

Interactive comment on “Air Quality Predictions with an Analog Ensemble” by Luca Delle Monache et al.

Anonymous Referee #1

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“The paper is yet another application of the technique already presented by the authors in several other instances, there is no new scientific value in the current manuscript, and the paper barely suites GMD. Many concepts are given for granted; several are the example of the imprecise use of language. The literature selection seems incomplete considering only weather forecast and air quality as examples, US papers mainly, and the author works. All literature on atmospheric dispersion that has preceded the air quality is neglected as if there is a specificity in the application case of techniques. The classification of multi-model ensemble is awkward (multi-physics, multi-model?) while there are classifications accepted that could be instrumental to the authors. The paper has a potential but not for ACP in my view.”

This was my assessment at the “short report” level, but I am afraid by point of view has

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not changed.

The paper is poor, it is just an application of a method used for weather prediction, solar power and now air quality forecasting. The authors clearly state it stems from Delle Monache et al. (2013, DM13), improved by the method of Alesandrinni(2015) and so much so that they do not even care about explaining the methods and present the novelty that relates to this paper. Then there is a publication in AE from almost the same authors referred to as “PM2.5 analog forecast and Kalman filter post-processing for the Community Multiscale Air Quality (CMAQ) model” that also refers to Delle Monache 2013 where the analog definition is also used and that already dealt with AQ. I see nothing new here compared to those publications and original that is worth publication in ACP. ACP aims at scientific novelty and originality since model developments and applications have long been confined to GMD.

The editorial style is that of an internal report, with reference to other publications for the details and rushing to the results. Sure the results are good but what is the surprise here? that a method that works when applied to dense networks of data (in space and time) works also when the variable is not called temperature but solar radiation or ozone or PM2.5? This is not serious in my view and this paper does not fit ACP at all. It did not at the beginning, I pointed it out, I gave an opportunity to intervene and make clear what is the scientific relevance, nothing has changed, I am sorry but I must reject it.

Accepting this publication not only would give a false sense of value to the authors, which was indeed in DM13, but it is not present here, but also would take away value from a large number of scientifically original and valuable publications that are present in the journal to date. To which very demanding reviews were presented that challenge the scientific standpoints at their very essence.

This is an application of a method that has the value of yet another application. The value of demonstrating that something that was proven in the past works also for this

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case, as if one were to publish papers on the laws of gravity showing that they work to a couch, a lorry, a cow, a planet, an asteroid or the pen that sits in front of me on my desk.

The authors are strongly encouraged to rewrite it and submit it to GMD, which is as prestigious and rigorous as ACP and fitting much better the content of this paper.

Do re-write it however since the English is a bit strange at times and many concepts are rushed over like for example:

• The disputable idea that operational AQ forecast prevents deaths and societal costs. In my view, the planning does more than the operational forecasting in that respect

• Ensembles of many different kinds are discussed generically as ensembles but never presented for what they are and their differences

• Many omissions in terms of ensemble applications are present thus giving a false sense of completeness to the paper content

These are minor issues compared to the lack of originality and scientific relevance but yet they will become important if the authors opt of GMD.

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