**Title:**

Commentary: Drop in Calorie Intake of Villagers due to stop in open defecation-IIMA Director

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The initial draft of the submission was discussed with comments on the MFC e forum and is not under consideration for publication elsewhere.

**Body:**

A very recent post in the MFC e group (Calorie intake in rural India down due to open defecation?) has gone on to question the credibility of the IIM-A Director Dr Errol D. Souza’s statement about the unintended positive impacts which have been caused due to the Swachh Bharat Campaign. The statement appeared under an article that read ( Indian Express, 2019). The programme in question here is directed at the reduction of open defecation on a pan India basis through toilet construction.

Having given the context, I would venture to present my case: explain the issue of direct rhetorical gestures, provide contextualised empirical examples, and conclude by giving possible explanations of how this could be a possibility.

1. Concluding on the newspaper report alone would be a biased estimation. Although it gives in an outline from the NSSO Factsheet where high percentages of access to open toilets have been reported. We do know, and there is enough reportage which shows that access does not apply to usage, and hence building toilets should not be taken as a proxy for utilisation (Coffey & Spears, 2014). One can certainly question the high percentages, but then state wise and district wise desegregation might reflect clustering effects which could also be correlated to the programme. This could point out perhaps that the programme’s distribution is dependent on the state’s polity.

One also needs to consider that all states did have sanitation programmes prior to 2014 before the wishing wand that Swachh Bharat is made to appear. Here I raise a fundamental question, should this outcome be thought of as a ‘secular trend’ and should the programme evaluation look into these potential selection biases. Also, one must question given the political atmosphere and the timing of the book launch as mentioned in the article. It comes from the Prime Minister’s home state; hence would one question it if it was another state or given the temporal effect was different (take any period for that matter).

2. What has been inferred is a “spill-over effect” which essentially is something which can be caused and might not have been initially designed as a programme outcome. These are of two types namely positive and negative, and it is in the theorisation that one needs to account whether it is truly positive or not. This means that the counterfactual to this could be an alternate universe where toilets are bad and open defecation is not detrimental to the health of the population. Here building toilets could have a negative ‘spill-over’ on the health of the population.

3.Regressions ( anything which you do, be it jack-knife or clustered or even linear univariate and multivariate models) often yield positive and significant results, the current cohort of scholars are the fixation on the ‘p-values’(Halsey, Curran-Everett, Vowler, & Drummond, 2015; Ramanathan, 2017) one can conclude that anything can cause anything. What is important which I often find these days is a lack of theorisation in the model. This of course has its source in the way research is sought these days (Tharyan & Thomas, 2018)

In its absence, anything can explain anything (perhaps quarks and cosmic rays could explain open defecation, and solar flares explain childbirth). Directionality also needs a fine eye.

I have not read the book which was launched at the mentioned event, but then it should allow for the ‘theory of change’. Only then could one model the outcomes of the mission and explain it in empirical terms. The challenge that comes here is that often we miss it as researchers and fixate on the ‘method’ and not the ‘’methodology’, Quantitative thinking is no higher ground other than allowing an explanation of reality.

This brings the next; that we often do limit ourselves to the thought that correlation is not causality. A step further would be that the causality in the social sciences is a contributory causality and not a causative causality, unlike the physical sciences. Even this is being questioned now for physical constants. They too are having consistent values as measurements become more precise. The limits of explanation need to be considered; it is not an absolute but a plausible cause (Kononogov & Mel’nikov, 2005.).

4. A similar challenge occurred in the sensationalisation by major newspapers of Albendazole as a ‘magic bullet’ for education uptakes in Kenya (“Explainer: Where were you in the #wormwars? | Global Development Professionals Network | The Guardian,” 2015.). This seminal work by Miguel & Kremer, (2003); shows in the referenced paper that the study was well designed for reporting the intended effects (here schooling was an intended effect). The entire deworming programme was based on it. What one also needs to see is that if the ‘theory of change ‘reflects it. If it does the empirical justification is only an addition. In the example of the sanitation programme, since it is not originally intended in the design, the spill-makes the outcome questionable unless empirically shown.

There are a lot of similar examples, but then a very recent work contextualised to open defecation in India is that of Diane Coffey and Dean Spears(Coffey et al., 2014).

The authors (Diane Coffey, 2018) have shown that a reduction in open defecation is correlated to stunting and theorised it as well. They have shown this as a positive spill-over effect and positioned it from the NFHS 4 data and also commented on the iceberg effect of ‘stunting’ as a variable. Their book (Coffey & Spears, 2016.) builds a good outline for the determinants of open defecation. However, my contestation is such secondary analysis of large macro level data cannot explain the micro or meso level outcomes. What also needs to be seen is the equity aspect and if it is provided for in such evaluation. This of course is not under the scope of the current commentary.

There is a need for supplementation by evidence derived at all levels( macro, meso and micro) as well. One also needs to be seeing the fact that there has been a gradual decrease of caloric intake with income in India (Subramanian & Deaton, 2002) this too can be questioned from the macro inference). Thus effectively rural populations do have lesser incomes which reduces their caloric intake. The direction of this causality could show how it makes a difference if toilet usage is accounted for or does it change at all.

What is also needed is that questioning does usage of toilets show the hypothesized outcome of rural areas has had a reduction in malnutrition as compared to the pre-programme period.

To conclude dissecting the evidence is needed. Comments and critiques are welcome. Except where referenced the remainder are the authors own words and comments.

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