March 26, 2019

Dear Dr. Brook,

We thank you for the opportunity to revise our MS and the three anonymous reviewers for their constructive comments. We have gone through the comments of the reviewers and generally agree with their recommendations. We provide a pointwise response on how we have addressed each of their concerns and suggestions in the revised MS.

To provide some perspective on this paper, we give the background of our efforts to address these concerns with the MEE that originally published the paper we critique i.e. Gopalaswamy et al. 2015 and our decision to submit our MS to Peer J. On noticing the anomalies in Gopalaswamy et al, highlighted in our current MS, we asked the authors for clarification regarding the data of their Fig 5 (which is the lynchpin of the paper since it provides the critical test of their theoretical model to field data, i.e. there is no correlation between tiger abundance and index of tiger abundance). Since at that time there was no supplementary data provided with the online paper and the reference cited under the figure was incomplete (as it covered only the sites in Maharashtra). The authors referred us to their published work from which they had sourced the data used in Fig 5. Arjun Gopalaswamy responded to our query "In regard to the tiger application, once again, *no raw data were used – only estimates from published resources have been utilized*, which are cited" (but were clearly not cited). Since we were familiar with tiger work in India, we painstakingly reconstructed the data from their published reports and papers and found serious discrepancies between a) the difference between the published values in the source literature and those used in Fig 5 differed in three out of seven data points and b) the time frame of camera trap based density estimation and the collection of scat index differed at three out of seven sites by two to seven years. This we found especially egregious, since the authors ought to have known that for any index calibration experiment collection of index and abundance values must be contemporaneous and within the same spatial extent. At this point we felt that at least the empirical validation of Gopalaswamy et al's (2015) arguments was grievously flawed. Being field ecologists, we were deeply disappointed to see such disregard to field experiment protocols and on the basis of which they aimed to refute our carefully conducted field experiment (Jhala et al 2011).

Subsequently, we notified the MEE editors of these issues on April 7, 2015. Our comments broadly addressed ecological concepts and empirical data used by Gopalaswamy et al, while serious criticism by Dr. Murray Efford were raised independently on the statistical formulation of Gopalaswamy et al's theoretical models. On May 22, 2015 we received a response from MEE informing us of the need to ask Gopalaswamy et al to respond to our

Commented [??1]: This background, after completing the incomplete information, should be part of the main manuscript. This will make the authors scientifically responsible for what they claim.

Commented [??2]: This is technically incorrect. The theory developed in Gopalaswamy et al. (2015a,b) requires NO data for validation.

Commented [??3]: Note, that Jhala et al. (2015) also convincingly demonstrates this. To put it simply, one can discard the whole dataset being critiqued by the authors (IC-Karanth: used in Gopalaswamy et al. (2015a,b)) and replaced with Jhala et al. (2015) and you will still get the same results.

Commented [??4]: At this point, I would encourage the authors to share the entire email correspondence (should this be the approach taken by the authors instead of the preferred option of scientific arguments), instead of pulling out convenient snippets to make their case. During this correspondence, there was also an offer made by one of the authors, Dr. Karanth, of Gopalaswamy et al. (2015a,b) to share all raw data used in Gopalaswamy et al. (2015a,b) and more with authors of Qureshi et al. (2018) with an intention of writing up a joint paper. This was specially relevant at that time because Jhala et al. (2011)(JAE paper) had selectively left out 8 data points in south India (see Jhala et al. 2011 – national tiger assessment report). Given that this was the only region with comparative datasets such a joint publication would have helped settle the scientific debate about index-calibration, with respect to tigers. The authors of Qureshi et al. (2018) ignored this offer then.

Commented [??5]: Wouldn't it have been easier for the authors to take up the offer made by Dr. Karanth, and constructively work on a joint paper, instead of going through this 'painstaking' effort? Therefore, it will be relevant for the authors to mention why they ignored this offer.

Commented [??6]: Note: Now Jhala et al. (2015) also refutes Jhala et al. (2011). Also, as discussed in the review Gopalaswamy (2019), Jhala et al. (2011) selectively leave out about 8 data points of south India (see Jhala et al. 2011 – national tiger assessment report). So, it will be misleading to state that this was a carefully designed experiment. The larger question that arises from this claim is this: if such a perfect result was obtained from Jhala et al. (2011), why was there a need to do anything else after that? Specifically ... [1]

Commented [??7]: Notified will be too mild a word to describe what the authors actually did. In fact, the authors summarily DEMANDED the retraction of Gopalaswamy et al. (2015a) from MEE (see Vishnoi 2015, Kempf 2016) without any valid scientific reason and severely pressurized the journal. There is no reason why PeerJ will not face the same ... [2]

Commented [??8]: It will be helpful if the authors can share THE VERY SAME note here for peer-review.

Commented [??9]: It will be important for the authors to share what Dr. Murray Efford wrote to the journal and EXPLAIN WHY AND WHAT they agree with Dr. Murray Efford's statistics. This will further help in the peer-review process.

objections, and asked us if we wished to reveal our identity to Gopalaswamy et al or not. We heard again from MEE on June 12, 2015, wherein we were informed that the authors (Gopalaswamy et al) were about to file a response to our objections and that these would be reviewed by two subject expert editors from MEE. Meanwhile a corrigendum to Gopalaswamy et al would be published, which the editorial office of MEE clarified "The corrigendum deals with a specific error within the paper (identified by Murray Efford). As the article is available on line, we feel that it is necessary to correct this error as quickly as possible. A senior editor has read and approved the corrigendum as a separate issue from the ongoing assessment." On June 19, 2015 we noticed a new 8th supplement to Goplaswamy et al. appear online, this supplement now contained the data used in Fig 5. When we questioned MEE about it we were informed that though "the supplement should have been published in February along with the paper, this was not done due to errors at Wiley ….. The file was submitted by the authors with their article and was available to the Editors and Reviewers. There has been no changes to any of the supporting information files since this article was accepted. All are available in the same form as they were in the final section of the peer review process." However, the foot note in the supplement 8 mentions the Tadoba data point discrepancy (error reported by us) – which contradicted Arjun Gopalaswamy's email clarification to us earlier, that only published information was used and no raw data were analysed. Also, if the supplement was submitted with the paper it would have been cited in the original paper (which was not the case). Due to these contradictions in MEE's explanations and facts, we checked the metadata of the supplement 8 and found that it was edited by Dr. Arjun Gopalaswamy on April 28, 2015, much after our complaint to MEE ! On bringing this to the notice of MEE editors, they acknowledged that indeed the supplement 8 was edited much after our complaint. The other two data errors that we had not mentioned in our letter of April 7, 2015 to MEE (Pench Maharashtra and Melghat) were not corrected in the supplement – and still remain wrongly portrayed in Fig 5. of Goplawamy et al 2015 (we mentioned these in our correspondence to MEE on June 19 after the supplement 8 appeared online; these discrepancies have also been pointed out independently by Peer J reviewer 1). The corrigendum to Gopalaswamy et al. corrected some mathematical errors pointed out by Dr. Efford and contrary to the assurance given to us by MEE, addressed the incorrect Tadoba data (pointed out by us) by referring to the supplement 8 (which did not exist earlier). It appeared like Gopalaswamy et al were building a ship of Theseus, replacing one leaky plank after another. In August 2015, MEE informed us of their review's conclusion "The reviewers found the methods that are discussed in the article (read Gopalaswamy et al) to be sound" and no further action was required based on our complaint. The review was not shared with us by MEE, nor were we informed on how

Commented [??10]: This entire section about the unprofessional handling of the issue by MEE should be written in the main part of the manuscript and take full responsibility for all these charges levelled against MEE. MEE is a very reputed peer-reviewed journal and a journal of the British Ecological Society. They put in place a very elaborate system (guided by the COPE guidelines) to deal with complaints such as these. Sure enough, MEE put in place a long procedure to thorough, investigative, review Gopalaswamy et al. (2015a,b) all over again from the standpoint of the statistical theory development and its application to tiger data, whilst maintaining their right to retract Gopalaswamy et al. (2015a,b). This procedure lasted a good period of 5 months before MEE made their decision. The authors are implying here is that MEE played dodgy politics with them. If they firmly believe this to be true, then they MUST write this background in the main manuscript of the paper to provide the motivation for their current study.

Commented [??11]: Again, more charges are levelled against MEE. All of this must move to the main text if the authors believe this to be true.

Commented [??12]: If this is the hypothesis of their submission, this too should be written in the main part of the manuscript.

our objections were found to be irrelevant. MEE invited us to submit a rebuttal through a peer review process. Given the above experience, and rejection by MEE of a rebuttal submitted by Murray Efford to Gopalaswamy et al 2015 addressing statistical flaws in their models (which we were privy to) we did not pursue the matter further with MEE. However, the authors of Gopalaswamy et al still persist to discredit our work and consequently India's conservation effort at conserving tigers despite tremendous challenges. We therefore, wrote this paper and hoped for a fair review with Peer J, which we are happy to note that we did get. The above long-winded chronology was, we believe essential, to bring into perspective our motivation and "emotive" language of our original submission. We agree with the reviewers that we do not need to be emotive to make our case, and have revised the language and MS as suggested by the reviewers.

Dr. Arjun Gopalaswamy has provided comments as a reviewer to our paper, which is more like a rejoinder and is already posted as a comment to our paper on Peer J preprint section. With the revised consideration of our article as a research paper, we believe that we are no longer obliged to respond to Dr. Arjun Gopalaswamy's rejoinder as a review of our paper, which in any case has not addressed our central concerns. Although, we do take cognisance of some of the relevant points he mentions, to bring in better clarity in our communication.

Our MS is a critique on the inappropriateness of the data used by Gopalaswamy et al. 2015 and their subsequent conclusions based on the six issues pointed out in our MS. We do not wish to engage on aspects of the mathematical modelling done by Gopalaswamy et al. 2015 or a debate on the use of indices for population abundance estimation in this paper. We revise the MS as mentioned below in this context and hope that this will encourage others to look further into the models and their formulations presented by Gopalaswamy et al (2015).

With Best Wishes.

Y. V. Jhala on behalf of all authors.

Response to specific comments of the reviewers:

Reviewer 1 (Anonymous)

Basic reporting

I few references provided in the text need to be clarified:

Commented [??13]: The authors must correct the order in which these events have occurred. Firstly, right when the authors of Qureshi et al. (2018) demanded retraction from MEE, MEE offered the authors a chance to write a rebuttal to Gopalaswamy et al. (2015a,b). They refused this offer because they wanted something bigger (a complete retraction of Gopalaswamy et al. (2015a,b)). Hence, MEE considered their request as a 'complaint' rather than as a formal scientific rebuttal. Yet, after MEE went through the whole procedure of re-evaluating Gopalaswamy et al. (2015a,b) and came to the conclusion that they did not find any evidence of scientific malpractice, they gave YET ANOTHER opportunity to the authors of Qureshi et al. (2018) to write a formal Forum article, not as a rebuttal anymore (because the authors refused this opportunity earlier), but as an original article that would be peer-reviewed as usual. The authors of Qureshi et al. (2018) refused this offer too. In general, the authors of Qureshi et al. (2018) refused TWO offers made by MEE to publish their critique about Gopalaswamy et al. (2015a,b).

Commented [??14]: The authors MUST write all of this in the main manuscript if they seriously believed MEE and the whole peer-reviewed system is flawed. If the authors were privy to this correspondence between MEE and Dr. Murray Efford, they MUST share this correspondence here for peer-review. And, in fact, state this fact in the main ... [3]

Commented [??15]: Does this mean that if the outcome is not favourable to the authors, will PeerJ have to undergo the same ordeal that MEE had to undergo?

Commented [??16]: The authors have already publicized the original submission (Qureshi et al. (2018)), with all the 'emotive' language, on Twitter, as though it is already a peer-reviewed publication. So in terms of the objective of discrediting the authors of Gopalaswamy et al. (2015a,b) they have already achieved that objective. Now ... [4]

Commented [??17]: The review provided by Dr. Arjun Gopalaswamy is NOT a rejoinder but it is an invited review by PeerJ. It only happens to be made available to the public, just like the non-peer-reviewed article (Qureshi et al. (2018)). Also, Qureshi et al. (2018) isn't really an original scientific publication. Rather think it is merely a collection of ... [5]

Commented [??18]: The presumptive nature of this remark and the authors' haste is disturbing. It was well known to the authors of Qureshi et al. (2018) that the lead author of the Gopalaswamy et al. (2015a,b) paper will be one of the reviewers of this submission even before submission since this is exclusively a critique of ... [6]

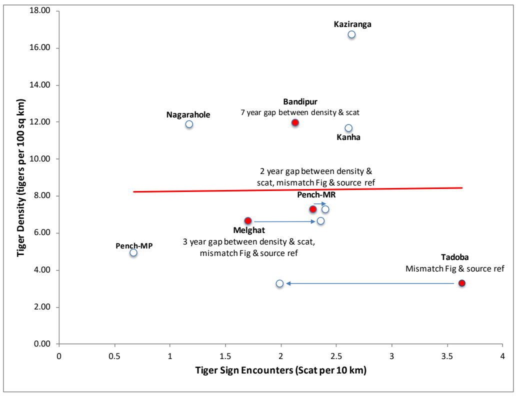
Commented [??19]: It will be important to mention that Jhala et al. (2015) also now refutes findings of Jhala et al. (2011).

Commented [??20]: This is not true. They have engaged in criticism of the statistical development of Gopalaswamy et al. (2015a,b) and seem to have added to all the confusions in by not recognizing the Royle and Nichols (2003) identity. Therefore, I would suggest they remove ... [7]

Commented [??21]: It will be helpful to all if the authors first demonstrate their own understanding of the Gopalaswamy et al. (2015a,b) paper before hoping for others to look into this.

Line 65: “…Jhala et al. (2011)…” is it a or b? Clarify please.
Response : Addressed.
Lines 119-120: “…Karanth et al. (2011)…” which one a or b? Clarify please.
Response : Addressed
Lines 127-128: “…Karanth et al. (2011)…” which one a or b? Clarify please.
Response : Addressed

Some important information is missing in the supplementary material. This would also ease the work of the reviewers. Please add the information regarding abundance estimates by means of photographic capture-recapture for Pench-MP, Nagarahole, Kaziranga, Kanha and Tadoba in the suppl. material. Same comment for the encounter rate for Pench-MP, Nagarahole, Bandipur (the value provided in Fig. 1 of the present manuscript does not correspond to the value provided in the suppl. material; see also my comment Figure 1; clarify please), Kaziranga and Kanha.
Response: We thank the reviewer for this thorough scrutiny. We have added a table in the supplementary material with all the information requested. The data represented in Fig. 1. of our MS is a reproduction of the Fig. 5. Of Goplaswamy et al. 2015 paper. The mis-match of the reference data and the Fig 1 of our MS are due to these errors in the original Fig 5 of Goplaswamy et al. 2015 which are reflected as per the published Fig 5. In Gopalaswamy et. al. 2015 fig 5, the referenced data value on scat encountered for 10 km are not the same for three sites (Tadoba reference data value 1.99, Fig 5 data value 3.63 {subsequently corrected through a corrigendum to the main paper (after we registered a complaint with MEE) saying that the original reference was incorrect, the other two data points not mentioned in our complaint remain unaddressed in Gopalaswamy et al Fig 5}; Pench Maharashtra reference data value 2.4, Fig 5 data value 2.29; Melghat reference data value 2.36, fig 5 data value 1.7. We have modified our Fig 1 to show these discrepancies and avoid confusion.
Revised Fig 1.



Experimental design

Although "experiments" in Bandipur, Melghat and Pench-MR have lags of several years between estimating tiger densities and tiger signs and hence should not be included in the experiment/graph (e.g. Figure 1), they do furthermore not match the original source. Clarify please and correct where necessary.
Response: We thank the reviewer for pointing out this discrepancy in our Fig 1 which was an exact reproduction of Fig 5 of Gopalaswamy et al. This is rectified as mentioned above and in the revised Fig 1 of our MS.

Validity of the findings

The manuscript does not answer if double sampling to establish relationship between any index of abundance and actual abundance is a worthwhile effort. The argumentation would gain in strength if the theoretical model e.g. developed by Gopalaswamy et al. (2015) would be parameterized using detection probability of tiger signs instead of detection probabilities to predict the R2 statistic based on the theoretical models and if data that demonstrate that there is in fact a strong positive relationship between tiger abundance and scat index would be presented. I encourage to include these aspects in the present manuscript.
Response: Although we agree with the reviewer that reparameterization would enhance the scope of this paper, we do not wish to address this

question in the current MS. Here, we merely want to point out the problems related to data and ecological concepts in the published paper by Gopalaswamy et al. 2015 that has often been cited as if it is the final verdict on the use of indices and double sampling for abundance estimation by reputed authors (see Darimont et al 2018, Cons. Biol., Hayward et al. 2015, J. Appl. Ecol. amongst others). The purpose of our paper is to put this information out in the public domain for a better informed opinion on this issue related to this publication. The literature on use of calibrated indices for abundance estimation is vast (e.g. Skalski et al 2005; Conroy & Carrol 2019) and we will not be in a position to address this here or present new evidence in support of indices of abundance with additional data or analysis.

Commented [??22]: The authors can show that in Jhala et al. (2015) the relationship fails at macro-scales.

Comments for the Author

Major comments

Lines 125-127: The detection probability varies with the size of the sample unit (e.g. 225 km2 in Barber-Meyer et al. 2012; 166 km2 in Harihar and Pandav 2012; 188 km2 in Karanth et al. 2011a) and the definition of the sampling occasion (e.g. Barber-Meyer et al. maximum of 40 km was surveyed per grid cell with each contiguous 1-km segment considered a 'spatial replicate'; according to Harihar and Pandav maximum survey distance as 40 km, the data were recorded along segments of 250 m as either detected or not-detected, detection histories were constructed for each cell by aggregating signs along 250 m segments at 1 km to form 'replicates; Karanth et al. 2011b fixed the total sampling effort (distance walked) in a cell with 100%habitat at 40 km, each type of sign detection was assigned only once to each 100-m trail segment, thus yielding the standard '1' (detection) or '0' (nondetection) histories required for occupancy analyses. These sign detection data were aggregated at 1 km length to form 'spatial replicates') considered in a given survey/study. Hence when attempting to compare detection probability estimates across multiple studies it is usually necessary to make sure that the per occasion sampling effort is standardised across sampling areas. Please make sure that the detection probabilities are comparable. If not please adjust them according to the procedure described e.g. in Gopalaswamy et al. (2015).

Response: Although we agree with the reviewer, in our paper we merely

point out the selective nature of literature from which Gopalaswamy et al 2015 picked values for parameterizing their models. However, both Harihar et al and Barber-Meyer et al use the same methodology and survey design as Karantha et al 2011 and report detection probabilities at 1 km length segments (same as what is used by Gopalaswamy et al). Both publications along with the relevant sections highlighted are attached for the reviewer to see. It may be relevant to point out here that Harihar et al got higer detection rates than what was used by Gopalaswamy et al. even from an area where only two tigers were know to be present and Barber-Meyer comment in their paper on the high detection rates they observed compared to Karanth et al 2011.

We do not compare or derive any mathematical formulation from these occupancy probabilities in our paper. We also point out in our MS that use of occupancy detection probability to parameterise detection probability of scat encounters is inappropriate as these probabilities address entirely two different phenomena.

Commented [??23]: The authors to make some statistical arguments in Qureshi et al. (2018-V1) which are essentially incorrect or incomplete.

Line 150: Scats were sampled in 2006 in Bandipur according to the information provided in the supplementary material. Correct please.

Response: Corrected. We thank the reviewer for pointing this oversight.

Lines 177-181: It does not make sense to calculate a coefficient of determination with only 4 points (e.g. Figure 1 in the manuscript). Ideally there should be at least 10 points. More over only three data points do not qualify and there is no reason not to include the value provided in the original source (namely 1.99/10 km) of the extreme outlier at the right corner of figure 5 in the calculation of the coefficient of determination! If this value is included the coefficient of determination will be lower (athough the sample size is still too low). Although "experiment" in Bandipur, Melghat and Pench-MR have lags of several years between estimating tiger densities and tiger signs and hence should not be included in the experiment/graph, they do furthermore not match the original source. Clarify and adjust the values where necessary (see my comment Figure 1).

Response: We agree with the reviewers assessment (as well as that of other reviewers regarding this aspect) and have removed the R2 computation in the revised MS. We have clarified and corrected the data discrepancy issue of Fig 1.

The argumentation would gain in strength if it would be possible to demonstrate that there is in fact a strong positive relationship between tiger abundance and scat index. Ideally data (abundance and index collected contemporaneously and over the same spatial extent) collected according to the same design and in the same landscapes (as it is possible that scat detectability varies from one landscape to the other) showing that there is in fact a strong positive relationship between tiger sign index and tiger abundance should be presented (see also my comment "validity of the finding").

Response: We encourage the reviewer to see our paper Jhala et al 2011 in J. Appl Ecol. wherein we have collected data adhering exactly to the same experimental design as suggested above (contemporaneous estimation of index and density, same spatial extent, and the same landscape). While the Fig. 5 of Gopalaswamy et al, representing the field verification experiment of IC-Karanth, has only 8 data points and that too a mixture from three different landscapes (Western Ghats, Central India and North Eastern Landscape), three of these being not contemporaneous, with spatial scales likely being different for density estimation and index measures.

Commented [??24]: Again Jhala et al. (2015) completely invalidates Jhala et al. (2011) and these arguments do not hold true.

Figure 1: I see some additional discrepancies in the values presented in Figure 1. According to the information presented in the suppl. material the encounter rates (scats per 10 km) are 2.36 in Melghat (year 2005), 2.12 in Bandipur (mean encounter rate of scats; year 2006) and 2.4 in Penchar-MR (year 2004), which differ from the values presented in Figure 1. Clarify please and adjust where necessary.

Response: We thank the reviewer for pointing out these additional discrepancies in the original Fig 5 of Gopalaswamy et al 2015, that we had not highlighted in our original Fig 1. The revised fig 1 shown above clarifies these discrepancies.

Minor comments

Line 67: a space is missing before “cherry picked”

Response: Corrected

Line 70: I am not sure if “incorrect” (per se the parameter namely occupancy is correct) is the right term here. I would rather say that they used an inadequate parameter, namely occupancy, as surrogate for detection probability of tiger scats.

Response: we agree with the reviewers opinion and have changed the word "incorrect" to "inadequate".

Line 119: What does CV mean? Coefficient of variation!? You should write the full name the first time it appears in the text.

Response: Corrected.

Reviewer 2 (Anonymous)

Basic reporting

It is not often that one gets to review a paper such as this with all the ingredients of a scientific pot-boiler –famous names in wildlife ecology, potential statistical skulduggery, arguably a creature with the greatest mystique in human history, and national honour over its status at stake. Qureshi at al. may perhaps not have been so rhetorical over their criticism of the Gopalaswamy et al. paper had the media, ever alert for juicy stories on failure of the system, had not jumped into the fray, or if one of the co-authors had not given an interview to a prominent newspaper questioning tiger numbers and their monitoring in India. The latter's paper may then have passed off as merely one that contributed to a staid scientific debate on animal population estimation methods. As matters stand, however, the paper has generated a lot of interest, publicity and controversy, and it would be in the interest of tiger conservation to set the record straight.

My task as a reviewer is not to do a thorough dissection of the Gopalaswamy et al. paper itself (which has gone through the process of peer-review), especially the theoretical basis of their statistical models, but to examine the claims in Qureshi et al. paper on the biases in the former which could render their conclusions infructuous. In other words, is there a strong enough case for publishing the Qureshi et al. paper in PeerJ?

Response: We thank the reviewer for his focused review and for his/her understanding of the context of our MS.

Experimental design

Not relevant

Validity of the findings

Qureshi et al. make six basic observations to refute the conclusions of the Gopalaswamy et al. paper. I shall provide my assessment of whether these observations are valid or not for each of the six points.

a.Use of incorrect ecological parameters
Yes, it certainly seems that Gopalaswamy et al. have incorrectly used detection probability from tiger occupancy studies, rather than a double-blind observer design, to parametrize their theoretical model as stated by Qureshi et al.

Response: We appreciate the reviewers comment and assessment.

b. Cherry picked references
Ostensibly, Gopalaswamy et al. have confined their model analyses to data from two sets of studies – Karanth and team, and Jhala and team – because these constitute the major tiger population estimation exercises from India in the past decade or more. However, Qureshi et al. have a valid point in that results of detection probability and coefficient of variation from at least two peer-reviewed papers on tiger estimation published in reputed journals have been (conveniently?) omitted by Gopalaswamy et al. thus rendering their conclusions less than robust.
Response: We appreciate the reviewers comment and assessment.
c. Inappropriate and wrong data
The most telling criticism of the Gopalaswamy et al. paper is that they use invalid data (in Figure 5) on tiger count and tiger sign intensity because at half the sites their data have been collected years apart or wrongly reported (from one site). In particular, a 7-year time separation of tiger count and sign data from Bandipur would certainly make this data point invalid for deriving R2 as tiger abundance at this site could have varied considerably during this period. I do not have access to the Karanth & Kumar (2005) report from which a scat encounter rate of 1.99/10 km has been taken and reported as 3.6/10 km in Gopalaswamy et al. (2015). If this were true (and I am assuming that Qureshi et al. have been diligent in their homework and assertion), Figure 5 is indeed completely useless in deriving R2

for the tiger count-sign relationship, even if we condone the 2- or 3-year gaps in measurements at the other two sites, Pench and Melghat. Indeed, Tadoba would shift much further left along the X-axis and strengthen the value of R2 substantially. It is of course possible that Gopalaswamy et al. discovered an anomaly in the original figure for Tadoba reported in Karanth and Kumar (2005), but in this case they should have made this explicit in their paper. However, Qureshi et al. should also not take a R2 value of 0.642 (lines 180-181) for only four valid data points too seriously. They have already made their point about the inadequacy of Figure 5 in Gopalaswamy et al. In fact, it would be interesting if they could compute R2 after merely changing the "outlier" Tadoba value to 1.99.
Response: We thank the reviewer for his/her assessment. As pointed out by reviewer 1, there were additional discrepancies in the original referenced sources of Gopalaswamy et al 2015 Fig 5 (please see our response to reviewer 1 related to our Fig 1). We have marked these in our Fig 1, (not only were Tadoba data incorrectly reported and later addressed in a corrigendum (saying that the original reference was wrong, after we pointed it out in our complaint to MEE, but there are still errors in reporting Pench Maharashtra as well as for Melghat). We have omitted the re-computation of R2 (as suggested by two reviewers) from our MS due to small sample size.

d. Variability in tiger capture …. mark-recapture

While I cannot make a judgment about the reasons for low precision in Karanth et al. (2004), Qureshi et al. are correct in saying that Gopalaswamy et al. should not have restricted their model to examining only data from Karanth et al. (2004).

Response: We thank the reviewer for his candid assessment.

e. Repeating non peer-reviewed literature

Yes, the statement in Gopalaswamy et al. about the improbability of a 49% increase in tiger numbers over four years has been made without a proper assessment of what the 2006 and 2010 estimates actually represent. More sites were added for the tiger population estimation in 2010 and this should have been factored in while making statements questioning the rate of increase.

Response: We thank the reviewer for his assessment.

f. Propaganda that is not consistent with facts

It is unfortunate that these days even scientific journals need catchy "blogs" to capture the attention of their readers. In this case, the blog in Methods in Ecology

and Evolution casting doubts on tiger population trends was unjustified. Subsequent articles in the media and even journal papers on the subject have slated towards a negative view of India's tiger assessment. Even though this is not a scientific issue, Qureshi et al. are aggrieved by the negative publicity to the outcome of a rather challenging task of estimating tiger populations on such a large scale.
Response: We greatly appreciate the reviewers understanding and appreciation of our work.

Comments for the Author

One piece of advice for Qureshi et al. is that they should not fall into the same trap of high rhetoric. They have made their case against Gopalaswamy et al. very clearly. I suggest some minor edits to remove the rhetoric – the edited paper would still retain its scientific punch!
Response: We agree with the reviewer and have toned down our language (as also suggested by reviewer 3).
Suggested list of edits:
Line 18: “double sampling”
Line 19: large scale animal surveys
Line 20: replace “elegantly” with “potentially” [after all, the statistical debate is not necessarily over]
Line 21: should be “Gopalaswamy”
Line 28: Could I suggest rewriting as “selectively-picked estimates from the literature”?
Line 29: suggest replacing “suspect” with “questionable”
Line 31: suggest “statistical design of large scale animal surveys”
Line 46: suggest “basic competence of scientists”
Line 48: “a peer reviewed publication”
Line 55: suggest “of the scientific method probably more than others”
Line 59: suggest “supposedly demonstrate”
Line 64: suggest “In particular, they claim the relationship…”
Line 67: suggest “selectively picked references from the literature” [the media can report this as cherry picked!]
Lines 88-90: Should be a single sentence with a comma (occupied site, while)
Lines 96-99: Should be a single sentence with a comma (Nichols et al. 2000),

where two observers)
Line 107: “the use of occupancy detection”
Line 114: again “Selectively picked references”
Line 120: Should this be Karanth 2011a?
Line 139: Gopalaswamy
Lines 179-181: Suggest rewriting this part taking into consideration my suggestions given above (C. Inappropriate and wrong data)
Line 189: Which model is being referred to? Model 1 or Model 2?
Response : Both models, change made in MS.
Line 200: suggest “adoption of sound and practical methods”
Line 235: The reference states Jhala et al. 2014.
Line 238: same as previous
Line 243: I am wondering the word “propaganda” can be replaced with another, equally effective word? Distortion? Distorted reports? Misleading reports?
Line 243: suggest “MEE paper by Gopalaswamy et al.”
Line 248: Gopalaswamy
Line 252: I wonder if this line is needed? PeerJ editors can decide.
Response: We thank the reviewer for his insightful comments and correcting our language. We have made all of the above suggested edits (including omission of line 252 which was hinted at by the reviewer) in the revised MS.

Reviewer 3 (Anonymous)

Basic reporting

The language used in the manuscript is too emotive. I think it needs a substantial rewrite to focus on the issues of the science in Gopalaswamy et al. exclude suggestions of deliberate falsification of data - just say the data quality undermines their results.
Response: We have tempered down our language and we hope the reviewer finds it suitable in the revised MS.

Experimental design

I think a table of RAI (though not a full scale review) showing examples of fits between indexes and independent abundance estimates would greatly strengthen the author's case here and wouldn't involve a lot of extra work.
Response: Our paper is not about validating index calibration, but rather about issues related to a published paper, that in our opinion, was poor science and propagates unjustifiable postulates. We do not want to deviate from this central theme of our MS and make it into a debate on pro's and con's of using indices of abundance for population estimation.

Validity of the findings

This manuscript focuses on responding to G et al's critique of their earlier work. It would be improved and strengthened by first concisely dealing with references to their work but then by comparing other RAI-based findings to broadening the perspective (see above).
Response: We do not refer to Gopalaswamy et al's critique of our work in this paper – what we point out is regarding improper data and concepts being used by Gopalaswamy et al (2015) in their paper. We do not wish to engage in proving or disproving use of indices in this paper.

Comments for the Author

Review: Twisted tale of the tiger: the case of inappropriate data and deficient science.
Quershi et al.

Summary:
The use of indexes in animal abundance surveys clearly divides researchers – in fact, I notice a number of the authors publishing in this literature, have co-authored both for and against views on the subject! The present manuscript is a (somewhat delayed) response to Gopalaswamy et al (2015) who - a) present a simulation model to support the assertion that index-based approaches are unreliable and ineffective, and b) use the simulation to argue that the results of Jhala et al (2011) with a reported high r2 between an index of tiger sign and tiger

density - is likely to be spurious. Gopalaswamy et al critique was very personal and harsh in their criticism of Jhala et al., and given this I can understand why the present manuscript uses highly charged language. But I still I think the sections on scientific integrity and short-comings of peer review do not help the authors make an effective case. That said, I think a shortened and more focus response to G et al, is important and would help to clarify differences of view in the literature and perhaps could allay wider concerns about Indian tiger population estimates.
Response: We thank the reviewer for understanding the context of our paper. We agree partly with the reviewer and have removed use of "highly charged" language in the revised MS as well as revised the introduction to address the suggestions of the reviewer.

Main Points:

1) The manuscript would be greatly improved if the authors removed sections on scientific integrity etc. e.g. remove first two sentences in the abstract. Lines 41-46; lines 53-55; simply refer to manuscript published in MEE, remove prestigious journal etc. Remove all phrases like "cherry picking" "highly suspect" it needs to be written as If the authors of G et al. may have been in error but not scheming to undermine Jet al.
Response: We agree to the suggestion by the reviewer and have modified the language of the revised MS accordingly. We have reworded the first two lines of the abstract, removed lines 41-46; and line 53 as suggested by the reviewer. We have retained lines 54-55, but reworded them to become more general and less personal to keep the perspective of reporting problems through a peer review process . Phrases like "cherry picking" and "highly suspect" have been removed from the text entirely.
2) A large part of G et al's paper focuses on criticisms of RAIs. Could the authors here provide a table of examples of RAIs to give readers a better sense of typical fits to independent estimates of density. The most appropriate one I can think of which matches the case of Jhala et al 2011 is Funston et al 2010 (referred to in G et al, but not in the text strangely) – which uses a spoor count based RAI. But others use for photographic rates to provide RAIs e.g. O'Brien et al (2003); Rovero et al (2009) and Palmer et al (2018). Provide more evidence for better fits

between indexes and actual density estimates. Demonstrating that indexes work in many settings, might be used to support the present manuscripts assertion that G et al's model is based on overly pessimistic parameter values.

Response: We appreciate the reviewers concern of making our paper cover a broader theme, i.e. of Indices (RAI's). But we are reluctant to do so in this paper which solely focuses on problems of a published paper. Here too, we do not dwell on the mathematical models of Gopalaswamy et al (which Murray Efford did critique), but purely on the inappropriateness of the data used and ecological concepts on which Gopalaswamy et. al. based their models.

3) It was really good to see the authors clarify estimates of percentage of increase in the tiger population correcting for increased survey area. However, it would be also useful to know what percentage of the total tiger population estimate is obtained from the double-counting GIS extrapolated estimate. E.g. what perc is dervided from tigers in core areas where SECR methods were used compared to the remaining population. Reading the note published by Karanth et al (2011) one could assume that all (or most) the tiger estimates were based on extrapolations from indexes. Are we talking about <20%?

Response: We thank the reviewer for his/her suggestion and we mention % of photo-captured tigers in the revised MS. We used capture histories 1506 individual tigers in our SECR models to estimate a country population of 2226 (SE 1945 to 2491) tigers (Jhala et al 2015). This amounts to 68% of the total population of >1.5 year old tigers being photo-captured.

4) The authors make an interesting point about the use of occupancy statistics for G et al simulation. Could they clarify a bit further why this might not be appropriate?

Response: Please see lines 85 to 111 of the revised MS wherein we explain why occupancy detection probability is not a surrogate for detection probability of scat. We also explain how detection probability of scat could have been obtained had Gopalaswamy et al. (2015) been serious in conducting a proper experiment for testing the use of indices for abundance estimation.

5) It may also be worth pointing out that the Karanth et al (2004) paper comparing tiger density against prey density, was based on tiger density using MMDM method and not SECR. Might this have contributed to the poorer fit between the two?

Response: We agree with the reviewer that the use of MMDM may have led to an imprecise fit. However, at the time when Dr. Ullas Karanth did his pioneering work on tiger population estimation there were several limitations (those of film cameras, camera costs, and analytical approaches). We appreciate the efforts by Dr. Karanth and his team, and cite his work in our publications for their contribution. However, much of these initial efforts relied on volunteers for sampling and cameras were fixed with concrete and iron posts on forest roads to avoid elephant damage and theft. As field biologists will understand, camera trapping is by essence a trapping exercise, where local site selection and adaptability is an important element to achieve good captures. Large carnivores (like tigers) learn to avoid obvious fixed cameras resulting in low recapture rates. It seems likely that the study of Karanth et al 2004 was limited with the number of camera traps, since as sample area increases the variance of the estimates increase as well and capture probability of tigers decreases. The fixed obvious camera placements along roads, poor site selection, few cameras with large spatial gaps, were we believe a major reason for the imprecise tiger density estimates (see Karanth & Nichols, 2002, Monitoring Tigers and their Prey, page 130, Appendix 5, pages 184-186 for these initial spatial designs). The study by Karanth et al. 2004, used square shaped transects for estimating prey – there are major issues of detections (and subsequent computation of effective strip width) as well as double counts around the corners with such a design. Selective poaching can significantly depresses tigers within protected areas which can continue to have good prey populations, this adds noise to a relationship with prey density.

But these issues are largely peripheral to our current MS. What is important here is that when better and precise estimates are available in published literature why should model development by Gopalaswamy et al be based solely on the imprecise estimates?

Page 1: [1] Commented [??6] **5/6/19 10:52:00 AM**

Note: Now Jhala et al. (2015) also refutes Jhala et al. (2011). Also, as discussed in the review Gopalaswamy (2019), Jhala et al. (2011) selectively leave out about 8 data points of south India (see Jhala et al. 2011 – national tiger assessment report). So, it will be misleading to state that this was a carefully designed experiment. The larger question that arises from this claim is this: if such a perfect result was obtained from Jhala et al. (2011), why was there a need to do anything else after that? Specifically, why did Jhala et al. (2015) suddenly ignore the findings of Jhala et al. (2011) completely?

Page 1: [2] Commented [??7] **5/6/19 11:10:00 AM**

Notified will be too mild a word to describe what the authors actually did. In fact, the authors summarily DEMANDED the retraction of Gopalaswamy et al. (2015a) from MEE (see Vishnoi 2015, Kempf 2016) without any valid scientific reason and severely pressurized the journal. There is no reason why PeerJ will not face the same wrath have to bear the wrath from these authors should they not subscribe to the authors' point of view. Given that Qureshi et al. (2018) has now appeared as non-peer-reviewed article and at least one of the authors has WIDELY publicized this over Twitter, and given that the new official tiger number estimate is made in India.

Page 3: [3] Commented [??14] **5/6/19 2:34:00 PM**

The authors MUST write all of this in the main manuscript if they seriously believed MEE and the whole peer-reviewed system is flawed. If the authors were privy to this correspondence between MEE and Dr. Murray Efford, they MUST share this correspondence here for peer-review. And, in fact, state this fact in the main manuscript as well. They MUST cover what Dr. Murray Efford wrote and why MEE rejected the manuscript (if they did so) and for what scientific reasons are the authors of Qureshi et al. (2018) concerned about. Once again, they must take more responsibility in what they write so it would help if they put all this material in the main part of the manuscript.

Page 3: [4] Commented [??16] **5/6/19 1:33:00 PM**

The authors have already publicized the original submission (Qureshi et al. (2018)), with all the 'emotive' language, on Twitter, as though it is already a peer-reviewed publication. So in terms of the objective of discrediting the authors of Gopalaswamy et al. (2015a,b) they have already achieved that objective. Now, by removing some of the "emotive" language, they do not repair anything.

Page 3: [5] Commented [??17] **5/6/19 1:43:00 PM**

The review provided by Dr. Arjun Gopalaswamy is NOT a rejoinder but it is an invited review by PeerJ. It only happens to be made available to the public, just like the non-peer-reviewed article (Qureshi et al. (2018)). Also, Qureshi et al. (2018) isn't really an original scientific publication. Rather think it is merely a collection of allegations, with no objective of improving scientific understanding of anything for that matter. At the most, it can be thought of as an attempted rebuttal of Gopalaswamy et al. (2015a,b).

Page 3: [6] Commented [??18] **5/6/19 2:31:00 PM**

The presumptive nature of this remark and the authors' haste is disturbing. It was well known to the authors of Qureshi et al. (2018) that the lead author of the Gopalaswamy et al. (2015a,b)

paper will be one of the reviewers of this submission even before submission since this is exclusively a critique of Gopalaswamy et al. (2015a,b). Now by taking this easy option of simply avoiding responding, ALL the scientific inadequacies discussed in Gopalaswamy (2019) remains. The authors also want this to be published in a rush now, suddenly, after patiently waiting for 4 years. This is perhaps because India's quadrennial and the latest national tiger estimation results are about to be announced very very soon and the authors perhaps are very keen that they get the backing of PeerJ in this effort. For the last twelve years, owing to the lack of transparency (both with the data and methods) in India's official tiger surveys, many have been asking questions about data transparency and methods used in the survey as none of them have been peer-reviewed. Hence, my recommendation for PeerJ will be, as before, to transfer the work and the responsibility of reviewer responses to the authors themselves.
Regardless, whether Dr. Arjun Gopalaswamy and other authors of Gopalaswamy et al. (2015a,b) decide to formally respond to Qureshi et al. (2018) (should this be considered for publication) in PeerJ or other journals (Nature, Science, PNAS, MEE or whichever) will be a decision that the authors of Gopalaswamy et al. (2015a,b) will take and cannot be decided by the authors of Qureshi et al. (2018) or even PeerJ for that matter.

Page 3: [7] Commented [??20] **5/5/19 9:12:00 PM**

This is not true. They have engaged in criticism of the statistical development of Gopalaswamy et al. (2015a,b) and seem to have added to all the confusions in by not recognizing the Royle and Nichols (2003) identity. Therefore, I would suggest they remove these statistical arguments entirely from Qureshi et al. (2018-V1).