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How sampling-based overdispersion reveals India's tiger monitoring orthodoxy

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Abstract

Agencies responsible for recovering populations of iconic mammals may exaggerate population trends without adequate scientific evidence. Recently, such populations were termed as "political populations" in the conservation literature. We surmise such cases are manifested when agencies are pressured to estimate population parameters at large spatial scales for elusive species. For example, India's tiger conservation agencies depend on an extrapolation method using index-calibration models for estimating population size. A recent study demonstrated mathematically the unreliability of this approach in practical situations. However, it continues to be applied by official agencies in Asia and promoted further by global organizations working on tiger conservation. In this article, we aim to: (a) discuss the ecological oddities in the results of India's national tiger surveys, (b) contrast these survey approaches to known statistical approaches for large scale wildlife abundance estimation, (c) demystify the mathematics underlying the problems with the survey methodology, and (d) substantiate these arguments with results from India's national tiger survey of 2014. Our analyses show that the predictions of tiger abundance reported by the 2014 survey, and consequently on tiger population trends, are misleading because of the presence of high sampling-based overdispersion and parameter covariance due to unexplained heterogeneity in detection probabilities. We plead for designing monitoring programs to answer clearly defined scientific or management questions rather than attempt to meet extraneous social or funding related expectations.

KEYWORDS

abundance at large spatial scales, index-calibration, large carnivore monitoring, political populations, species conservation, survey design

1 | INTRODUCTION AND BACKGROUND

Krebs (1991) recognized that monitoring programs must advance our knowledge of the underlying dynamics of animal populations if they are to improve either science or conservation. Towards this end, Nichols and Williams (2006) recommend a priori designing of animal monitoring programs to answer clearly defined scientific or management questions. In practice, Williams, Nichols, and Conroy

(2002) identify two major sources of uncertainty (imperfect detection and inappropriate spatial sampling), which must necessarily be addressed by any monitoring program aiming to generate strong inferences about animal population dynamics.

Monitoring programs for some of the world's most iconic endangered mammals appear routinely to ignore these profound insights leading to claims about their population dynamics resting on weak inferences and untested leap-of-faith arguments. Darimont, Paquet, Treves, Artelle, and Chapron (2018) explained how population trends reported by national agencies for several charismatic carnivores lack adequate scientific support. Using case studies of wolves (*Canis lupus*) in United States and Sweden, and brown bears *(Ursus arctos)* in Romania and Canada, they demonstrated how population changes claimed by agencies are exaggerated. Hypothesizing that these claims largely serve political interests, they coined the term "political populations" (Darimont et al., 2018) to identify such populations. Therefore, it is important to independently assess and identify whether agency claims about populations of iconic carnivores arise from poorly framed monitoring questions, inadequate sampling designs, or from social considerations such as "motivated reasoning" (Kunda, 1990). In this case study, we attempt to disentangle these factors by examining official monitoring reports on wild tigers (*Panthera tigris*) in India, which is one of the hypothesized political populations in Darimont et al. (2018), because of the global attention and massive conservation investments the species has attracted (PTI, 2016).

2 | INDIA'S CLAIMS ON TIGER NUMBERS

The four official surveys of 2006, 2010, 2014, and 2018 (hereafter referred to as "National Tiger Estimation (NTE) surveys"; Jhala, Gopal, & Qureshi, 2008, Jhala, Qureshi, Gopal, & Sinha, 2011, Jhala, Qureshi, & Gopal, 2015, Jhala, Qureshi, & Nayak, 2019) report country-wide estimates of tiger population sizes at 1,411 (1,165–1,657), 1,706 (1,507–1,896), 2,226 (1,945–2,491), and 2,967 (2,603–3,346), respectively. The numbers in brackets putatively indicate the range, without any statistical explanation about how these values are derived or what their associated confidence levels are. If considered at face value, these numbers with their reported error bounds indicate spurts of increases in tiger numbers in India during the period between 2006 and 2018.

Considering only those specific areas that were surveyed in all the first three surveys (2006, 2010, 2014; summarized from Jhala et al., 2015), these tiger population trends translate to a 17.3% increase in tiger abundance and a corresponding increase of 34.6% in local tiger density, implying that local tiger density D rose at twice the rate of tiger abundance N ($\Delta D/\Delta N = 2 > 1$) between 2006 and 2010. Between 2010 and 2014, an even steeper 29.8% increase in tiger abundance was reported, but this time with a corresponding decrease in local tiger density by 18.7%. These results imply a *reversal* of the tiger metapopulation dynamics mechanism (from $\Delta D/\Delta N = 2$ to $\Delta D/\Delta N = 0.63$) after 4 years, with the year 2010 as the point of inflexion. The full results of the 2018 survey are not available yet. However, based on summarized results (Jhala et al., 2019) and comparison with the previous 2014 survey we obtain a $\Delta D/\Delta N = 0.98$, suggesting yet another major change in the tiger metapopulation dynamics pattern.

Furthermore, between 2006 and 2010, the surveys reported a contraction of tiger range by 12.9% (or 11,400 km^2). In contrast the next survey interval (2010–2014) claims an abrupt *reversal* of the earlier pattern, reporting a range expansion of 9.4%, claiming tiger recolonization of 7,250 km^2 of new habitats (computed from Jhala et al., 2015).

These tiger population increase mechanisms imply a concave *upward* relationship between tiger abundance and occupancy (Figure 1) and stand in contrast to the general mechanism of a monotonically increasing, but concaving *downward*, relationship based on occupancy-abundance principles (see Gaston et al., 2000). Here, we define occupancy as the area occupied by tigers. Furthermore, long-term studies of tiger population dynamics using rigorous photographic capture-recapture surveys even in some of the better-protected tiger reserves of India (Karanth, Nichols, Kumar, & Hines, 2006) and Thailand (Duangchantrasiri et al., 2016) indicate far lower annual rates of density increase (approximately 2–4%). If these survey outcomes are considered together the implication of NTE surveys is that tiger populations in large, poorly-protected, low-prey density, sink landscapes exhibit higher growth rates than do populations in better protected source populations (Karanth, Miquelle, Goodrich, & Gopalaswamy, 2016). These claims from the Indian tiger surveys stand in stark contrast to scientific understanding derived from the source-sink theory in population biology (Pulliam, 1988) which are foundational to most global recovery plans for large carnivores including those for tigers in India (NTCA, 2012; Walston et al., 2010).

Recently, Harihar, Chanchani, Pariwakam, Noon, and Goodrich (2017) analyzed these NTE survey results to show that an increase of sampled areas in tiger "source sites" among successive surveys led to decreases in tiger density and thereby supporting the proposals of Karanth et al. (2016) and Harihar et al. (2018), that tiger population recovery rates will be far slower than expected.

What are the reasons for these gross ecological anomalies that arise from Indian tiger surveys? The explanation by Darimont et al. (2018) is that ecological claims about

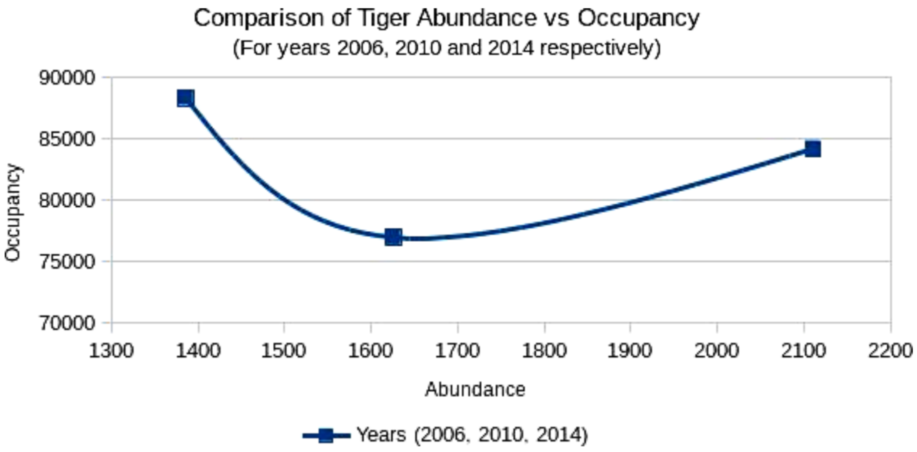

FIGURE 1 The occupancy-abundance relationship of India's tigers based on NTE studies of 2006, 2010, 2014 (Jhala et al., 2015). Computations are based on estimates of occupancy and abundance only from regions sampled in all the three surveys

political populations can often be disconnected from formal science. But the acceptance of the Darimont et al. (2018) explanation, without critical examination, will impede our understanding of tiger population biology due to potential scientific serendipities (Wintle, Runge, & Bekessy, 2010). Therefore, here we examine the basis of India's claims on tiger numbers by assessing, in detail, the methods and models used as bases for these claims.

3 | ESTIMATING WILDLIFE ABUNDANCE AT LARGE SPATIAL SCALES

Double-sampling as a survey methodology was developed because rigorous estimation of abundance at large spatial scales is often impractical due to ecological, environmental, and logistical constraints (Eberhardt & Simmons, 1987). In principle, double-sampling is a design-based approach and involves the following steps:

1. Selection of a *sample* of sites from a larger pool (*population*) of sites spread across a focal region using a spatial sampling design, for example, using simple random sampling or systematic sampling (Thompson, 2002).
2. Conduct of surveys at the sampled sites to estimate true animal abundance using a rigorous, reliable, method which is typically expensive, intensive, and relatively difficult to implement (e.g., photographic capture-recapture sampling, distance sampling). If the number of samples selected during (1) is large and the sample selection is representative, it is possible to reasonably estimate animal abundance for the focal region of interest immediately after (2). But, if this is not feasible, it might become necessary to sub-sample from this larger sample (Buckland et al., 2001; Pollock et al., 2002; Thompson, 2002).
3. Conduct of a less rigorous, but more practically feasible, field survey using an appropriate index of animal abundance. Examples of such indices include, the raw count of pictures from a camera trap survey, encounters at scent stations or the number of animal tracks per unit effort. The index is measured at the sub-sampled sites as well as across the larger sample of sites or if possible over the entire focal region.
4. Development of an index-calibrating model (often a simple linear regression model) to establish a statistical relationship between true or estimated animal abundance (2) and its index (3; Eberhardt & Simmons, 1987).
5. Prediction of animal abundance for the larger area of interest using the results of (2), (3), and (4).

Consequently, the accuracy and precision of the large-scale abundance estimate from the double-sampling approach will vastly rest on the sample selection of sites (1) and the predictive ability of the index-calibration model used (4).

There are other model-based alternatives for estimating abundance at large spatial scales that utilize joint likelihood models. For example, Conroy, Runge, Barker, Schofield, and Fonnesbeck (2008) combine information from two survey types of differing reliability, both independently attempting to estimate abundance, and both estimating detection probabilities associated with their individual surveys. Such a joint likelihood model (with abundance as an explicit parameter in the likelihood) permits investigators to evaluate the quality of abundance estimates (using simulations) for various combinations of practical field designs. But as with most likelihood-based estimators, sample size requirements for reliable estimation of abundance is usually large.

4 | INDIA'S OFFICIAL TIGER MONITORING PROGRAM

The NTE survey methodology was developed and implemented in 2005 as India's new official tiger monitoring approach (Jhala et al., 2008) after the failure of the previous "pugmark census" method to detect tiger extinctions in a key reserve in India (Tiger Task Force, 2005). NTE surveys claim to implement the "double-sampling" approach. But, they omit the critical spatial sampling step, as described above in (1), and thereby losing the advantages of a formal design-based approach. The NTE surveys also claim to utilize the "joint likelihood modelling" approach (Jhala et al., 2015; Jhala et al., 2019). But, instead, they only use a single likelihood (Efford, 2019) for estimating site-specific density estimates where some model parameters are refined using covariates. However, the efficiency of extrapolation to larger regions will still be defined largely by the spatial sampling step (1). And since this critical spatial sampling step has been omitted during the survey design stage of India's official tiger monitoring program (Jhala et al., 2008), the program has relied on a fragile and untested methodology till date.

The theoretical basis of the NTE survey methodology can be traced to an unpublished pilot survey (Jhala & Qureshi, unpublished data), referenced in Jhala, Qureshi and Gopal (2011), where they found raw tiger sign encounters to *stabilize* after a minimum walk effort of 5 km. While the meaning of the term "stabilize" in this context is not clear, this assertion was followed up by a larger tiger sign index-calibration experiment (herein referred as IC1; Jhala, Qureshi, & Gopal, 2011) based on a selective subset of data from the first NTE survey of 2006 (data from southern India were omitted; see Jhala, Qureshi, Gopal, & Sinha, 2011). The high estimated R^2 value ($r^2 = .95$) suggested that tiger sign indices could be used to predict tiger abundances reliably based on the developed linear regression model in IC1. This predictive model would have logically raised questions about the very need to employ more rigorous, but expensive, methods such as camera trapping to count tigers. But, there was already a latent but major scientific contradiction emerging at this point which was not noticed at the time. The NTE survey of 2010 (Jhala, Qureshi, Gopal, & Sinha, 2011), while claiming to use the findings of IC1, also demonstrated that the linear regression model of IC1 would perform *poorly* if applied across India due to non-random sampling in Jhala, Qureshi, and Gopal (2011).

Indeed, the next NTE survey of 2014 (Jhala et al., 2015) proved conclusively the failure of using the IC1 model to make predictions because it demonstrated (a) nonrobustness of IC1 over space and time, (b) nonlinearity in index-calibration experiments. Yet, the subsequent NTE surveys (Jhala et al., 2015, 2019) continued to predominantly use tiger sign indices as a means to make predictions of tiger abundance over regional scales. Recently, Qureshi, Gopal, and Jhala (2019a), published within a month after the NTE survey of 2018 (Jhala et al., 2019), introduced a fresh set of confusions about India's official tiger monitoring program because Qureshi et al. (2019a), simultaneously, validate both Jhala, Qureshi, and Gopal (2011) and Jhala et al. (2015) models, which is not mathematically reconcilable. The emerging scientific contradictions (more details are discussed later) suggest that India's estimates of tiger numbers have larger uncertainties associated with them than previously thought. Consequently, these findings weaken India's claims of a *doubling* tiger population size over the past 12 years (Jhala et al., 2019).

At least some of these methodological uncertainties were revealed in 2015 when a mathematical study, Gopalaswamy, Delampady, Karanth, Kumar, and Macdonald (2015a, 2015b), developed formulae to estimate certain parameters (especially detection probability) from measures of R^2 in typical index-calibration experiments. They applied these formulae on two tiger index-calibration experiments to re-emphasize the need to consider a formal spatial sampling design (Thompson, 2002). However, this central message was obscured by the mathematical treatment of the topic making it inaccessible for conservation practitioners and field ecologists to utilize in practice. Therefore, we present here a simpler interpretation of Gopalaswamy et al. (2015a, 2015b) and assess their findings in relation to results from the NTE survey of 2014 (Jhala et al., 2015).

5 | INDEX-CALIBRATION MODELS OF INDIA'S TIGER SURVEYS

In the past, index-calibration experiments have yielded divergent results in terms of efficiency (see Hayward et al., 2015, Gopalaswamy et al. (2015a, 2015b) and citations therein). Gopalaswamy et al. (2015a, 2015b) mathematically modeled typical index-calibration models to analyze the factors that produce such divergent outcomes in real world field surveys. In Appendix S1, without getting into the mathematical complexities of Gopalaswamy et al. (2015a, 2015b), we heuristically present concepts of sampling-based overdispersion (SOD) and parameter covariation.

5.1 | Assessing the predictive strengths of tiger sign index-calibration experiments

Gopalaswamy et al. (2015a, 2015b) examined two different tiger sign index-calibration experiments, which produced extremely divergent calibration successes. One of these experiments is IC1 (Jhala, Qureshi, & Gopal, 2011) and we name the other as IC2. As a framework for statistical comparisons, they assumed that over India's tiger-occupied habitat of about approximately 80,000 km^2 (Jhala, Qureshi, Gopal, & Sinha, 2011), there could be a potential pool of more than 400 sites, each of approximately 200 km^2 size (approximately the size of sites used in the two experiments). In the two experiments, at each site, an estimate of tiger density was derived from photographic capture–recapture sampling from replicated surveys (see Karanth, Nichols, Kumar, Link, & Hines, 2004, Jhala, Qureshi, & Gopal, 2011, for field work details).

At these sampled sites, tiger signs (scats and/or tracks) were counted by observers walking along trails to derive encounter rate indices (number of scats and/or track sets/km walked). These index count data, $S|N$ were fitted to linear regression models by Ordinary Least Square solutions. The first experiment (IC1), with a sample size of 21 sites, returned a high R^2 estimate of 0.95 (as reported in Jhala, Qureshi, & Gopal, 2011), whereas the second experiment (IC2), with a sample size of 8 sites, returned a low R^2 estimate of 0.0004 (computed using the lm function in *R* that uses the Equation 4 from Kvalseth, 1985 for estimating R^2).

We note that the slope of these index-calibration relationships is $\beta = kp^*$, where p^* is the average detection probability per individual. Because the index based on tiger signs was computed from counts summarized as a rate parameter per unit length (1 km) over a single sweep, we set the value of $k = 1$ to permit comparisons with the estimate of population p^*, which we will see later. Thus, making $\beta = p^*$ in this case. We note here that the estimate of R^2 is invariant to the scale used. That is, we would obtain the same value for R^2 whether we regress $Y \sim X$ or $Y \sim 10X$, where Y is the response vector and X is the predictor vector. If we apply the mathematical formula derived by Gopalaswamy et al. (2015a, 2015b) for population R^2 using the binomial model (less overdispersed case) we obtain the estimate of detection probability p^*. This computed value is seen to be high for IC1 (p^*=0.0003) and low for IC2 (p^*=0.92). These two slopes are plotted as blue lines in Figure 2.

5.2 | Estimating the population parameter for tiger sign index-calibration experiments

From the larger tiger distribution surveys conducted by Jhala, Qureshi, Gopal, and Sinha (2011) and Karanth et al. (2011), the average from these two surveys was estimated to be 0.125 (represented by the red line in Figure 2; Gopalaswamy et al. (2015a, 2015b), lying between the two blue lines. The result demonstrates that sample sizes in both experiments (IC1 = 21 sites and IC2 = 8 sites) were far too small to reflect the population characteristics accurately. Secondly, the sampled sites selected non-randomly were not truly representative of the assumed larger pool of >400 sites because they both failed to converge on the population estimate. This implies that *both* these index-calibration models have poor predictive power when used to predict abundance across the wider spatial region of interest. The presence of large SOD makes index-calibration models very data-

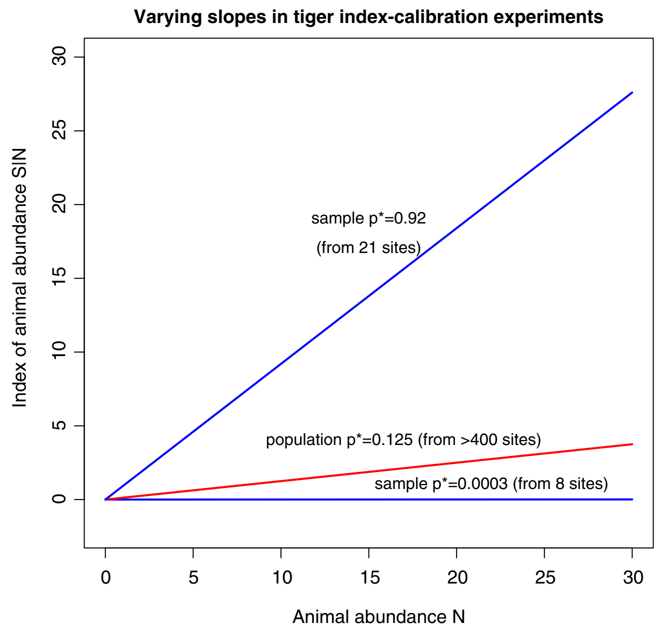

FIGURE 2 Illustration of the contrasting estimates of p* under the binomial model of tiger index-calibration, N versus S (which is conditional on N). The lines are generated by the model S|N ~ Binomial (kN, p*), so that E(S|N) = kNp*. The sampling occasion k is assumed to be a constant with a value of one. The two blue lines represent sample p* estimates from two different tiger index-calibration experiments. The red line represents the line generated by a p* estimate from an independent survey of the larger population of sites over Indian landscapes

hungry, and any such selective, and potentially biased, subsampling of sites will compound the predictive inefficiency of these models.

5.3 | Factors likely to influence the potentially large variation in p^*

We note that $p^* = \alpha p$, which is a product of two probabilities, where p is the probability of detecting an individual tiger conditional upon a tiger utilizing the trail segment and α represents probability of the tiger utilizing the trail segment. We anticipate that the magnitude of p is primarily determined by the type of substrate (see Figure 3; Harihar & Pandav, 2012)—an observation covariate. We note here though that the value of p can potentially be very high and unvarying only in exceptional circumstances, for example, detecting tiger tracks in snow in Russia (Miquelle, Smirnov, Zaumyslova, Soutyrina, & Johnson, 2015; Stephens, Zaumyslova, Miquelle, Myslenkov, & Hayward, 2006) but variable in other tiger habitats. For example, in drier forests (that comprises about 50% of tiger habitat in India), tiger scats can remain intact for days prior to counting, whereas they disappear rapidly in wetter regions (Marques et al.,

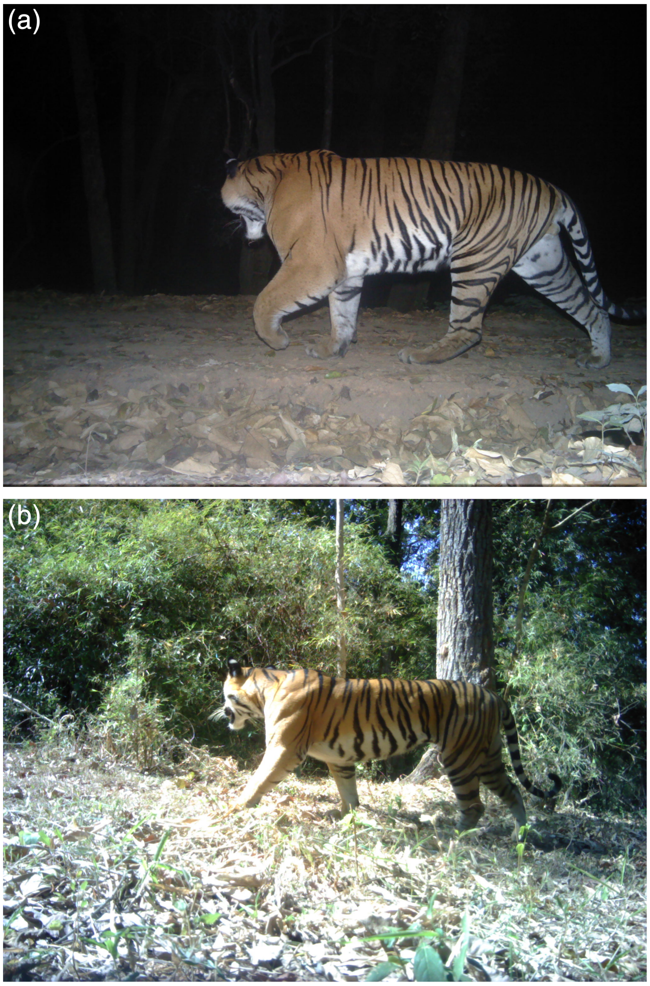

FIGURE 3 Photo-trapped images of tigers on contrasting substrate types. A dusty substrate (top) is conducive for detecting tiger tracks yielding a high detection probability p. In contrast, it is virtually impossible to detect tracks of tigers on leaf-littered, grassy, substrate types (bottom) yielding a low detection probability p. Picture courtesy of Ullas Karanth/Wildlife Conservation Society (WCS)

2001). And α is influenced by within-site spatial sampling as well as individual effects (Williams et al., 2002). For example, in a photographic capture–recapture study, Karanth et al. (2004) demonstrated that detection probability α was much higher for tigers in the denser forests of Tadoba (0.174) and Bhadra (0.22), compared to the open forests at Panna (0.039), or Bandipur (0.055) sites, perhaps as a result of differing trail densities. Marques et al. (2001) indirectly highlight the relevance of using a formal design-based approach to counter such random heterogeneity using advantages of "pooling robustness" (Buckland et al., 2001). Thus, the combined random site-to-site variation in α and p is likely to cause high overdispersion in such index data. The resulting implication is that if SOD is not taken into account then any estimate of tiger abundance at the national scale will not reflect the true uncertainties.

6 | ASSESSING SAMPLING-BASED OVERDISPERSION IN NTE SURVEY OF 2014

To assess the generality of this conclusion, we evaluate estimates of tiger abundance from the third NTE survey of 2014 (Jhala et al., 2015), which was not discussed in Gopalaswamy et al. (2015a, 2015b). The calibration models developed during this survey incorporated a few environmental covariates (but also indices) in addition to tiger signs as explanatory variables to model tiger density. By the measure of *relative importance* of covariates (see Burnham & Anderson, 2002), the survey results confirm that tiger sign index is the most important predictor of tiger density as this covariate is featured in three models developed with independent samples. This should not be surprising because this is a known relationship: (for the binomial case; see Appendix S1). Whether treating tiger sign index as a covariate in such modeling effort is relevant at all is a separate question worthy of independent investigation. Because, the argument is now circular and additionally it now implies that for a fixed k (effort), p^* contains all the information to describe the spatial variation in density (the state process) but since $p^* = \alpha p$, this is rarely the case as we shall see in the next section. But, what is relevant to us here is whether there is SOD present in the relationship between tiger sign index and tiger abundance by treating the models of Jhala et al. (2015) as index-calibration models.

From the intensively monitored sites in Jhala et al. (2015), the surveys estimated the *beta* coefficients corresponding to the tiger sign index covariate to be, and, where SG, CIEG, and WG correspond to abbreviated forms of samples obtained from Shivalik-Gangetic Plains, Central-Indian and Eastern Ghats, and Western Ghats, respectively. We note here that the definition of *beta* in these reports will differ from our definition of β earlier in that *beta* is meant to represent the rate of change of animal density for a unit increase in the detected sign index and will therefore be related to $(1/kp^*)$. However, our purpose is to investigate SOD and parameter covariation and these estimates of *beta* serve that purpose well enough.

The full mathematical specification of the model used in Jhala et al. (2015) is not available. However, from the model coefficients reported, they appear to be generated using the default log-linear model in the package secr (Efford, 2019) and the alternative identity-link function assumption leads to a prominent negative intercept. Therefore, the above *beta* estimates must be back-transformed exponentially for appropriate interpretation. Accordingly, one unit increase in the tiger sign index results in a corresponding *exponential* increase in tiger density of (a) 10.5% from the Shivalik-Gangetic Plains samples (b) 29.4% from the Central Indian and Eastern Ghats landscape, and (c) a large 174.6% from the samples of Western Ghats. These results immediately contradict the linear regression model results of Jhala, Qureshi, and Gopal (2011) in that there is a nearly threefold difference in the compounded rate parameter *beta* between (a) and (b), which was not the case in Jhala, Qureshi, and Gopal (2011). These results also imply that the predictive ability of the Jhala, Qureshi, and Gopal (2011) linear regression model now collapses due to high variability (SOD) between these samples. Interestingly, these estimates of *beta* indicate that the nonlinear nature of the relationship between tiger sign index and tiger density is also pronounced, perhaps indicating a strong interaction between p and/or α and N as discussed in Appendix 1.

It also becomes important to ask whether such stratifications by landscape carry any relevance. These are landscapes of several thousands of square kilometers in area and the basis for such stratification, as seen earlier, is governed by the population heterogeneity in detection probabilities (observation processes). And studies have demonstrated high within-landscape, site-to-site, variation in detection probability. For example, Harihar and Pandav (2012) report a large variation in segment-level detection probability (from 0.179 to 0.951) within a single landscape based on substrate type and protection status. Similarly, Barber-Meyer et al. (2013) report a variation in detection probability due to observer ability (from 0.22 to 0.73) and Karanth et al. (2011) demonstrate the link between site-level occupancy and segment-level detection probabilities with the use of common covariates to indicate parameter covariance. Hence, there are many factors influencing detection probability (tiger density, substrate type, forest types, protection status, observer type, and/or their co-varying combinations) and stratifying by any one factor appears to be insufficient. We also note that all the samples for conducting index-calibration experiments (2 experiments from Gopalaswamy et al., 2015a, 2015b and 3 experiments from Jhala et al., 2015) are from protected areas, so from the standpoint of survey design these samples are already potentially biased.

The preferable way of treating these detection probabilities, if possible, is by actually accounting for them in the estimators at the design stage in joint likelihood models (e.g., Conroy et al., 2008) and refining them with on-ground covariates. But if they are treated as random effects (Jhala et al., 2015), then it calls for employing a fully design-based approach harnessing the advantages of formal spatial sampling (Pollock et al., 2002).

7 | CONSERVATION OUTCOMES

Soon after Gopalaswamy et al. (2015a) was published (about a month after India announced results of the NTE survey of 2014), some scientists and officials associated with the NTE survey called for the summary retraction of Gopalaswamy et al. (2015a) from the journal (Kempf, 2016; Vishnoi, 2015) without adequate scientific evidence. This outcome deviates from the customary scientific practice of formal and open debates in scientific forums. Regardless, after Gopalaswamy et al. (2015b) was published and subsequently the full report of the NTE survey of 2014 (Jhala et al., 2015) was made available, the scientific issues of SOD and parameter covariance in India's tiger surveys were well established. Ideally, the expected conservation outcome would have been to reanalyze data from past surveys NTE surveys to assess more realistically the underlying uncertainties around India's tiger estimates. And to re-examine the survey designs prior to the NTE survey of 2018.

Instead, and despite a growing body of scientific evidence expressing concerns over India's claims of tiger population rise (Harihar et al., 2017; Harihar et al., 2018; Karanth et al., 2016), major international conservation agencies, such as the Global Tiger Forum (GTF), Global Tiger Initiative (GTI), and the World Wide Fund for Nature (WWF), continued to endorse claims of India's success (WWF, 2016). Consequently, India's tiger conservation budget jumped from USD $70 million to $144 million in 2016 to reward this achievement (PTI, 2016). And index-based monitoring methods continued to be applied uncritically for India's subsequent quadrennial survey (Jhala et al., 2019; Jhala, Qureshi, & Gopal, 2017) and promoted by GTF and GTI for adoption in other tiger range countries (e.g., Aziz et al., 2019).

Recently, a delayed rebuttal to Gopalaswamy et al. (2015a, 2015b), Qureshi et al. (2019a), was published. Qureshi et al. (2019a) attempted to question the mathematical approach of Gopalaswamy et al. (2015a, 2015b) to compute p^*, arguing it was more appropriate to estimate *tiger sign* detection probability r. In their example, Qureshi et al. (2019a) set $r = (.1, .9)$ at two imaginary sites, each containing 10 tiger signs (let $C = 10$). They assumed that the probability of detecting presence of a tiger, p^*, was equal to 1 at both the sites. This assumption is of course incorrect. Because, using the identity (from Royle & Nichols, 2003), we get the corresponding $p^* = (.65, 1)$. Clearly, $p^* \neq 1$, for *both* the cases. This fundamental error in Qureshi et al. (2019a), negates virtually all of the resulting inferences and introduces a fresh set of scientific contradictions and confusions about India's tiger surveys. The publication in fact further *strengthens* the evidence of high SOD (Gopalaswamy et al., 2015a, 2015b) in the NTE surveys. For instance, (a) the mere introduction of r into the argument immediately refutes both the linear regression model of Jhala, Qureshi, and Gopal (2011) and the tiger sign index “stability” assumption derived from Jhala and Qureshi (unpublished data) trials (Jhala, Qureshi, and Gopal 2011), (b) the larger difference observed in r assists in identifying a larger quantum of dispersion than when using p^* during estimation from survey data, (c) since C is also usually a random quantity in practice, the variation in p^* will be larger than due to the variation in r alone, (d) the simultaneous defense of both Jhala, Qureshi, and Gopal (2011) and Jhala et al. (2015) models, as adopted in Qureshi et al. (2019a), is not possible mathematically.

We note that Qureshi et al. (2019a) was published in spite of explicit feedback (Gopalaswamy, 2019a, 2019b) provided on earlier preprint versions of the paper (Qureshi et al., 2019b; Qureshi, Gopal, & Jhala, 2018). Further, Qureshi et al. (2019a) was published at around the same time as the release of the 2018 survey results by India's Prime Minister on World Tiger Day. We surmise that, and in line with implications of Darimont et al. (2018), the timing of Qureshi et al. (2019a) assumed precedence over the need to achieve scientific coherence, in this example.

8 | CONCLUSION AND DISCUSSION

We conclude that changes in tiger population size and occupancy reported from Indian tiger surveys, which are anomalous in the context of ecological rationale (see *India's Claims on Tiger Numbers*), are results of an unreliable monitoring program (see *India's Official Tiger Monitoring Program*). Consequently, these results do not challenge or contribute to our current understanding of wild animal metapopulation dynamics in ecology. As a result, we argue that India's claims of a *doubling* of tiger population size over the 12-year period (from 2006 to 2018) are not backed by reliable scientific evidence.

We anticipate that if SOD were appropriately accounted for, the resulting variance around India's tiger population estimates would be much larger than reported. And if parameter covariance were accounted for, then the linear applications corresponding to the first two NTE surveys (Jhala et al., 2008; Jhala, Qureshi, Gopal, & Sinha, 2011) must be revised and the mean tiger abundance estimates are likely to change.

The NTE surveys also appear to be inefficient in terms of using manpower and resource use. For example, 593,882 man-days of effort were invested in the 2018 NTE survey (Jhala et al., 2019). We submit that survey design flaws (see see *India's Official Tiger Monitoring Program*) and the resulting paradoxical ecological inferences (see *India's Claims on Tiger Numbers*) inveigh against such massive resource investments.

We recognize that current claims about increases in gross tiger numbers, local range expansions and contractions, or both, and consequent population extirpations (Jhala et al., 2019) are reminiscent of patterns that came to light 15 years ago (Karanth et al., 2003). In the previous "tiger population bubble", tiger numbers were claimed to have risen to 3,642 (Ramesh, 2008) before agencies discovered that the "pugmark census" methodology failed to detect tiger extirpations in key reserves in India (Chundawat, 2018; Tiger Task Force, 2005).

We instead recommend conservation agencies to rely on annual demographic vital rates (survival, recruitment, and movement parameters), which are drivers of state variables such as density, and can be estimated reliably at key populations (Duangchantrasiri et al., 2016; Karanth et al., 2006). And, at large spatial scales (e.g., at the national scale), we recommend a shift in monitoring attention from tracking quadrennial changes in gross tiger numbers (estimated unreliably) to tracking changes in tiger range contractions and expansions (estimated reliably; Karanth et al., 2011, Barber-Meyer et al., 2013, Harihar & Pandav, 2012, Karanth & Nichols, 2017).

A recent statistical study (Dey et al., 2017) combined information from occupancy surveys and camera trap surveys at multiple scales by exploiting the occupancy-abundance relationship (Royle & Nichols, 2003) and accounted for spatial random effects by utilizing a Bayesian conditionally-autoregressive (CAR) prior in the analysis. This approach offers promise to estimate abundance at regional scales with reduced SOD and parameter covariances. However, we do not recommend it as a real-time intensive monitoring tool.

Doubling the number of wild tigers by 2022 was proclaimed as the official goal in 2010 with financial commitments of about U.S. $ 330 million pledged at the 2010 Global Tiger Summit in St. Petersburg (Watts, 2010). We worry that such large financial investments to meet ecologically unrealistic goals (Harihar et al., 2018; Karanth et al., 2016) may have created social pressure or motivation bias (Darimont et al., 2018; Kahneman, 2011; Kunda, 1990) on tiger conservation and impacted NTE survey designs, inferences, or both. In this context, we argue that while claims about population changes of iconic mammals based on unreliable scientific evidence may assist in short term fundraising, they will be seriously detrimental in the longer term because they promote the most *advertised* conservation strategies in contrast to the most *effective* ones. For example, the sudden change in the tiger occupancy-abundance pattern observed after the NTE survey of 2010 was initially attributed to conservation measures taken at the landscape scale (Jhala et al., 2015). However, Jhala et al. (2019) now indicate that this change may also be attributed to data errors during fieldwork (especially in species assignments during sign surveys) and subsequent analysis, which are apparently addressed with the use of a new mobile application. We worry that such post hoc, and often conflicting, inferences may even percolate into mainstream scientific literature, owing to the widespread interest in popular media about these claims (Ratcliffe, 2019), and vitiate ecological and conservation science itself.

We found it very challenging to untangle methods and models used in the NTE surveys, because of the opacity of the available reports. This lack of transparency in the methods and analyses is a matter of concern, given the ongoing official promotion of the same methods to tiger range states beyond India (see *Conservation Outcomes*). There is thus an urgent need that all the NTE survey data, methods, models and the computer codes used be shared with the wider scientific community in sufficient detail to ensure reproducibility of the methods, identify and correct for possible sources of error, and lead to scientific improvements. Additionally, we call for a shift from the current *peer participation* process, which involves a cursory inspection of the NTE survey effort (usually conducted towards the end of the survey) and an accompanying endorsement (see Jhala et al., 2019) to a more formal and thorough *peer-review* process that encourages critical questioning of the scientific hypotheses, methodologies, the data gathering, and analytical mechanisms, from the start to the end. We believe that such measures of transparency will ensure that monitoring programs of threatened iconic species are based on well-defined scientific and management objectives (Nichols & Williams, 2006) so that they assist conservationists in real time and will also avoid iconic mammal populations from being stigmatized as political populations (Darimont et al., 2018).

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CONFLICT OF INTEREST

The authors have no conflict of interest.

AUTHOR CONTRIBUTIONS

All authors conceived and designed the study. A.M.G. synthesized and analyzed the data. A.M.G., M.D. developed the statistical arguments. All the authors wrote the first draft of the article. All authors contributed to subsequent revisions of the article until finalization.

DATA AVAILABILITY STATEMENT

This article was based on publicly available estimates and all the sources are appropriately cited in the article. It did not require the collection of empirical data outside of the synthesis of existing published studies or reports. Therefore this work required no special permits outside of basic ethical guidelines for the production of science.

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