Geosci. Model Dev. Discuss., 6, C508–C517, 2013
www.geosci-model-dev-discuss.net/6/C508/2013/

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.

Anonymous Referee #3

Received and published: 7 May 2013

Review of:

M. De Dominicis, N. Pinardi, and G. Zodiatis MEDSLIK II, a Lagrangian marine oil spill model for sort-term forecasting – part I: Theory.

This paper is relatively detailed overview of the formulation of the MEDSLIK II oil spill fate and transport model. However, there is little, if anything, new in formulation of the model, so the paper serves more as technical documentation than an academic paper. The authors argue that "analytical and discreet formalism ... has not be adequately described in the literature". This may indeed be true. However, the algorithims used in particular models have, for the most part, been described in the literature, particularly

when the gray literature and technical reports are considered. In addition, I am not convinced that the authors have done a particularly good job of describing the discreet formalism – the mathematic connection between the Lagrangian particle tracking approach and the advection-diffusion equation is not really accomplished – though it's a well established technique in any case.

This reviewer feels the paper is better suited to be published as a technical report or technical documentation accompanying the model code. However, if the editors decide to publish the work, it should be made clear that it is a description of a particular implementation, mostly a derived work (i.e. little new algorithms) and not a description of the current state of the art of oil spill modeling.

Particular comments follow:

Throughout the paper, the authors should make it clear what, if anything, is new to the implementation, rather than simply a description of pre-existing work.

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/oil-spills/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.

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.

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.

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 framework is a method can can be chosen without reference to the advection-diffusion equation.

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.

The approach of modeling the oil transformations only as a function of the bulk slick properties is not universal in oil spill modeling – 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.

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 meso-

sale 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.

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.

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 understanding 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.

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).

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

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.

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.

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.

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?

Most (all?) oil spill transport models include a wind-driven transport term (usually around 3

The authors state that in the past, spill modelers used climatological currents – this may be true, but present day standard practice is to drive the oil spill transport with sophisticated circulation models – rarely simple climatology. None the less, the key point is that regardless of the model used, there are some near-surface processes that drive movement of a oil slick that is not resolved by circulation models – a wind drift factor of some sort is required. I'm not sure that the authors have provided a correct explanation of the physics involved, however.

It'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).

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 mis-applied. 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

and in:

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.

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.

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.

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 int eh 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.

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.

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.

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.

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.

@inproceedingsLehrBarkerEtal:1999, author =Lehr, William and Barker, C. H. and Simecek-Beatty, Debra, year =1999, title =New Developments in the Use of Uncertainty in Oil Spill Forecasts, booktitle =Proceedings of the Twenty-Second Arctic and Marine Oil spill Program (AMOP) Technical Seminar, organization =Environment Canada, address =Ottawa, ON, pages = 271–284

Interactive comment on Geosci. Model Dev. Discuss., 6, 1999, 2013.