detailed toxicity of BPA taking into consideration the *in vivo* and *in vitro* studies and enlisted some potential chemicals as alternative to BPA. In the report, the EPA's Design for the Environment (DfE) program suggests and encourages innovation and product development, when preferable alternatives are not available. This can incite innovation with design challenges and will give an insight on hazard end-point and its exposure. These efforts will help demarcate safer chemicals for which we can look forward to the field of green chemistry designs[21].

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Outbreak of dengue in Tamil Nadu, India – a rejoinder

I read the research communication by Chandran and Azeez[1]. They have used incorrect data for regression modelling. They have selectively used just two years of actual National Vector Borne Disease Control Programme (NVBDCP) data and wrong (provisional) data for 2012, while up-to-date actual data are available at the NVBDCP website (http://nvbdcp.gov.in/den-cd.html). Considering the fact that the paper was submitted for publication in 2014 to the journal, an up-to-date data should have been used. The dengue data from multiple agencies are extensively used in this paper without proper citation to the data source and date of accessing on-line data.

The authors misquote Brunkard *et al.*[2] to support their statement: 'Earlier studies have reported no specific role for climatic factors in dengue infection'. However, the conclusion of Brunkard *et al.*[2] states that 'Climate and weather factors play a small but significant role in dengue transmission in Matamoros, Mexico…'. The authors arbitrarily state that the earlier studies have reported no specific role for climatic factors, when there are several studies available proving the contrary as has been quoted by the authors themselves. For instance, Johansson *et al.*[3], unambiguously state that 'The associations between temperature, precipitation, and dengue transmission reported here are strong and consistent through time'. Moreover, elsewhere in the paper[1] it is also stated that temperature plays a role in dengue spread citing earlier studies.

The paper[1] also has several mistakes that could have been easily rectified through proper editing. When the authors state that 'Interestingly, every year, until 2011, there was 175% increase in dengue cases', a reader can find it even more 'interesting' to see that the very statement itself is false and the actual increase was around 201%, 188% and 138% as the data in figure 1, clearly show. Again on p. 173, it is stated that 'During the study period while the rainfall deficit increased, the number of reported cases of dengue decreased'. Quite the opposite trend is apparent in figure 5 and the data show that the highest number of dengue cases was reported in 2012, the most rain-deficient year. On p. 171, the paper discusses about three consecutive rain-deficit years (2011, 2012 and 2013) in Tamil Nadu, citing a newspaper article, while figure 5 shows around 25% surplus rain in 2011.

Further, there are self-contradictions at several places. On p. 173, the authors state that 'The rainfall varied significantly (ANOVA $P < 0.05$) across the seasons'; while on p. 175 they state that '…the difference between total rainfall and power supply during the four seasons in a year not being statistically significant ($P < 0.05$)...'. And 'the present study also indicates the failure of the surveillance system in 2012, while it was relatively satisfactory in 2010 and 2011'. The data provided in table 4 show exactly the opposite, with the highest accuracy (28.1%!) of the prediction figures during 2012 and much less in 2010 and 2011.

On p. 174 the authors state that 'When the predicted dengue cases were plotted against the actually reported cases for the respective years, the model exhibited significant correlation between the predicted and the actual number of cases ($r = 0.999$, $P = 0.031$)'. The overall difference between the predicted and actual number of dengue cases was also found insignificant, thereby suggesting the goodness and suitability of this model for dengue case prediction. However, on the very next page the authors state: 'The flaws in the surveillance and reporting system could be a possible, but crucial, reason for the failure of this prediction model. Thus, possibly this model emphasizes the need for accurate IDSP alert reporting through better collection, collation, compilation and validation of data.'

There is false information provided in this paper[1]. For instance, according to figure 5, there is about 150% deficit rainfall in 2012, which is obviously not correct. The dengue data given show 15,770 cases in 2012 (according to the NVBDCP data) against the actual NVBDCP data of 16,332 cases (as available in the NVBDCP website).

Based on a regression developed using just two years of real dengue data and provisional (wrong) data for the third year, the authors are reading too much into the patterns. They arbitrarily state that '....such an outbreak did not happen in 2011 and 2010, because the deficit in one of the determinant variables was counterbalanced by the surplus in the other determinant variable'. While it is an appreciable imagination by the authors with no supporting data, those factors are clearly not 'determinant factors' as the authors themselves accept on p. 175 'However, across the years, these (rainfall and power) did not correlate adequately. This signifies the role of other factors…'.

It is claimed that the study was designed to 'explore the relationships of rainfall and power supply with the dengue incidences to develop a model that can predict future possible seasonal dengue cases in Tamil Nadu and Puducherry…'. However the paper[1] does not provide any details of this 'prediction model' (regression equation) anywhere, although it discusses the accuracy and failure of this 'prediction model' without giving any clue about the regression used or its significance.

According to the figures 4 and 5 in the paper[1], only rainfall and not power is strongly correlated with dengue cases. But the authors have a different opinion and state 'overall rainfall and power supply showed significant positive correlation with the weekly IDSP reported dengue cases ($r = 0.967$, $P = 0.033$ for rainfall, and $r = 0.972$, $P = 0.028$ for power supply)'. Either these correlation coefficients or the figure can only be true; both cannot. The authors further contradict it stating '… power-cut alone was ruled out of any significant role'.

The paper[1] has committed basic flaws in interpreting the collected secondary data and the results. According to the authors, they have used the IDSP data on dengue cases as dependent variable for their regression modelling and they conclude that 'the present prediction model showed significant correlation with NVBDCP dengue cases, but not with IDSP dengue cases'. It is logically impossible and quite obviously the opposite is the fact as evident from the presented data and figures in the paper. How can one accept the authors' argument that the IDSP figures, i.e. 401, 422 and 4443 dengue cases respectively, in three consecutive years, are not correlated with the 'predicted' cases of dengue, i.e. 400, 421 and 4442 respectively, for the period? Interestingly, the entire discussion regarding the inadequacy of IDSP data is based on this wrong premise of this factual error and hence irrelevant and needs to be retracted.

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Response:

While commenting on our paper, Arun avers that incorrect and selective data have been used by us. At the outset, we wish to state that for the analysis we have used available appropriate and reliable data from authentic and responsible agencies. We did not attempt to use selected data with any bias. The agency from which we have sourced the data generally updates data of vector-borne disease incidences even after a year. Moreover, it clearly marks the numbers provided to indicate their provisional nature. It should be appreciated that the agency has the right to change the numbers at any point of time, as it gets further inputs from its sources. It may be noted that even as of 12 September 2015, dengue data (for 2014) are marked provisional (http://www.nvbdcp.gov.in/den-cd.html). It also may be noted that scientific analysis on secondary data, is always done on available data and the interpretation would be constrained by the data, which may be provisional or have limitations of data-collection protocols. As and when the data are improved, the results of the analysis could also change; the earlier conclusions would be strengthened, changed or at times refuted or dumped. We believe that this is the way science progresses.

Arun states that the paper does not cite the data source. While describing the methodology we have clearly mentioned the sources. It has been stated that our paper 'blatantly misquotes Brunkard *et al.*'. We disagree, since we have rightly quoted the study, which insightfully and commendably reports the small and significant role of climate factors, and does not specify their role in the spread of dengue. Further, it would be right to take note that relationships/associations among climate variables and other factors in dengue transmission, for that matter any such disease, are complex and dynamic. A climate variable may augment transmission potential of a disease through a specific (may be species-specific) variable, while simultaneously weakening its transmission potential through another highly dynamic situation. This intricacy should be kept in mind, especially while exploring/explaining statistical associations between vector-borne disease and climate, social or other variables. Statistical models, while to a great extent can account for the complex dynamics, at times pass over important factors of disease ecology, notably host/pathogen/vector species interactions, which may be apparently small but significant. There are several papers discussing such issues (e.g. Morin, C. W. *et al.*, *Environ. Health Perspect.*, 2013, **121**, 1264–1272). Further, in contrast to what Arun asserts, in our paper Johansson *et al.* (2009) was quoted in the introductory section to build our arguments with respect to role/relationship of the factors in dengue outbreak, and was not a misquote. We reiterate that the introduction section is a logical lead to further sections of the paper and need not necessarily conform to the results/conclusions of the same.

Regarding clarity on the dengue growth across the years under consideration, we admit that we regrettably missed the words 'an average of' before 175% in the 'Interestingly, every year, until 2011, there was 175% increase in dengue cases', as the value denotes an average of 3 years of (2008–09, 2009–10 and 2010–11) dengue growth (201%, 188% and 138%).