# SCIENTIFIC AND TECHNICAL ISSUES INVOLVED IN MONITORING TIGER POPULATIONS IN INDIA sent on July 12, 2005.

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The subject of monitoring changes in animal populations across space and time (at different places and/or at different times) to either answer biological questions or to assess management effectiveness, is a well developed field of scientific endeavor with a substantial accumulated body of theoretical and practical knowledge on both the ecological and statistical issues involved (Seber 1982, Williams et al. 2002). Rather than trying to reinvent this scientific wheel, my recommendations herein are based on this existing strong foundation of peer-reviewed science. Therefore, I have chosen to examine the tiger monitoring issue in its entire scientific and historical context in India, and not merely restricted myself to commenting on the non-peer reviewed, unpublished, "Technical Note" (Jhala et al. 2005) and "Field guide" (Gopal et al. 2004) sent to me by the Tiger Task Force (TTF) for comments.

## THE TRADITIONAL PUGMARK CENSUS (TPC) –VERSUS- TIGER POPULATION SAMPLING (TPS): THE FUNDAMENTAL DIFFERENCE

At the very outset the TTF must recognize the *fundamental difference* between the tiger monitoring approach adopted since 1960's by Project Tiger (the total count pugmark census of late S. R Chouhdury and embellishments made to it subsequently by other forest officers – i. e, "the traditional pugmark census" which I will call TPC here) on the one hand, and, all other tiger monitoring methods that are based on the concept of *population sampling* rather than *census or total counts* on the other.

The note by Jhala et al. (2005) is strangely silent on this fundamental difference although, the note itself appears to squarely rest in the paradigm of *population sampling* wherever the writing is clear enough. However, the present Director of Project Tiger, who is also a co-author on this note, is on record saying that that the traditional pugmark census (TPC) is quite an effective method and will continue to be used. But technically a *census* assumes that *detection probability* and *sampling fraction* are both equal to 1, whereas *sampling* methods recognize that this is almost never true, and go on to address the problem of estimating these (Williams et al. 2002; Karanth et al. 2003). So whatever else it may or may not be the note by Jhala et al. 2005 is not based on the concept of *census* as far as I can discern.

To set the stage for my analysis, it is necessary to briefly present some background history. As a result of widespread scientific criticisms of the TPC (summarized in Karanth et al. 2003) and following internal discussions within MOEF during 1996-97, with scientific inputs from several scientists and managers, fresh guidelines were issued by the Directorate of Project Tiger for tiger monitoring on 28-4-1997. Subsequently, a major international technical workshop of experts was organized in partnership with the

Directorate of Project Tiger in January 1999 to discuss this issue, resulting in a comprehensive technical manual (Karanth and Nichols 2002). This workshop was inaugurated by the then Hon. Minister for Environment, GOI and had participation of national and internationally recognized tiger biologists, statisticians, senior officials of MOEF, tiger conservationists including biologists from Wildlife Institute of India (WII) including Dr. Jhala and Mr. Qureshi. The workshop was followed by a high-level, wide-ranging consultations held under the chairmanship of Secretary MOEF in 2001. Based on the consensus arrived therein, a final set of guidelines on tiger monitoring, approved by Secretary MOEF, were issued on 15-6-2001. These were directly linked to earlier detailed guidelines issued on 28-4-1997 and 22-5-2001 by Project Tiger.

These earlier guidelines clearly specify that the inaccurate term "tiger census" should not be used at all and also specifically stated that exact numbers of tigers should not be generated for tiger reserves by managers based on the TPC method. It also strongly recommended a switch over to sampling-based methods for monitoring tigers and prey species. However, these guidelines issued by the then Director of Project Tiger were not at all followed by field managers, who continued to employ the TPC and generate unreliable numbers at Sariska and other places, until the present crisis exploded. Even during the recent tiger crisis in Panna, Ranthambore etc. it is seen that TCP is being employed by Project Tiger and local managers and even by NGO conservationists.

If indeed it is now once again the opinion of MOEF-Project Tiger authorities that generation of tiger numbers from "total counts based on pugmark census" (TCP) a valid scientific approach (contrary to earlier guidelines) this should be stated by them explicitly before the Tiger Task Force. Furthermore, if MOEF really believes that TCP is works well, it automatically means that almost every tiger (within a reserve, state or country) can be identified individually and its fate tracked across time and space at a very low cost and effort. If this is indeed the case, all relevant tiger demographic parameters including occupancy, population size, density, mortality, survival and dispersal can be easily be estimated from TCP data. In other words, there is no need at all for employing the various alternative sampling methods (such as those of Karanth and Nichols 2002 or for that matter of Jhala et al. 2005) to monitor tiger populations in India.

I am therefore quite puzzled why sampling-based methods like mark-recapture and indices are recommended by Jhala et al. (2005), if the entire population of tigers (or a large fraction of it) can be individually counted using the traditional pugmark census (TPC). If that is indeed the case, instead of investing large sums o money in these more expensive, intensive alternatives, the WII scientists engaged in this exercise should be immediately asked to analyze the data from past pugmark censuses right from inception of Project Tiger and derive all demographic parameters for each reserve. Why is this not being done by Project Tiger? The reason for this basic paradox is buried in the technical note, hidden from the public scrutiny. These are elucidated below for the benefit of TTF.

Several leading scientists have thoroughly criticized TPC and pointed out its numerous failures and impracticalities (See Karanth et al. 2003 for a detailed peer-reviewed critique of TCP, which has not been rebutted by Project Tiger or anyone else in the peer-reviewed

literature). In fact, two leading scientists of WII who had done the most work on tigers at that point in time, (Dr. AJT Johnsingh and Dr. RS Chundawat) are co-authors on this paper, which essentially argues that TCP is demonstrably failure prone and suggests deployment of more appropriate sampling based alternatives for monitoring tigers.

Furthermore, Dr. Y. V. Jhala, now the lead WII scientist engaged in the present collaboration with Project Tiger, and, who is also the lead author of the technical note (Jhala et al. 2005), has confirmed to me in person verbally, and to others in a written communication, that: **"Neither me or any scientist in WII is corroborating or backing the numbers that have been reported earlier using the pugmark census technique – We are not advocating any method based on total counts"**. When I presented this statement in my audiovisual submission made before the TTF at Delhi in May 2005 (attributing the statement as my own opinion), Dr. Jhala and Mr. Qureshi, two of the co-authors of the technical note present did not express any disagreement with it at all.

It should be clearly noted that whatever its other merits and demerits are, the note by Jhala et al. (2005) recommends collection of tiger pugmarks only as a part of large-scale regional occupancy and encounter rate *sample surveys*. In these occupancy surveys there is no attempt to identify individual tigers from shape of their pugmarks as in TPC.

The note only recommends that attempts should be made to identify individual tigers in smaller intensive areas with appropriate substrates etc. by employing a statistical technique of track shape discrimination based on multivariate analyses (Sharma et al. in press) to identify some individual tigers *only for use in the subsequent estimation of tiger numbers using the standard capture-recapture sampling* approach. This type of use of tiger pugmarks *in a sampling-based estimation* protocol is not a "refinement" of the traditional pugmark *census* (TPC) as is being claimed by Project Tiger and MOEF authorities.

In fact statistical track discrimination approach uses the tiger identities (if it works at all) in the same manner as identities based on photos or DNA are used in CR sampling. On the other hand TPC is totally divorced from the concept of sampling. The fact is that there is a major technical disagreement on this key issue of census versus sampling between WII scientists and Project Tiger/MOEF. This is a fundamental disagreement, which Jhala et al. (2005) appear to consistently hide and obfuscate for reasons that are not clear to me.

Therefore, TTF should note that the persistent claims being publicly made by the Directorate of Project Tiger and MOEF officials that WII scientists are "refining" their Traditional Pugmark Census (TPC) are disingenuous and misleading. The note by Jhala et al. (2005) is fundamentally based on the concept of *population sampling* and does not advocate *total counts or censusing* of tigers. The technical note artfully masks this fundamental difference through clever verbiage, obfuscation and by setting up the straw horse of photo trapping versus sign surveys, to divert attention from this key area.

**Therefore, I believe there is a total scientific consensus that the method of obtaining total counts of tigers in reserves, states, regions using the 'traditional pugmark census' (TPC) - has no scientific support from any quarter in the realm of peer-reviewed science, including among WII scientists involved in the present collaboration.** However, unfortunately this is failure-prone, unscientific census method is the one that is being still used widely by the entire forest service establishment in India. It is also being taught officially to managerial trainees at the WII (although not by wildlife scientists in the faculty) and was recently applied at Panna reserve to claim that there are 35 tigers in order to discredit serious scientists who contested that untenable claim based on past data on radio-telemetry and camera trap sampling.

**Therefore, the TTF must make a clear and categorical recommendation that the traditional pugmark census, which is not all based on the concept of population sampling, and has proven to be failure and manipulation prone - should be totally abandoned. Given the history of how a blind "loyalty" to this method has blocked the introduction of scientifically valid tiger population *sampling* methods for over 30 years in India, TTF will not be able to initiate any reform in the Indian tiger monitoring system unless this flawed and discredited TPC approach is abandoned totally and explicitly. Attempts in the past to "reform" and "refine" the tiger pugmark census (TPC) method in response scientific criticisms (in 1994 and again in 1998) have only led to its persistence in the field under a cloud of disingenuous pseudo-scientific verbiage.**

## COMMENTS ON SAMPLING-BASED APPROACHES TO TIGER MONITORING

I have been provided by TTF with the following two documents for my comments:

1. Monitoring Tiger Status and Habitat: Technical Note (2005). Jhala Y. V, Qureshi Q and Gopal R. I am hereafter referring to this as Jhala et al. 2005 for brevity.

2. Monitoring Tiger Status and Habitat: A Field Guide (2004). Gopal R and 10 other contributors including Dr. Jhala and Mr. Qureshi, hereafter referred to as Gopal et al. (2004).

Based on arguments I will put forth below, I believe both these note by Jhala et al. (2005) and the Field Guide by Gopal et al. (2004) contain serious flaws and cannot be used as blue prints for monitoring wild tiger populations in India.

Furthermore, I note that all these technical issues of tiger monitoring have been earlier covered extensively in a technical edited volume: (Karanth KU and Nichols JD (Eds). 2002. Monitoring Tigers and their Prey: A manual for researchers, managers and conservationists in tropical Asia), which I will refer to here as Karanth and Nichols (2002).

**Specific Comments on Jhala et al. 2005 ("Technical Note")**

**1**. Before I get into specific detailed comments, I must express my strong philosophical disagreement with the following statement (Page 5), which appears to be the thematic guideline underlying this document: **"The stage 2 of the proposed methodology answers the question of: How many tigers and ungulates are there? It basically caters to the numbers game. The numbers are needed for reporting to public and politicians, they are not essential for population and site specific occupancy trends"**. I most emphatically disagree with this rather cavalier approach to animal population analysis and management. Practice of modern wildlife biology and monitoring within an adaptive management framework emphasize the need for rigorous estimates of variations of animal abundance, and of the vital rates that drive them, across both space and time. These estimates must take into account the central issues of spatial sampling and detectability (Williams et al. 2002). Jhala et al. (2005) seriously underplay the importance of estimating detection probabilities and relies primarily on *samples* from raw field counts and encounter rates to monitor tiger and prey populations and make management decisions. This basic approach to animal abundance estimation (of not worrying too much about detection probabilities) for making decisions is about fifty years out of date.

If we accept this logic underlying the above cavalier statement that tiger and prey numbers are simply meant for public consumption, and occupancy data (tiger presence or absence data) alone are sufficient for effective monitoring and management, then we will have many more Sariska like disasters on our hand in the future. We will know the truth about the tiger's status only when the occupancy parameter hits a value of zero – when all the tigers are gone. I am quite surprised that such a patently absurd proposition forms the fundamental basis for the carrying out the monitoring prescriptions Jhala et al. 2005. surveys.

**2. Page 2:** In the second paragraph, it is stated that "the alternative" to estimation proposed in this note proposed by tiger biologists is to use camera-traps. On the contrary Karanth and Nichols (2002: page 4) propose four possible objectives of tiger monitoring programs. Objectives 3 and 4 indeed require intensive (e.g., camera-trapping) studies, but objectives 1 and 2 do not require as such effort and seem to have been merely repackaged and presented by Jhala et al. 2005. Again, my objection is not to the suggested use of the methods, but to the apparent straw man being erected (Karanth and Nichols vs. Jhala et al. 2005), whereas the former had recommended occupancy estimation and relative abundance methods as far back as in 1999 at the original workshop in which Dr. Jhala and Mr. Qureshi were present.

It is also stated in this paragraph that it is not possible to extrapolate photographic capture-recapture densities beyond the small study areas, whereas the Karanth et al. (2004) paper showing a relationship between prey and tiger densities provides some basis for such extrapolation, in cases where prey densities can be estimated. It is also to be noted that in any sampling-based estimation, including those proposed in the note by

Jhala et al. (2005), scope of inference is restricted to areas thought to be represented by the sample. It is also implied in the note that that camera trapping cannot be used for temporal population monitoring, whereas Karanth et al. have done such monitoring and analyses for over 10 years at Nagarahole and they work fine. This result has been presented at meeting of the Wildlife Society a few years ago and is now being written up.

I have 2 objections to the statements about coefficients of variation (CV's) of abundance estimates and their inability to estimate or detect a trend. My first thought is that placement of trend detection in a hypothesis-testing context is not a smart way to manage or conduct conservation. If we are serious about management/conservation we should never rely on trend estimates or hypothesis testing, but would use a decision-theoretic framework that yields optimal (or at least smart) decisions (please refer the last quarter of Williams et al. 2002 book). My second thought is that using separately estimated abundances to estimate trend reflects a worst-case scenario. It will frequently be possible to share parameters (e.g., constant capture probability *p* over years within a location or constant rate of population change [this is the trend assumption anyway]) and obtain more precise estimates of trend, than those based on ratios of abundance estimates. Actually, there is no statement in this technical note about exactly how trend is to be estimated or tested using the data proposed to be collected.

**3. Page 3:** The stage I material on spatial mapping and monitoring corresponds to the first 2 objectives of Karanth and Nichols 2002 (also see chapters 5-8). Because the field situation with respect to marked forest beats on ground varies greatly over India, conducting geo-referenced sign surveys and then superimposing a grid for occupancy estimation may be a smarter and statistically better way to do this.

In the second paragraph, it is stated that time series analyses will be used for monitoring occupancy, whereas I believe that the approaches presented by Mackenzie et al. (2003; 2005), and tailored for multi-year inference, are more appropriate for estimation and modeling with covariates. As a point of interest, it should be noted that the single-season methods of Mackenzie et al. (2002), Royle and Nichols (2003) and Royle (2004) cited by Jhala et al. 2005, were anticipated and preceded by Karanth and Nichols (2002), who suggested this framework for measuring occupancy in tigers. So again, I think the basic selection of methods is good for the single-season approaches, but not for the multi-season approach. I also do not understand the lack of citation in this context of Karanth and Nichols (2002) who developed these approaches originally and specifically for tigers.

**4. Page 4**: The claims about having "tested the above methodology" are exactly the sorts of things that I worry about. The occupancy estimation methods cited on the previous page were not implemented, but only simple maps based on raw data summaries are provided in Jhala et al. 2005. There is no attempt to deal with the issue of false absences (locations where tigers are present but were not detected) in the development of maps, whereas this is the major reason for development of the cited methods of Mackenzie et al. (2002), Royle and Nichols (2003) and Royle (2004). A common failure of monitoring programs stems from the design of programs by scientists who have never tried to analyze the data resulting from their programs. Jhala et al. 2005 have a merely identified

appropriate methods for measuring occupancy during a single season, but these should be used with the test data (and perhaps simulated data), so that any large-scale employment of the design is based on knowledge of how to use the methods and to expect what kinds of estimates are produced by them. At the moment Jhala et al. 2005 have merely presented some raw data, and no estimates of occupancy parameter have been presented. As such, their claim that this methodology has been tested is premature.

**Pages 5-7**: Capture-recapture sampling based on pugmark-based identifications of tigers is recommended as one of the intensive tiger density estimation techniques. I view this sort of capture-recapture sampling as similar to DNA-based sampling from scats, in that identification errors are possible. Because of this, I recommend that the authors consider, and experiment with, the models of Paul Lukacs and Ken Burnham designed to deal with errors in identifying individual animals. It is very nice that methods are now available to deal with such identification problems (imperfect assignment of tracks to individuals), but they carry a huge cost in terms of extra parameters and thus precision. My point is that there are promising approaches, but that they should not be embraced naively, but with full knowledge of costs and benefits. Again, it is difficult to say much about estimation methods as no details are presented in this note. The statement about trap shyness causing bias in abundance estimates is not reasonable, as capture-recapture models $M_{b \text{ and }} M_{bh}$ were developed specifically to deal with behavior and yield unbiased estimates in the presence of such behavioral response. Just what sort of trap shyness is expected to produce bias and why do the standard trap response models not deal with it?

Secondly, the identification of individual tigers based on statistical track discrimination is fraught with several problems not recognized by Jhala et al. 2005. The central issue here is the one of establishing the applicability of the multivariate statistical protocols recommended by Jhala et al. 2005 (based on Sharma et al. in press) to identify a small number of captive individual tigers under controlled study conditions, in the field. Since this multivariate DFA approach is usable only when number of groups involved are defined a priori (when number of individuals are already known as in captive studies), it is not clear to me how such a *supervised classification system* can be applied to an unknown population size. The note also lacks comparisons with similar approaches developed in the past, most notably the two *unsupervised* multivariate methods (based on neural networks and Bayesian auto-class) used by Riordan (1998) that do not need this a priori information. It would be interesting to see how these other approaches would perform. I also believe that based on these controlled studies alone (Sharma et al. in press) the authors cannot automatically assume that these methods would work under field conditions, where the size and complexity of the foot print, substrate, and other logistical issues (detailed by Karanth et al. 2003) could pose problems that are not encountered under controlled conditions. Extensive work by Carloyn Miller (unpublished data) to attempt similar pugmark-based identifications using multivariate methods for jaguars have not worked out well at all in the field.

**Pages 6-7**: The methodology proposed for estimating prey numbers using distance sampling (Buckland et al. 2001) presented here is seriously flawed in several ways. The modern survey designs insist on at least 20 spatially replicated transects that are placed

over the sampled area in either using systematic, or random sampler geometry with a random starting point. The proposed use of purposive placement of transects, with beats or compartments serving as basic units for placing transects, violates this basic requirement of proper survey design making it impossible to apply results of the inference beyond the specific line transect itself. Alternately, if the suggestion is to have 20+ spatial replicate transects in each sampling unit (beat or whatever) then authors clearly have no idea of the amount of effort involved in carrying out proper line transect surveys.

The approach suggested to develop detection functions using cardboard cutouts of animals is simplistic and outdated (this was used in some very early transect work involving colored stakes by Jeff Laake and beer cans by Otto for some simulation work: See Burnham et al. 1980 for details). The actual process of animal detection and the shape of the real world function that generates the effective strip width (or the average probability of detection within the strip) result from a set of complex biological stochastic factors, and can be modeled best by actually measuring distances to real animals under field conditions. Cardboard cutouts cannot be substitute of live animals for generating such detection functions. However, having trained hundreds of forest department staff as well as motivated amateur naturalists over a 16 year period, in line transect surveys, I know that it is quite feasible to generate valid detection functions and effective strip width (ESW) from actual distance data collected in the field using range finders or distance marker flagging placed in the field. Even if a sub-set of the data is used for estimating the ESW, it can then be applied to a larger data set collected by observers who record only raw counts to derive rigorous estimates of prey densities.

I most emphatically disagree with the statement that (page 6 last paragraph) that encounter rates along transect lines would be sufficient for provide ungulate population trends in an area. Without accompanying estimates of detection probabilities, such encounter rates in forested habitats are merely random count data, which may or may not have any relationship to real ungulate densities. Such encounter rates cannot be a useful tool for management. I would argue that the detailed line transect survey approach described in detail elsewhere (Buckland et al. 2001, Karanth and Nichols 2002 Chapters 9 and 10), provides a far superior way for estimating ungulate densities whether one uses forest staff or other sources of man-power.

**Pages 7-8**: In Stage III a, A lot of sampling is recommended without a procedure for translating collected data into an estimate of something of interest. I believe that the robust capture-recapture design recommended by Karanth and Nichols 2002 - now Karanth et al. (in prep.) is useful for monitoring abundance, vital rates and rates of population change in intensively studied tiger populations. In less intensively studied areas, characterization of occupied locations as containing evidence of breeding or not (where absence of evidence includes locations with and without breeding [there is uncertainty that is estimated and dealt with]) can be used with newly developed occupancy estimation approaches to estimate proportion of areas (1) occupied by breeding tigers, (2) occupied by tigers with no breeding, and (2) not occupied by tigers (Mackenzie et al. in press).

However, among these various proposed alternative methods suggested for rigorous estimation of tiger abundance in key reserves, as of now only capture-recapture sampling based on camera trap photos has been scientifically peer reviewed, published and detailed protocols and training manuals are available to implement this method. It is a method that has worked reasonably well in tiger reserves with tiger densities of 2-17 tigers per 100 $km^2$ (Karanth et al. 2004), some of which are currently under threat from poachers. Even more importantly, photographic capture-recapture sampling method can be used to estimate survival rates and other demographic parameters of tigers for assessing poaching impact and other factors, which cannot be assessed from sign-based indices or raw counts. Most importantly these tiger photographs cannot be artificially 'created' like pugmarks through dubious manipulations and can be more easily verified independently.

The camera-trap photographic monitoring approach has been used for over 12 years successfully in Nagarahole-Bandipur on tigers and has also been used in 10 other tiger reserves in India in partnership with the respective forest departments (Karanth et al. 2004).

These photographic records of tigers also have a unique potential for forensic uses (unlike pugmark tracings), while dealing with wildlife crime: if tiger skin seizures take place and the skins are found to match past photos of live tigers in important tiger reserves, early warning as well assignment of locations/responsibilities for failures will become feasible. Yet Project Tiger Directorate has not shown any serious initiative to try out this method within several key reserves, even where it is quite feasible to use this method.

Managers often come up with a strange reason for not using camera traps: the units will get stolen or tampered with by intruders. Now protective devices are available to prevent such theft or vandalism. Further this argument begs a question: if intruders can enter your tiger reserve with impunity and steal cameras, the same intruders can even more easily set traps and snares for tigers and poison tiger kills. Clearly such an inability to deploy camera traps is in itself the first warning that protection is collapsing in that reserve. In 12 years of camera trapping in Nagarahole reserve in Karnataka involving thousands of trap nights of effort I have had only a couple of instances of theft, which were promptly recovered by the vigilant park staff.

Therefore, the reluctance of many managers to accurately document tiger populations in their reserves using camera traps may have less to do with technical issues and more to do with a desire to cover up managerial failures. This must be recognized by TTF and dealt with as a major management problem afflicting that specific reserve.

**Specific Comments on Gopal et al. 2004 ("A Field Guide")**

My earlier comments on Jhala et al. 2005, technical note are very much relevant to this document also, but I will not repeat them here, and instead make detailed specific comments.

**Page 3**, **Introduction**: I disagree with the statement that monitoring programs developed by individual scientists and organizations "would serve only academic purposes". I believe that some of the best work in distance sampling and photographic capture recapture sampling done by Centre for Wildlife Studies and Wildlife Conservation Society in India since 1986, has been far from academic and, to the contrary, has been applied at over a dozen sites in India (Karanth et al. 2004) in studies that involved amateur naturalist volunteers and forest department staff in data collection. Theses methods are now being used to monitor tiger populations in Thailand, Malaysia, Indonesia and Myanmar successfully. They have been adapted for use and are being applied widely for monitoring of other naturally marked species such as jaguars in several countries, leopards in central Africa, cheetahs and wild dogs in South Africa through our collaborative projects. I agree with the authors that the government machinery of the forest department should be harnessed (assuming that this is possible) for large-scale, regional spatial occupancy monitoring work, and my simple recommendation is for this program to be designed by the right group of investigators so that that public resources are used optimally and not inefficiently.

**Page 9**: The description associated with the 7$^{th}$ bullet point was not entirely clear to me, so I am guessing it would not be clear to field workers either. Basically replication is needed in order to deal with detection probability (tiger sign is often not detected at all locations at which it is present). There are several possible designs by which such replication can be achieved (Mackenzie et al. in press). But issues of whether there is overlap in the space covered during the replicate visits, whether knowledge and memory of previous findings of sign are used in the estimation, whether the same or different personnel are used, are all important to the modeling of the data. I won't go into the different approaches as they will depend on the answers to these questions, but simply suggest that such efforts should involve someone who understands the sampling and modeling approaches that are available.

**Page 14-15**: I made this comment in greater detail on the technical note as well, but it strikes me that given all the effort going into this sampling, standard distance sampling may require less effort and training and produce more scientifically defensible results.

**Page 19**: The sampling for human disturbance and vegetation etc. is fine, but I note that this appears to require a huge amount of effort. This is fine too, and I only relay my experience with other studies that there should be a very good reason for collecting the extra data. The data should be very relevant to tiger conservation and associated modeling to warrant the effort. Authors should also have a clear idea of exactly how such data are to be used in an overall analysis. I know this sounds obvious, but it is surprising how frequently this is not true of monitoring programs I know of.

**Page 20**: The material on ungulate pellet count sampling mentions nothing about replication. Is this to be done once each year? If more frequent sampling is envisioned, then is the sample area to be swept clean of pellets after each visit? Or decay rates to be

measured instead? Details such as this are important to design, implementation, and analysis of such studies.

Another important problem with dung counts not recognized by the authors is that decay rates differ vastly between species, sites, between seasons etc. Several effective ways have been developed to estimates of decay rates (e. g. Marques et al, 2003) that authors seem to be unaware of. The point being that dung counts uncorrected for decay rates are not very useful even as indices of prey abundance.

**SOME CONCLUSIONS**

1. While some of the tiger monitoring estimation methods proposed (Jhala et al. 2005, Gopal et al. 2004) for being tried out have some potential value, none of the ideas that are valid among these are new or original. The key ideas in these notes: (1) Occupancy mapping at large spatial scale (2). Generating tiger and prey abundance indices at reserve levels where estimation is not possible (3) Estimation of prey densities using line transects and (4) Estimation of tiger densities using photographic capture recapture sampling are all topics that have already been far more comprehensively covered by Karanth and Nichols (2002) manual three years ago. Many of these methods have already been implemented in Karnataka and Maharashtra in collaboration with the State Forest Departments. The other aspect of a pilot occupancy-mapping project is currently underway in Karnataka. I will be happy to share this knowledge and experience with Project Tiger authorities at the national level and assist them in improving their proposed monitoring protocols.

The most important thing for the TTF to note is that that nothing contained in these two notes given to me, is an endorsement of the traditional pugmark census (TPC), which needs to be discarded immediately as a matter of policy.

2. A key national need now is to link the best possible rigorous, sampling based tiger and prey population estimation methods to the actual tiger management policies/interventions within a formal adaptive management framework, using decision theoretic approaches (Williams et al. 2002). This approach has yielded substantial gains to conservation in waterfowl management in USA. I believe this is a key gap that needs to be addressed, by involving world's top specialists in the field of biostatistics, decision-theory, model selection and parameter estimation, like Byron Williams, James Nichols, Dave Anderson (USGS), Steve Buckland (University of St. Andrews) and others. The forum of a formal technical workshop would be ideal for this. It is important to have also the participation of the world's leading tiger biologists also by involving the IUCN's Cat Specialist Group with this workshop.

3. For achieving real progress on ground in tiger monitoring (going beyond mere ideas), I strongly believe that the past practice of tiger monitoring activities being monopolized by the government should now be abandoned. Government-sponsored and implemented tiger monitoring has basically failed in India for thirty years. As in other applied fields of agriculture, medicine, weather prediction, geologic mapping etc. time has come for the

government to restrict its own dominant and stultifying its role, and allow a free play of open science in the arena of wildlife monitoring.

If the vast machinery of the government forest department can be effectively mobilized to map tiger distribution and occupancy at regional and national level once in five years using reliable, rigorous methods that is the best that can be done. Rest of reserve level and site level tiger monitoring responsibilities should be entrusted to qualified scientists and managers where they have all the necessary resources at their command (at least in important critical tiger reserves). Much better results can be achieved compared to the past thirty years of countrywide government monopoly over tiger monitoring if such an open policy is adopted.

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