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Interactive comment on “Predictions for oil slicks detected from satellite images using MyOcean forecasting data” by G. Zodiatis et al.

G. Zodiatis et al.

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Reply to Anonymous Referee #2

General Points: 1. The differences between the two approaches is outlined in an expanded first paragraph of the Discussion section. The reviewer is certainly correct in considering the second approach is the better one. 2. The examples given in the paper were part of work done under an agreement with EMSA which restricted the work to observed slicks in the Levantine Basin and the Black Sea. In the first of these areas we were able to use CYCOFOS forecasts which are of higher resolution than the MyOcean MFS regional forecasts available for this region. Since in fact most of the observed slicks were in open sea we would not expect much difference between the predictions using the different forecasts. (For example, comparisons made exam-

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ple in the case of the Lebanese spill of 2006 bear this out, pls see Coppini G., M. De Dominicis, G. Zodiatis, R. Lardner, N. Pinardi, R. Santoreli, S. Colella, F. Bignami, D. Hayes, D. Soloviev, G. Georgiou, G. Kallos (2010), “Hindcast of oil spill pollution during the Lebanon crisis in the Eastern Mediterranean,” July-August 2006. Mar. Pollut. Bull., doi:10.1016/j.marpolbul.2010.08.021.) It is only near coasts that the higher resolution forecasts are more valuable. 3. The reviewer is correct is saying that the reliability of SAR detection of slicks is a vital question, answering which would require a whole new investigation. It is doubtful at this stage whether sufficient data exist to draw any conclusions on it. In the present work we have taken the view that one must assume that the observation is a valid one and then determine if any resource may be threatened by the oil. 4. The computational time rises in proportion to the number of parcels used. We have found that for instantaneous spills of moderate size very little is gained by using more than 10,000 parcels. For spills lasting a long time this is not the case. For example if the spill goes on for 10 days and we use four time steps an hour than only about 10 parcels would be released each step. The resulting slick then appears as scattered spots. To obtain a reasonably smooth picture of the slick at least 100,000 parcels are needed in a case like this. Also with a very large spill, the number of parcels must be increased. For example, the al Ahmadi spill of 1991 was estimated as four million bbls, so with 10,000 parcels each parcel would contain 400 bbls or about 60 cu m, again not an acceptable size. It is such considerations that decide the appropriate number of parcels. 5. To include the formulae for all the fate processes would double the length of the paper. They are available in the Medslik manual and also in the forthcoming paper by De Dominicis et al (2012b) which are both referenced in this papers. A further reference to the latter paper is added at the end of Section 2. 6. Any confusion in the mind of the reader with other uses of the word ‘hindcast’ is avoided by an enlarged footnote at the beginning of Section 2. The term ‘backward simulation’ would be accurate but unnecessarily long. The term ‘hindcast’ has long been used in work on oil-spills as the antithesis of ‘forecast’.

Specific comments (note: the pages and lines may different in the corrected version

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compared to the initial submitted ms in pdf, because we had to convert the pdf file to rtf, as well we have considered in the current version the corrections/comments from the reviewer#1. However we done the modifications and corrections as suggested by the reviewer#1, page by page and line by line, based on the text itself).

P 1974 lines 2 & 3: The suggested changes has been made P 1974 line 17: This line has been left in the abstract but an explanatory sentence added at the end of Section 2 P 1974 line 25: Done as suggested p.1975 lines 2, 6, 11, 13: Done as suggested p.1975 line 14: We removed the reference to floating objects . p.1975 line 21: Done as suggested p.1976 line 3: explanations are given p.1976 lines 6, 12: Done as suggested p.1976 lines 25, 26: Corrected/modified in accordance to the comments p.1977 line 5 Done as suggested p.1977 line 17 Done as suggested p.1977 line 21 An addition has been made to explain the meaning. p.1978 line 16 Done, as suggested p.1978 line 22 See the comments under general point #6. p.1979 line 15. Innumerable experiments have been made over the past couple of decades to test the best values of the wind-drag parameters. The default values quoted are nowadays used by almost all oil spill models. p.1979 line 19 Done as suggested p.1977 line 26. We wished to make two points here. Firstly that the second-order Euler algorithm has been tested and found without question sufficiently accurate over simulations of a few days' duration. And secondly that the errors in the forecast currents and wind are so large that it is not worth using a more accurate algorithm. It was a mistake to mention computing cost since, as the reviewer points out, this is not usually a consideration these days. This phrase has been removed. p.1980 line 24. Most hydrodynamical forecasting systems are based on the shallow water approximation in which vertical advection is ignored. It would not be consistent to include it for the oil particles when it is already discounted for the water body itself. p.1981 line 14 Done as suggested p.1982 line 10, p.1983 line 26 Diffusion causes any pollutant to spread as time advances. One can see this for example from the forecasts in Figure 5. It would be perfectly feasible in theory to use the random-walk model in the backward time direction, and it is easy to predict the result: it would also generate spreading of the slick backwards in time which would

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be a completely unrealistic result. A sentence has been added at the end of Section 2. p.1984 line 4 It is unfortunately rare to have observations of a slick on two different passes of the satellite. (Perhaps this says something about the validity of the satellite observations!) But in such cases as may exist, discrepancies between the forecast and the observed slicks would say more about the inaccuracies of the forecast currents and winds than such fine features as the number of parcels in the simulation. We know this from results for ordinary oil spills as well as for drifting buoys. p.1984 line 10 Done as suggested p.1984 line 28 A change has been made in the fifth paragraph of Section 2 explaining more clearly what a tab file is. p.1985 line 20 The offending sentence has been modified to something, we hope, more acceptable. p.1986 line 9. This sentence has been modified. The request from EMSA was a single one. p.1987 line 2 Since we recognise that some readers skip straight to the Conclusions section, we have added the words 'backward simulations' to avoid their possible confusion. p.1988 line 3 We have removed the word 'regional' although the Black Sea forecasts used in Fig 8 surely count as 'regional'. We could easily have shown a simulation comparing say ALERMO and CYCOFOS forecasts but didn't wish to expand the paper beyond what would be acceptable. (See general point #2.) p.1988 line 6 Done as suggested p.1988 lines 6-18 Done as suggested p.1988 line 18-21 Its moved in the introduction. Numerous inter comparison was carried out in the past using high and lower resolution ocean forecasting data from MFS and CYCOFOS (Coppini et al. 2010). p.1988 line 22-24. Its done as suggested, reference added also to the relevant workshop organized by EMSA back in 2009. p.1989 line 1. Done as suggested p. 1995-2000. New figures 3,5,6,9 were provided following the suggestions of the reviewers#1 and #2.

Please also note the supplement to this comment:

<http://www.ocean-sci-discuss.net/9/C1236/2012/osd-9-C1236-2012-supplement.zip>

Interactive comment on Ocean Sci. Discuss., 9, 1973, 2012.

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