

# Review of: "Did Vulture Decline Increase Death Rates in India?"

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**Potential competing interests:** No potential competing interests to declare.

## Peer review of: "Commentary: did vulture decline increase death rates in India?"

Dear Akshay, Sayali, Sajal, Premsagar, Sonal, and Milind,

Thank you for the opportunity to read and respond to your commentary on the paper by Eyal Frank and Anant Sudarshan entitled, "The social costs of keystone species collapse: evidence from the decline of vultures in India". I found both the original paper and your commentary very engaging and interesting, with many important points raised.

I will focus my suggestions on your commentary and what might strengthen it, but this at times includes reflections on the original paper.

Frank & Sudarshan seek to test the hypothesis that increased veterinary use of diclofenac in India, in particular amongst cattle, led to a precipitous decline in the population of vultures as a consequence of them eating 'contaminated' meat as those cattle died, and that this in turn led to an increase in rotting cattle which had impacts on health through contamination of water sources and increases in other animals carrying health risks such as feral dogs. They test this hypothesis by comparing crude (as in not age-standardised) all-cause mortality rates in areas classified as being highly suitable for vultures with areas with high baseline livestock, to areas with low suitability for vultures and low baseline livestock. Difference in Differences (DiD) and Difference in Difference in Difference (DiDiD) are employed using a panel dataset with these explanatory variables of vulture suitability and livestock size, with a shock introduced over time with the increase in veterinary use of diclofenac. As such, the paper seeks to make use of a 'natural experiment' of the introduction of diclofenac to infer the mortality consequences of the reduction in vultures on mortality and to estimate a financial 'cost' of that additional mortality.

Your commentary casts doubt on the validity of the Frank & Sudarshan paper for a number of reasons:

1. Mortality data reliability. You draw attention to the potential for the mortality data used in the original analysis to undercount deaths, for this undercounting to be uneven across regions, and that increased (improved) counting over time may have been concentrated in those areas with historically worse ascertainment. As a consequence, you propose that this may have biased the findings because any/all of these miscounting differences may have patterned in a similar way to the vulture and livestock datasets, thus biasing the results. Here there are further contributions that I think you could usefully make.

- a. Other sources of mortality data are available – for example, the Global Burden of Disease study and the World Health Organisation (it may also be worth checking the Human Mortality Database to see if they cover India). Checking whether these are based on the same underlying data, or whether they have made different assumptions and adjustments for undercounting, would be useful.
  - b. The biggest problem I can see with this data is not mentioned as a limitation in the original paper, nor in your commentary, and that is that the mortality data are crude rates (i.e., not standardised for changes in the underlying age structure of the population). This is problematic in any trend analysis, and particularly where different population structures across regions are possible or likely. If, for example, the areas with the greatest reductions in vultures are also the areas with younger/older populations (and therefore systematically lower/higher crude mortality rates), and if one set of areas is ageing faster than the other, this has the potential for quite large, but artefactual, impacts.
  - c. For each of these potential sources of bias – estimating the direction of the bias (i.e., towards or away from the null), and the likely size of this effect, would be helpful.
  - d. In general, the original paper and the commentary would be easier to interpret if the overall trends were provided in the key parameters (mortality outcomes, exposure variables, confounders) for each region over time in absolute terms rather than only summarised as relative differences or differences in differences. It isn't clear to me whether overall trends in crude mortality are increasing, decreasing, diverging across regions, or converging. This is important context to understanding the argument.
2. Reliability of data on vulture and livestock populations. Here you rightly call into question the reliability of the data sources. Frank & Sudarshan, and your team, all go to some efforts to triangulate across what are imperfect data sources to try to test whether using different data might impact the results. When only imperfect data are available, as is the case here, sensitivity analyses to test the effect of different underlying estimates is appropriate. Further, looking at each of the sources of bias and testing the direction and size of the impacts (as per 1c above) would be appropriate to see whether this makes the conclusions less certain, and in what direction the results might shift if these biases could be accounted for. I do agree that the data and methods used to calculate vulture suitability by region is a major limitation in the original paper.
  3. I didn't find the argument convincing that because many districts with residual populations of breeding vultures were labelled as unsuitable for vultures, nor the anecdotal evidence of abundant nesting in low vulture suitability areas, undermines the vulture suitability dataset. Clearly, the regional estimates are averages and will contain a lot of smaller areas with substantial misclassification. You also rightly note (although using different language) the Modifiable Areal Unit Problem, whereby the boundaries could be drawn differently to realise different results. I think it is reasonable to question the validity of the vulture data, but the anecdotal evidence you present is not a strong basis from which to do so.
  4. Rabies and waterborne disease trends. You argue here that the available data show declining trends, which contradicts the putative mechanisms offered by Frank & Sudarshan. This could be relevant, but might not be, and so I think is a less convincing argument against the approach taken for three reasons:

- a. Overall trends don't need to decline for the vulture hypothesis to be true – only that the trends in the impacted areas diverge from the unaffected areas. Thus, the average trend isn't that helpful.
  - b. The reliability of non-fatal conditions is highly likely to be less accurate than mortality data. Thus, given your critique of the mortality data, you would need to do more to convince me that the data here are more reliable if these are to be used to argue against the validity of the vulture hypothesis.
  - c. Finally, although demonstrating mechanisms can be important in convincing people that something is a cause, it isn't necessary. For example, we didn't need to know the details of germ theory for John Snow to realise that the water pump in Broad Street, Soho, London, was the 'cause' of typhoid.
5. Alternative causal explanations. This is an interesting point, but a difficult one to put into practice here. Simply put, the authors did not set out to test the different potential causes of higher mortality in some parts of India, nor the potential causes of the decline in vultures. Instead, they are using the decline in vultures to evaluate the impact of their loss on one outcome. Instead, what would be a more helpful critique to make here is to invoke the ways in which people find evidence persuasive. Perhaps utilising the Bradford-Hill criteria, or the various expansions and updates of them, to examine critically how likely any difference in mortality outcomes has been caused by the loss of vultures. However, common practice is often to do this across a number of studies addressing the same epidemiological question, rather than in a single study which will often only provide a single test/method.

If I understand it correctly, the new analysis you present is interested in testing problem 1 above, i.e., whether better mortality recording over time occurred and whether it occurred disproportionately in the areas categorised as having greater vulture suitability, and that this is responsible for the relative increase in crude mortality over time. You seek to test this by looking for changes in the extent of variation in crude mortality over time. I confess to finding the logic here very difficult to follow, as I wasn't clear if the areas with greater vulture suitability had higher or lower crude mortality at baseline, and thus whether an increase in the crude mortality in those areas would increase or decrease variation. This in part reflects the lack of descriptive data on the actual levels (not just relative differences) in crude mortality trends provided in the original paper or the commentary from which to understand what impact better mortality ascertainment would make over time. I think it would help if you could explain this more, perhaps with the help of diagrams showing the crude mortality trends in the different categories of regions, and then whether you would expect the narrowing or widening of this gap in the case of either changed mortality ascertainment or changed mortality trends due to the vulture-loss mechanisms.

The original paper, and your updated analysis, could usefully increase the replicability of the analysis by making all the data publicly available and all the analytical code available on an open-access repository. Coding errors are easy to make, as are data errors. Sometimes this level of transparency and sharing can help science, and there is no shame in simple errors creeping in if we are transparent.

One issue that you do not critique in the original paper is the ethical implications of valuing a species only in terms of the impacts it has on human life, and indeed the financialisation of human (and other species') lives. Even more fundamentally, the original paper has to work hard to turn the problem of vulture loss into a scarcity problem and one

amenable to a standard economic problem. Although it is highly commendable that Frank & Sudarshan recognise (some of) the system problems that may have both caused vulture loss and which result from vulture loss, I find the idea of creating a 'targeting problem' of whether conservation policy is financially justified entirely misconceived. This may or may not be an issue that you wish to address in your commentary, but the idea that this 'problem' is solvable by targeted conservation efforts is barely credible in an era of mass extinction, and there is a risk that this framing does more harm than good.

As a minor issue, I'd remove the phrase 'compete fiercely with each other for unreliability', and I'd refer to the authors consistently by their full names throughout to avoid unnecessary distraction or inadvertent disrespect. There is also a typographical error here: "The suitability maps shown in figure 2 of the paper do not agree quite well with...".

Figure 2 could usefully have the axes labelled, the axes aligned across the 4 plots (and scales harmonised). The R squared values provided in Figure 2 exhibit spurious precision and should be given to only two decimal places.

Finally, I'd encourage you to structure your commentary more clearly, and perhaps tabulate those aspects of the original paper you agree and disagree with. I found it a little difficult to follow in places as the logic jumped about a lot.

Thank you for the opportunity to read and review your commentary. I hope these comments are helpful in strengthening your arguments. Best of luck with publication.