Fig.2 of Part 2. When the wind drift term is used with a 0 deviation angle this term should not be considered as an Ekman current correction, but a term that could account for other near-surface processes that drive movement the oil slick, as shown in one case study of Part 2 (Fig. 4)."

Regarding the Stokes drift, its importance has been demonstrated by the simulations reported in Part 2 and by the work of Pugliese Carratelli et al. (2011), as highlighted by Referee 2 .

Section 2: It seems the goal of the paper is to provide a formal definition of the model equations and solution methods. In this case, it should be noted that the goal of an oil

spill fate and transport model, particularly an operational one, is to simulate the fate and transport of oil released into the sea. The goal is NOT to solve the advection-diffusion equation. Indeed, the advection-diffusion equation is the result of a simplifications of transport processes that may not be well suited to the modeling of actual oil spills in the sea. This may explain to some extent the apparent lack of formalism in other model documentations – modeling by tracking discreet elements in a Lagrangian frame work is a method can can be chosen without reference to the advection-diffusion equation.

(4) The aim of the paper was not to describe the connection between the Lagrangian particles trajectory equation and the Eulerian advection – diffusion equation for a passive tracer. This is a procedure which is conceptually difficult but well known and standardized (Griffa, 1996, Risken 1989), whose outcomes were only partially described in Section 6. The object of the paper was to describe how the advection/diffusion is connected to transformation processes by using particle trajectory equations and empirical formulas to model the weathering processes. To this end eq. (2) and (3) are new for this problem and they have never been written before. We have now added a clarification at page 1954 saying that: "While solving Eq. (3) with Lagrangian particles is well known (Griffa, 1996), the connection between Eq. (2) and Eq. (3) explained in this paper is completely new."

In the description of eq 1, I'm not sure what the authors mean by "modify the tracer concentration by means of mechanical stirring" – that term in the equation is the source/sink term, which could be driven by any number of processes.

(5) The sentence will be rephrased into "modify the tracer concentration by means of general physical and chemical transformation processes".

The approach of modelling the oil transformations only as a function of the bulk slick properties is not universal in oil spill modelling – I'm pretty sure ASA's SIMAP does not work that way, other models are getting more sophisticated in this regard. Indeed, that authors draw attention to their "tracer grid", but do not appear to be making use *of it other than computing the resulting concentrations – something that could be done post-processing, as other models often do.*

(6) This paper does not want to describe the generic way of modelling oil spill transformation processes, but a particular model implementation. If Referee 3 would like to suggest available papers on concentration calculation and more sophisticated way of solving transformation processes, they will be cited in the text.

Introducing eq 4: the authors state: "the oil slick presents itself as a coherent thin layer of material..." This in fact, is not at all how real slicks in the environment behave. During a spill, the surface slick is characterized by a great deal of patchiness, different surface structures, streamers, wind rows, etc. The oil itself can be fresh, emulsion, tar balls, thin sheens, all in similar locations. These structures are driven by mesoscale circulations that are not commonly modeled (if ever). Making the continuous slick assumption may be a reasonable approximation for computational purposes, but that fact that it is a gross simplification should be acknowledged. Note that this in one reason that modeling oil spills is not considered as primarily solving the advection-diffusion equation. I'm a little confused as to why the authors have chosen to work in volume units, rather than mass, while ultimately reporting concentrations in mass units. Of course, conversion is easily accomplished if the density is known, or assumed, so this is really an implementation detail.

(7) Of course on a scale of tens of metres an oil slick does display, after a few hours, the micro-structure the reviewer describes. But to model would require knowledge of the corresponding micro-eddies in the oceanographic and atmospheric circulations. Nevertheless, stepping up the scale an order of magnitude, it does make sense to speak of average properties that advect and diffuse. Just as for any fluid, at the molecular scale there are great fluctuations in concentration but when averaged over larger scales of space and time the fluid satisfies the usual macroscopic equations. In our paper we have shown the complete formulation of the oil spill predictive modelling case. In fact we consider both adv/diff and transformation. The latter are done commonly for the

oil volume changes and the formalism acknowledges that there are two parts in the oil, the thin and thick slick. However we agree with the reviewer that talking about a 'the oils spill presenting itself as a coherent thin layer' is not clear and we have written now at page 1955: "the oil slick is assumed to be represented by a continuous layer of material" and later we modified page 1956 after line 15: "In order to solve Eqs (7) and (8) we need now to subdivide the surface oil volume into a thin part, VTN, and a thick part VTK. This is an assumption done in order to use Mackay (1980) transformation processes algorithms. Despite their simplification, Mackay's algorithms have been widely tested and they showed flexible and robust in operational applications."

Section 3: From Figure 1, it appears that the shoreline is discretized to match the "tracer grid" – this could lead to limitations when the coastline is complex or does not match that grid well. It can also cause problems if the model coastline does not match the boundaries of the underlying circulation model.

(8) The shoreline does not match the tracer grid. Since it is not clear, the figure will be redone. The coast is digitised to a resolution appropriate for each section, which may be a few metres or may be a hundred metres for an almost-straight section.

Eq. 10 indicates that the slick is considered as simply two components – thick and thin portions – this is simplification that is perhaps unnecessary with modern under standing and computational resources. Eq. 14 indicates that the oil is treated as two components: the part that evaporates, and the portion that does not – again, a major simplification that may not be called for – modern oil spill fate models usually use multiple "pseudo components" The introduction of Eq. 22 describes the use of the environmental variables from the "center" of the spill, to be applied to all elements. Again, a perhaps unnecessary simplification. They give a good justification for the approach for the water temperature, but winds are a different matter, and, as the authors point out, even water temperature can vary over relatively small distances in certain locations. Even if operational limitations require the use of a single value, the model could be developed with the framework in place to accommodate spatial and time varying *environmental conditions driving the fate processes.*

(9) The modelling of the thick-thin slicks is one of the sophistications of the Mackay fate model that, in spite of its age, places it at a higher level than the simpler fate models used by many of the existing oil spill algorithms. The reviewer is correct that it would probably be more accurate to use many components of the oil rather than just two but this is complicated by the lack of available fractionation data for many if not most of the oils in our database. Since evaporation is generally almost complete within a day or so we have considered a simpler model no worse than artificially constructing fractional evaporation data. We agree with the reviewer on the possibility to use spatial and time varying environmental conditions. This is certainly a good advice and it will be tested in future version of the code together with the coupling with high spatial resolution winds. We have now written in the conclusions that this will be one of the most urgent updates of the model in the future. At page 1978 we have now added: "In the near future we will update the model formulation to consider first the improvement of the environmental condition resolution for the transformation processes, the space dependent thin:thick ratio and we will develop a complete three-dimensional model maintaining this formulation at the surface."

For initial conditions, it's not clear why a continuous release needs to be broken up into discreet "spills", rather than simply releasing particles continuously (at the time step resolution, anyway).

(10) In the case of a continuous release, the oil spill is discretized into sub-spill to take into account the spatial and temporal variation of temperature and winds. Each sub-spill will be transformed according to the wind and temperature at the centre of each sub-spill, this procedure will overcome the assumption normally made considering one centre of mass of a long lasting oil slick covering a wide region.

The authors discuss initialization from satellite observations – but is seems the model is still limited to a single global thick:thin ratio, rather than a spatially variable concen-

trations. Of course, one can't generally derive thickness from satellite observations, but there is sometimes some information available from over-flight observations, etc.

(11) We understand that we could use different thin:thick ratios, but to our knowledge operational satellite images do not yet have information on the oil slick thickness, and we are not talking about over-flight observations. We have mentioned in the conclusions that we will improve this aspect in the future (see comment 9).

Section 4. pg 1963, line 13: "the lighter fraction of the oil will disappear, while remaining fractions can be dispersed into the water column". In fact, light components of oil often disperse – indeed the lighter components are often the larger fraction of the dispersed oil. The authors mention that emulsification is part of dispersion but they are quite different. Emulsification is not a loss mechanism from the surface slick, and it alters the future fate and transport of the oil – i.e. emulsified oil does not evaporate or disperse the same way, nor is it moved the same by the wind.

(12) We agree with the Referee, the lighter component may be dispersed, especially in case of the high sea and low inter-facial surface tension between oil and water. That is taken into consideration in (B11). We meant only that in the majority of historical cases, evaporation tends to overcome dispersion. The sentence has been rephrased into "In general, the lighter fraction of the oil will disappear, while remaining fractions can be dispersed into the water column". We agree with the note on emulsification up to a point. We do not mean that emulsification is a loss mechanism from the surface, but we need to clarify our explanation. In addition, we thank the reviewer since we found that our explanation was inaccurate and now the emulsification has been described in a separate Appendix and at page 1964 line6 the sentence has been reworded as follows: "The model can also simulate the mixing of the water with the oil, and this processes known as emulsification, is described in Appendix B3. " Furthermore, we now have added a clarification at page 1983, after (B15): "According to (B14) and (B15) emulsification influences the oil viscosity, which, in turn, influences dispersion (B11). Apart from emulsification the oil viscosity increases due to evaporation (B7),

which also influences dispersion."

Section 5: This appears to be straightforward MacKay 1979/1980 approach – this was state of the art in 1980, but there are more sophisticated methods available today – this simple approach may be justifiable for an operational model but the authors need to be clear that they have chosen such a simple approach, and why they think it is appropriate.

(13) For an operational model, Mackay's parameterizations were tested for a long time and proved their stability and flexibility. This is why we have chosen this formulation (see comment 7).

The inclusion of the "adsorption process" on the beach is interesting, and as far as I know, unique – It does make sense that some of the oil that impacted a shoreline would, by various processes, no longer be available to re-float. I like to see more discussion as to how the adsorption half-life could be estimated.

(14) Half-life is a parameter which describes the "absorbancy" of the shoreline by describing the rate of entrainment of oil after it has landed at a given shoreline. Half-life concept has been already used by Shen et. al (1987). At page 1965 the reference to Shen et. al (1987) has been added.

The diffusion approach is a straightforward random walk – very commonly used in particle tracking models. The authors should make some mention of the limitations: only isotropic diffusion in the horizontal, no spreading based on spatial scale, etc. They mention that they use a different vertical diffusion coefficient below the mixed layer – I'd like to see more discussion of this – it is not trivial to determine what to use, or what to do at the boundaries. Also, mixing in the mixed layer is often due to non-diffusive process, notably Langmuir cell circulation – does this approach match what's observed in mixed layers?

(15) The limitations of random walk approach will be clarified. We will add at page

1971 after formula (53) the following phrase: "As for modern high resolution Eulerian models, horizontal diffusivity is considered to be isotropic and the values used are consistent with the estimation of Lagrangian diffusivity carried out by De Dominicis et al. (2012). Regarding the vertical diffusion, the vertical diffusivity in the mixed layer, assumed to be 30 m deep, is set at 0.01 m2/s, while below it is 0.0001 m2/s. This value is intermediate between the molecular viscosity value for water, i.e. 10-6 m2/s usually reached below 1000 m, and the mixed layer values."

In all existing operational oceanographic models, processes such as Langmuir cell horizontal convergence/divergence and vertical cells are not resolved and no parameterization is added into the oil spill model to take into account this process.

Most (all?) oil spill transport models include a wind-driven transport term (usually around 3It's not entirely clear whether the MEDSLIK II model does indeed include the term given in eq 47, though I think it does. In any case they are right that the choice of coefficients is a subject of some debate. Operational experience has shown that the drift factor varies with oil type and weathering state, and that for the most part, the Ekman angle correction is of limited use given changes and uncertainty in the wind direction (particularly with forecast winds) In addition, it has been repeatedly observed in the field that the oil slicks tend to spread out in the downwind direction, generally concentrating toward the "leading edge". Many models account for this with a randomized variation in the wind drift factor (NOAA's GNOME, for instance).

(16) We confirm that MEDSLIK-II has the possibility to insert the wind correction in the form given by (47). We believe that we answered this question at point (3) of this document.

Section 6.2 The application of Stokes drift in oil spill models has been the subject of debate for years. While it makes sense to include the effect, it is often mis-understood and misapplied. In particular, Stokes drift is integration of the Lagrangian particle motion in the waves, integrated over the wave period, and sometimes the depth. However, *while it captures the average forward movement of the water, it only makes sense to use the same value for a tracer moving precisely with the water. Floating oil stays at or near the surface, so does not follow the same orbital velocities as the water in the waves, and thus should not be expected to move at the depth-integrated Stokes drift velocity. This issue is discussed in: Sobey, R.J. and C.H. Barker. 1997 Wave-driven Transport of Surface Oil Journal of Coastal Research Bechtel, Ryan D. and Wickley-Olsen, Erik and Boufadel, Michel C. and Weaver, James and Barker, Christopher H 2008 The Movement Of Oil At Sea Due To Irregular Waves Proceedings of the International Oil Spill Conference. Savannah, GA and other works. Also of note with Stokes drift – it is a small effect compared to Wind Drift. Thus it wouldn't be expected to play a significant role unless there were significant waves that were in a different direction that the local wind-generated waves, such as low-wind conditions with significant swell, or near the shoreline where diffraction aligned the waves with the bathymetry, rather than the wind. The Authors do indicate that they may include driving the stokes drift component with a wave model, but until such time, there may not be much point in including it.*

(17) From the case studies reported in Part 2 of this work, the Stokes drift correction has been found to be of the same order of magnitude of the Wind Drift correction and the Authors believe such drift would play a significant role in the transport. Adding this term allow to lower the discrepancies between the observed and modelled trajectories. Other publications, such as Pugliese Carrattelli et al (2011), support the importance of using the Stokes drift.

We added the references (Sobey et al. 1997 and Bechtel et al. 2008) in order to show the different Stokes drift implementation for oil spill transport.

Regarding the usage of a complete wave model instead of our parameterization, we have compared the two solutions many times and validated our parameterization in Part 2 of this paper. We do not believe we should make any other explanation here.

Section 7.1 The interpolation method appears to be a simple bi-linear interpolation – reasonable, but nothing special there. It does not appear to accommodate non-rectangular grid models, however, which could be a serious limitation.

(18) Our model does not consider non-rectangular grids. This development will be done in the future.

Section 7.2 The time integration scheme is about as simple as it gets, but adequate. It's not clear to me why they would need 30 weathering time steps per transport time step – this is a pretty simple weathering algorithm, and you're not updating the environmental conditions that often at all.

(19) The weathering processes equations are numerically stiff, and we need to select the step size small. We have replaced part of the phrase at page 1973 near line 20: "The transformation equations are stiff and to integrate them the time step should be a fraction of the Lagrangian time step, as done in other active tracer modelling (Butenschön et al., 2012)."

Section 7.3 Dispersion into the water column is very much a function of wave conditions – if you are computing sea state for the Stokes drift, maybe it should be used here. When a particle is returned from the shoreline to be a surface particle – where is it placed? The sedimentation algorithm has no explanation – in general, dispersed oil droplets to not sudden stick to the bottom when they get close – and the 20cm seems completely arbitrary. In practice, "sedimentation" is usually used to describe the process whereby oil droplets stick to sediment particles in the water – the combined particles often are then more dense than sea water and may settle out – but this has little to do with coming close to the bottom. Look in the literature for recent work on "Oil Mineral Aggregates" for up to dare info about this process.

(20) We agree with the reviewer, but at the moment we have not included the wave height in the dispersion calculation.

We have rewritten Line 11-12 at page 1975 Line 11-12: “When a particle is washed back it is of course depleted by the oil that has become permanently attached (Eq. 40) and it is new position is calculated using Eq. 46. The model does not follow any further the fraction of a particle’s oil that is permanently deposited. It is important to realise that whole particles are not lost as permanently beached but only a fraction of them. The actual number of parcels remains constant. The only case in which a parcel is lost from the model is when the particle is supposed to be deposited on the bottom. However, no proper parameterization of sedimentation is now included into the model, the particles are considered to be lost from the water column when they are less than 20 cm from the bottom.”

Section 7.4 For selecting resolution of the tracer grid: the advection scale, combined with the grid scale of the input variables (such as surface currents) drives the limit of model time step. But tracer grid scale isn’t limited by this – if particles "skip over" grid cells, and you want to know where there were at the intermediate time, you could compute those skipped cells as impacted – but for the most part, one wants the results at the model time step – there is no need to worry about skipped over cells.

(21) The impact of this approach is examined in Part 2 of this work where the sensitivity of the tracer grid resolution and number of particles is analysed. Conclusions are in agreement with the reviewer arguments so we feel there is no more explanation to give.

Selecting the grid size based on minimum oil concentration to be resolved is not quite that simple either. Where one particle is in fact the smallest amount of oil, you can’t properly resolve one particle in a grid cell – there is a stochastic element to the process (the diffusion), so a single particle may or may not actually be in the box when the underlying distribution would be that small. So you really should consider the minimum concentration value to be a few particles in each grid cell. This is one of the issue with counting particles in grid cells – the results are resolution dependent. For low concentrations, you will tend to get very "patchy" results. see: Lehr, William and Barker, C. H. and Simecek-Beatty, Debra, 1999. New Developments in the Use of Uncertainty

in Oil Spill Forecasts Proceedings of the Twenty-Second Arctic and Marine Oil spill Program (AMOP) Technical Seminar pages 271–284 for a discussion of similar issues.

(22) As the Authors already commented in (2) it might be trivial that the procedure to calculate oil concentration is to count up particles into a grid a cell, but we never found this explained in the peer reviewed literature and so we explained in details in our paper.

Conclusions: The authors state that they have paid particular attention to the connection between the Lagrangian particle approach and the oil concentration reconstruction – however all they seem to have done is compute concentration by counting particles in grid cells – this is about the simplest way to do it it is very common, and they did not address the issues that result from this approach. The authors argue that the model presented provided a good platform for development of a proper 3-d model – I see little argument for this. It has no features that make it seem particularly suitable for that use.

(23) It is fundamental to explain all the model details to allow reproducibility and to have a rigorous description of the model. Older oil spill model results are given only in terms of particle positions that are then displayed as just black dots. One can see the limitations of this approach by considering the Nowruz spill in the 1980s: this spill was allowed to continue for more than six months by virtue of being in a war zone. To model such a long spill adequately requires several hundred thousand parcels and the resulting multiplicity of black dots would be confusing to say the least if concentration are not computed. The same could probably be said of the more recent BP spill in the Gulf of Mexico.

A 3D model for oil slicks and spills should consider the problem when the oil emerges at the surface or at a subsurface layer. At that moment, we believe we could retrieve the MEDSLIK-II formalism to describe the movement and transformation at this fixed water level position. We believe this will be decided in the future and we have not added more comments on this.

Appendix A is unnecessary, API <-> density is a standard conversion Appendix B seem to be a re-has of MacKay's work – I'm not sure it needs to be in this paper in this much detail. Again, this approach was state of the art over 30 years ago – I'm disappointed not to see something new. Appendix B2 is a re-hash of other MacKay work, also quite old – there are better formulations available now, and this is certainly nothing new. Appendix B3: same issues – also, with all the attention to the advection terms, using a global-to-the-slick spreading and thickness approach seems a shame – why couple the fate and transport models if you are going to do this? Appendix C: I already discussed issues with Stokes drift above.

(24) As already clarified before, Mackay is not new, but the formulas have never been re-written in a consistent mathematical formalism. Thus we have put them in the Appendix for an interested user of the model. We believe that if a person starts working with the oil spill model he or she will need to have published material to refer to. The Appendices B helps to bridge the documentation gap.

New references included in the manuscript:

Bechtel, R. D. and Wickley-Olsen, E. and Boufadel, M. C. and Weaver, J. and Barker, C., 2008 The Movement Of Oil At Sea Due To Irregular Waves. Proceedings of the International Oil Spill Conference.

Butenschön, M., Zavatarelli, M., Vichi, M., 2012. Sensitivity of a marine coupled physical biogeochemical model to time resolution, integration scheme and time splitting method. Ocean Modelling 52-53 (0), 36 – 53.

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Griffa A., 1996. Applications of stochastic particle models to oceanographic problems. In: Adler R et al. (eds) Stochastic modelling in physical oceanography, Birkhauser

Boston, pp 113–140.

Pugliese Carratelli, E., F. Dentale, and F. Reale, 2011: On the effects of wave-induced drift and dispersion in the Deepwater Horizon oil spill, in Monitoring and Modeling the Deepwater Horizon Oil Spill: A Record-Breaking Enterprise, Geophys. Monogr. Ser.,195, 197–204, doi:10.1029/2011GM001109.

Sobey, R.J. and C.H. Barker. 1997 Wave-driven Transport of Surface Oil. Journal of Coastal Research.

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