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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Towards a new BOLAM-MOLOCH suite for the SIMM forecasting system: implementation of an optimised configuration for the HyMeX Special Observation Periods" *by* S. Mariani et al.

Anonymous Referee #2

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General comments: This paper describes some improvement brought to the SIMM forecasting system in use in ISPRA and the assessment of the performance of the resulting configurations on two HyMeX case studies both for atmospheric and sea surge forecasts. It presents the current operational chain of ISPRA including atmospheric models, wave and surge models, as well as three different configurations tested in this study for predicting heavy precipitating events (HPE) during two HyMeX IOPs, and two different surge model configurations used during the same IOPs to predict sea level elevation in Venice. The results obtained using the different configurations for the two

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case studies are compared to in situ rain rate accumulations to estimate the performance of the models. For the atmospheric part, the HPE (accumulated rain amount) are better predicted by the more resolved and more sophisticated configurations, while the results appear rather inconclusive for the sea elevation part. Whereas the subject of the study is of real interest for improving operational forecast chains and for better capturing the atmospheric fine scale phenomena of a HPE, I have some major concerns that would prevent the publication of this paper as it is. 1- Several models and configurations (initial conditions; boundary conditions; resolution) are used in this study, both for the atmospheric (precipitation) and sea surge prediction. The presentation of these various schemes is very unclear, as descriptions of these models are interlaced with each other. The model presently in use in ISPRA is described at the beginning but not used (as far as I understand) in the rest of the study. The wave model is described in details but is used with the same configuration throughout the study. From what I understood, three different configurations are used in this study for the prediction of HPEs: a- BOLAM nested at 0.3 and 0.1° with a 0.5° ECMWF forcing; b- BOLAM-MOLOCH at 0.07°, with a 0.25° ECMWF forcing; c- BOLAM-MOLOCH limited area at 0.0225°. For the sea surge forecast, two different configurations are tested with two different resolutions, making four configurations in total: 1- SHYFEM forced by ECMWF (+same HR); 2- SHYFEM forced by BOLAM 0.1°. The description of these configurations should be significantly improved before giving further consideration to this work. The improvement should include (not comprehensively): 1- a table providing a complete and clear description of the different configurations (grid size, initial conditions, boundary conditions, domain size, father characteristics if relevant...), probably a table for the atmospheric configuration and the surge model configuration would be nice. 2- a comprehensive list of all the configurations used in this study with clear separation between the atmospheric/wave/surge models. 3- a sketch of the general processing scheme (like Fig.1, but not centred on the operational configuration, as this configuration is - as far as I understand not used in this study) and including the differences between the configurations. 2- The results on atmospheric forecast are globally

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conclusive and interesting, but insufficiently assessed and interpreted. In particular, no statistical comparison between model outputs and observations or between different model outputs is provided. This is not acceptable, provided that efficient skill scores exist and have been widely used in the community (sometimes by the authors of this paper) in the recent years. This part of the study must be complemented by a complete performance assessment including (indicative list): numbered estimates of the performance of the different configurations, based on statistical (basic) tools like BIA, HK, FAR, ETS, on several time spans for accumulated rain; figures comparing accumulated rain (models/observations) on an individual rain gauge basis (e.g. comparison of the amount recorded / modelled on a x/x plot of all rain gauges; or plot of distribution function of 24h accumulated rain wrt rain amount in mm, for the observations and the model outputs). The results themselves are presented in rather vague terms in the present version; even if the reader understands that higher-resolving and more sophisticated models perform better (which is not an amazing result) a more accurate assessment is requested here. The same results are insufficiently discussed, even is some effort to relate the observed improvements to atmospheric fine-scale phenomena is appreciated here. This part of the paper should bring some insights on simple questions like: are the changes observed only due to the finer resolution of the model itself/of the forcing/to more accurate initial conditions? if so, which is the role of the relief/resolution of the model (for instance for better capturing the low level flow)? some examples would be appreciated. What are the limitations of the present configuration and the near future improvements scheduled for this kind of configuration? Also, only the rain gauges data are used here as a ground truth. This is probably sufficient as they are numerous and of good quality (probably much better than satellite measurements, if they exist, and easy to use than radar reflectivity) but mentioning other data sources and clearly stating why they are included or not would not hurt (especially if they provide information over the sea). 3- The part on the sea surge modelling and comparison with observations is also insufficiently developed, and bringing to inconclusive results. Trying to summarize the conclusions of this part, the results would be:

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either our performance assessment gives very straightforward results like the better the resolution and forcing, the better the accuracy of the model outputs (and we do not show them, like IOP16 and IOP18 in Acqua Alta) or the results are mixed and inconclusive (and we cannot discuss or even understand them like in Venice, IOP18). So, I really wonder about the interest of including this part, provided that the discussion - or conclusion - is non existing. If this part is to be kept, some comparisons on other stations are to be added. One IOP event on one single station is not enough to conclude on the skills of two (+two) models. 4- This paper is its present version is not only rather unclear but not well balanced between presentation (state-of-the-art of modelling in ISPRA, atmosphere-wave-surge model configuration, companion work already published on the same subject, HyMeX SOP and HPE) and results. As mentioned above, the parts on the results are insufficiently developped and the results insufficiently put in perspective. Part of the information on the models and forcing is provided in the results part (beginning of 4.2, end of 41 with the comment about different initialization times for the precipitation forecast), no information is available about the data (observations) available (both for the precipitation forecast assessment and sea surge forecast assessment) and why they are retained or not. This should be added in part 2 or 3. Rather, the part 2 could be renamed "Models and data" or "Methods" and include every piece of information about the model configurations and the performance assessment strategy. Conversely, some descriptions of companion experiments (already validated) using similar configurations (p. 655 and 656) are not relevant here - they rather add to the confusion - and should be skipped.

Specific comments: Abstract: Only two configurations for the precipitation forecasting are mentioned here, while three are described and used in the study. The abstract is not clear and does not correspond to the work actually presented. Especially, the two parts of the current version (precipitation / surge) are not clearly visible here. Introduction: this part describes the recent history of model evolution in ISPRA. While probably important for weather forecasting in Italy, this could be more concise, and the introduction could rather raise some scientific questions about current and further im-

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provement of precipitation forecasting (or sea surge if relevant). Some developments (p. 652, l. 1 to 13) are not relevant here. Also, a quick summary of HyMeX and the objectives of SOP1 should be included here rather than later on in the text. Part 2: several models/configurations (including the original SIMM, as specifically named here and illustrated in Fig. 1) are described here but not used afterwards. Please focus this part on a clear description of the (only) models/configurations in use in this study, with a separation between those used for precipitation forecasting and for sea surge forecasting. Figure 1 should be changed to include all the configurations used here. The figure 2 and 3 should be merged to present all the domains used in the study (with colors). Figure 4 is not relevant and should be removed, as Figure 5. The HyMeX experiment is widely known now, including the general description paper of SOP1, and listing all the IOPs is not necessary here. A description of the data available, or their quality, and of the reasons of their rejection should be added. Part 3: the description of the two IOPs used here is fine (even if it could probably be developped on the fine scale phenomena, the role of the relief or of the feeding of the systems in heat and moisture over the sea), but synoptic charts (Fig 6) are not very informative and barely readable (contours of the continents). Part 4: 4.1: please add here a statistical assessment of the results (see above). The maps of accumulated rain are fine but could be improved - for instance by adding the values of the maximum of rain in the different model outputs/ in the observations. 4.2: as above, a (simple) statistical assessment of the performance of the model should be added here. Comparison on one station, for a few days event, is not enough. Please provide additional comparison spots (a map would be better) or consider to simply remove this part.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 649, 2014.