Status of monsoon forecasting in India

Recognizing the need for mathematical modelling-enabled, forecast-centred research in dynamical long-range forecasting in India, the CSIR Centre for Mathematical Modelling and Computer Simulation (C-MMACS), Bangalore initiated an effort to bring long-range dynamical forecasting to India in early 2000. Recently, in an article in *Current Science*, Gadgil and Srinivasan[1] (hereafter GS) have referred to the C-MMACS forecasts and the methodology in a manner that is erroneous and improper.

For subsequent discussion, we note here that the C-MMACS website (www.cmmacs.ernet.in) explicitly stated the forecasts to be experimental, as against operational forecasts, with the primary purpose of an objective validation, with a disclaimer that forecasts contained their inherent uncertainties. As noted by GS, this was the only forecast available in the public domain by an Indian agency before the season, enabling an objective post-forecast evaluation; this was also the first initiative in the country to forecast monsoon at high spatial and temporal resolutions.

We first note our major objections to GS; a few comments on the methodology of GS are provided next.

(1) GS begin with a description of a single C-MMACS forecast for 2005, and mention (p. 343) that a very large deficit (of 34%) for June was predicted.

This statement is erroneous. The predicted departure for June was about 1.5 times the standard deviation quoted by GS; the departure in a year can be much more than that. Even for seasonal (June–September) all-India rainfall, the observed departure can be as much as three times the standard deviation. For July and August also the predicted anomalies were well within the observed variability quoted by GS (table 1 of GS).

(2) GS then claim (table 1, p. 344) that the errors in C-MMACS forecasts were large.

This statement is also erroneous. The observed anomalies quoted by GS do not match with the official IMD observations (Figure 1). In fact, the C-MMACS forecast of a large (–34%) anomaly for June 2005 matched well within observed anomalies, as can be seen from the advance of weekly anomalies (Figure 1); the observed deficit was –20% on 29 June (Figure 2), and much larger until 28 June 2005. GS need to explicitly mention the source of their 'observation'.

To substantiate their claim GS should compare errors in C-MMACS forecasts with corresponding errors in other forecasts worldwide, and at least for the models used by them in SPIM. We shall be happy to provide our data.

(3) GS state that the methodology for C-MMACS forecasts is not available. This statement is wrong because:

(a) GS omit publications by C-MMACS in reputed journals that provide details on methodology and model performance (for example, Goswami and Gouda[2]), although they copiously cite essentially unrelated and non-standard references (like project reports) of their own.

(b) In the initial years the first page of the forecasts provided links to other pages containing model climatology, variability, etc. until these details appeared in a number of published works[3–5]. GS should at least refer to C-MMACS publications on-line at journal websites before the date of acceptance of their article.

(4) GS then refer (p. 343) to a report of a DST directive to withdraw C-MMACS web forecasts and justify suppression of such information by citing 'confusion' created by forecasts from two agencies.

A research article is not a suitable platform for this discussion; we believe this needs a separate national debate. It will remain forever questionable whether the said 'suppression' was the best response to any claimed 'confusion'. For example, it could have been a possible alternative to clarify that while R&D institutions may post their research (experimental) results for objective evaluation (for advanced studies), only IMD's forecast was official.

(5) GS state (p. 345) that the scientists involved in generating the predictions at C-MMACS refused to participate in the SPIM project.

This statement is a wrong representation of facts. Gadgil did approach us with a proposal to join the SPIM project. I had, at that time, pointed out serious flaws and technical short comings in the methodology, and suggested a mode for our participation. My suggestions were ignored, and Gadgil insisted on first (and only) transfer of the code to their platform; GS omit reference to our offer and suggestion.

(6) GS mention (p. 343) that announcing predictions of drought based on models that have not been shown to generate reliable predictions of the monsoon is unacceptable.

This statement is true in general, but its reference to the C-MMACS forecast is unscientific and inconsistent because:

(a) All long-range monsoon forecasts worldwide have errors; the errors in C-MMACS forecasts are comparable to the best; however, the scope of C-MMACS forecasts is higher than many worldwide[2–4].

(b) GS consider a single C-MMACS forecast for 2005 to support their statement. Only in the previous paragraph they have stressed that forecasts have to be evaluated statistically; all forecasts have inherent uncertainties. Detailed comparison of C-MMACS forecasts with observations has been presented in national review meetings organized by IMD; GS make no reference to these national efforts and events, and only highlight SPIM.

(c) GS should provide a benchmark for reliability being used by them to compare forecasts from different models. For example, GS consider only grossly averaged values, such as monthly averaged rainfall. C-MMACS forecasts are at higher resolution and for a larger sample than considered by GS.

(d) The C-MMACS forecasts are communicated to and acknowledged by IMD and are also reviewed in a national meeting each year, ensuring an objective reliability assessment; GS make no reference to these national efforts.

(7) GS state (p. 344) that error has to be evaluated from retrospective forecasts.

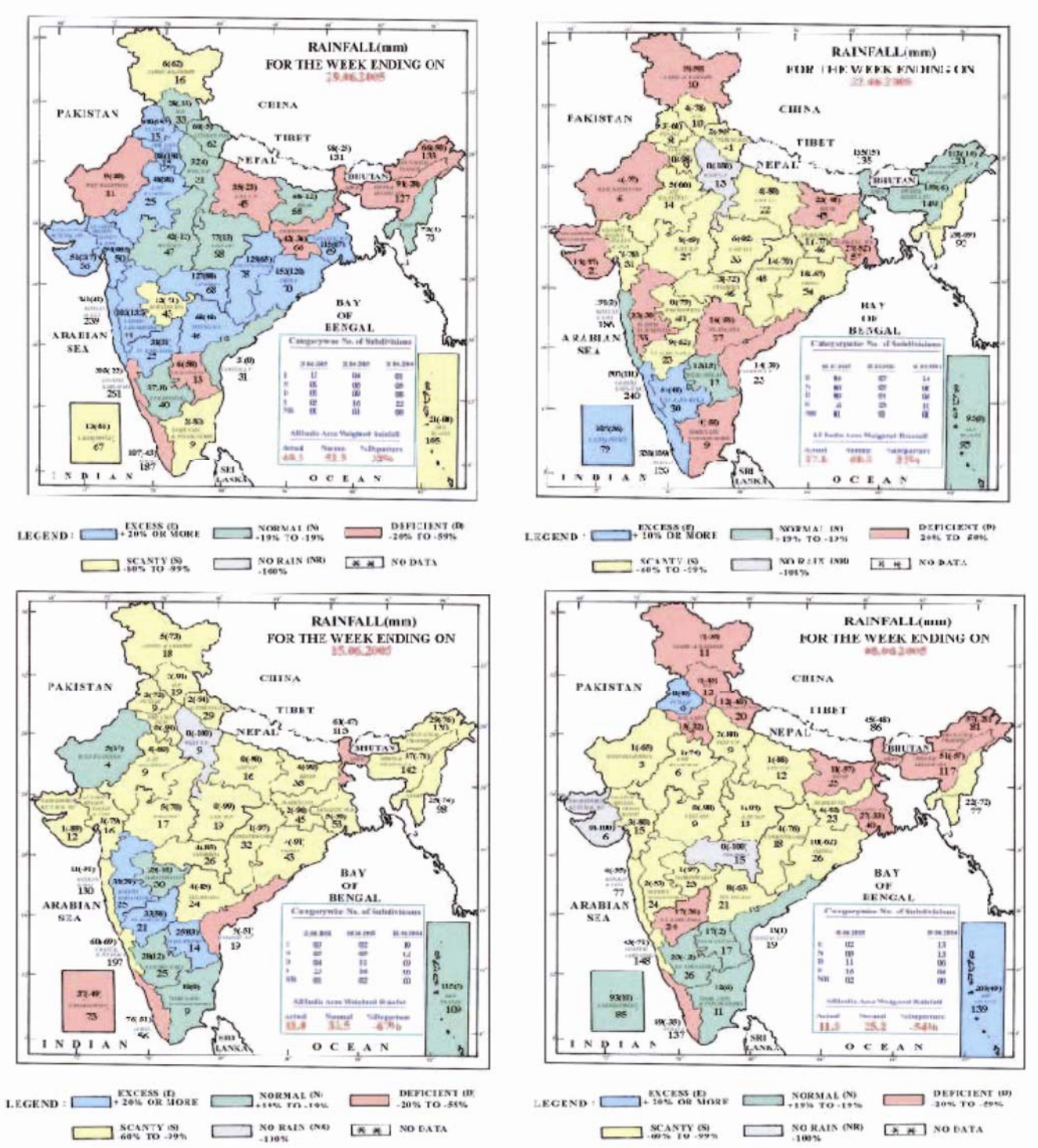

Figure 1. Observed weekly rainfall anomalies for June 2005, adapted from IMD observations (www.imd.gov.in). (Clockwise from top right) Anomalies for fourth, third, second and the first week of June.

This statement is not technically correct. The most objective way is to evaluate actual forecasts generated against subsequent observation, as was done by C-MMACS; the hindcasts are only a substitute for actual forecasts and, depending on the methodology, may not represent actual skill as explained below with reference to SPIM methodology.

(8) The AMIP methodology in GS: The concept of model inter comparison is not new, and the Atmospheric Model Inter-comparison Project has been going on for more than a decade. The basic premise of GS seems to be the use of a single computing platform to provide what they call 'a level playing ground'.

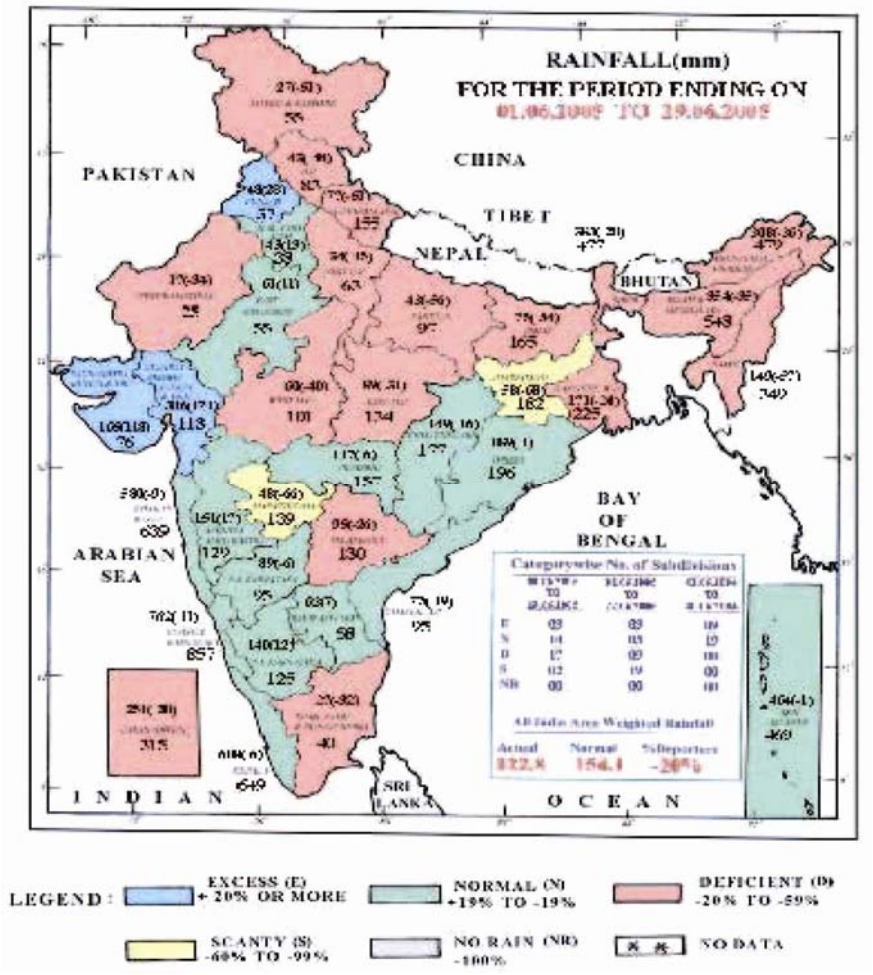

Figure 2. Observed rainfall anomalies up to 29 June 2005, adapted from IMD observations (www.imd.gov.in). The departure from mean at this stage is –20%.

However, the planning and methodology lack the following:

(a) It needs to be shown that variation of the computing platform created enough dispersion in simulations, comparable to uncertainties from other sources (such as due to change in initial conditions).

(b) Logically, it is (and is going to remain) a multi-platform world. For it to have a scientific basis, the authors would have to show that a single platform provided significantly higher forecast skill.

(c) It is clear that the models used have different resolutions (GS refer to only a project report, with no source address). It is not meaningful to use such simulations for comparative evaluation of forecast skill.

(d) The hindcasts conducted with observed SST do not represent realizable forecast skill for evaluation, for the simple reason that observed SST is not available during the period of prediction.

To ensure that the efforts put into SPIM by the participants is not a complete waste, I suggest the following simple steps (these are some of the issues that needed discussion before launching SPIM):

(i) Define evaluation parameters relevant for seasonal forecasting even if they are challenging.

(ii) Use the simulations by the models utilized in the respective platform as control experiments.

(iii) The simulations on the CDAC platform then provide the (corresponding) test experiments.

(iv) Use test–control to determine the degree of variability introduced by a change of platform; compare this variability to the one introduced by change of initial conditions (ensemble standard deviation) with respect to the selected evaluation parameters. Although the sample size is small, the results will provide some insight.

In view of many omissions and technical errors in the article by GS, it is necessary to carry out an objective, scientific and in-depth study, by an agency like IMD, of methodology and status on seasonal forecasting of monsoon in India.

1. Gadgil, S. and Srinivasan, J., *Curr. Sci.*, 2011, **100**(3), 343–353.
2. Goswami, P. and Gouda, K. C., *Mon. Weather Rev.*, 2009, **137**(9), 2893–2907.
3. Goswami, P., Patra, G. K., Gouda, K. C. and Prakash, V. S., In *Understanding and*

Forecasting of Monsoons (ed. Vinayachandran, P. N.), Daya, 2008, vol. XVIII, pp. 35–49.
4. Goswami, P. and Gouda, K. C., *Mon. Weather Rev.*, 2010, **138**(8), 3120–3141.
5. Goswami, P., Mandal, A., Upadhyaya, H. C. and Hourdin, F., *Mausam*, 2006, **57**, 111–118.

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Response:

The SPIM project whose results were reported in our article, assessed the skill of prediction of atmospheric models used in the country for generating monthly/seasonal predictions. A major comment that has been made by Goswami is with regard to the omission of references to papers from C-MMACS. It is important to note that our article was not a review of the dynamical forecasting work done in India or elsewhere, and hence only references pertinent to the theme of the article were included.

The methodology used is an internationally accepted technique of generating retrospective forecasts by running all the models with identical initial and boundary conditions for several years. Furthermore, the project envisaged that the models must be run on the same computational platform to avoid differences arising from different platforms. In fact, despite several requests, 'The scientists involved in generating the predictions at C-MMACS did not want to get their model run on the CDAC computer and hence did not participate in the SPIM project.' as reported in our article. We have shown that such an assessment of atmospheric models can lead to insight into the nature of the deficiencies and hence suggest directions for research for improvement of the models.

We are surprised at the objection to our deduction that the errors in the predictions by the C-MMACS model for June–August 2005 are large, although they are a substantial fraction of the mean. Since the C-MMACS group did not participate in the SPIM project, we cannot judge whether such large errors are typical for the model predictions. However, it is clear that work has to be done towards improving the model to generate better predictions.

A question has been raised about the observations we used. We clarify the point here. The observed anomalies quoted in table 1 of our article were from the data available on operational basis, which are also used for the weekly weather report. The monthly anomalies 2005 derived from all the data available at the end of the season are given on the IMD website as: June: –9.5%, July: +14.7% and August: –28.4%. So the predictions are even farther from the observed than indicated in table 1, with errors of –24.5%, –27.7% and 41.4% respectively.

The other comments involve an opinion about what is appropriate for inclusion in our article, such as a brief account of how the SPIM project was launched. We believe that decisions on such matters are best left to the authors and the referees. The paper was accepted for publication only after it was revised by taking into account all detailed comments by the referees on the entire paper.

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