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Original research article

Need for longitudinal studies of Asian wildlife in the face of crises

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HIGHLIGHTS

- Longitudinal studies of wildlife are not as prevalent in Asia as elsewhere.
- Regulatory frameworks lack adequate data for protecting species.
- Examples of longitudinal assessments show their utility for conservation.
- Researchers should try to publish datasets & results in journals or repositories.
- Funders should diversify support and opportunities for longitudinal research.

GRAPHICAL ABSTRACT

Clockwise from top: *Hippocampus sindonis*, Shiho's seahorse (Photo: Honutomo, cc-by nc 4.0); *Otis tarda*, Asian Great Bustard (Photo: Martin Gilbert, all rights reserved); *Elephas maximus*, Asian elephant (Photo: Shermin de Silva, cc-by nc 4.0)

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ABSTRACT

Conservation biology is conceived as a discipline that must balance the short-term aim of gathering data on pressing conservation issues with the long-term vision of protecting populations, species, and functional ecosystems. Asian wildlife face imminent threats ranging from hunting to loss of critical habitats, but there are few examples of detailed longitudinal wildlife research in the region. Longitudinal research is essential for protecting populations especially in light of the high volumes of legal and illegal trade, understanding basic population dynamics, notably with respect to long-lived species, as well as accommodating the spatial needs of animals. It is also critical for evaluating the success of conservation or management interventions and adaptively improving outcomes. Such studies, particularly when requiring sustained field work, are impeded

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by mismatches between needs on the ground vs. the priorities of different stakeholders, the ephemeral and inefficient nature of funding mechanisms, and by the logistics of maintaining sites and personnel. Yet we cannot adequately protect biodiversity in Asia unless the magnitude of human impacts on its species is quantitatively understood and used to inform management.

"Although crisis oriented, conservation biology is concerned with the long-term viability of whole systems".

– Soulé (1985), *BioScience*, "What is conservation biology?"

"At times we seem to be documenting paths to extinction, telling ourselves that we need to do more research, developing theoretical models with insufficient consideration of their practical application, and giving each other advice on what others should be doing. If that is the limit of our expectations then conservation biology is succeeding as a field. But if we are intent on holding back the forces driving extinction, then we are failing in a major way".

– Whitten et al. (2001), *Conservation Biology*, "Conservation Biology: A Displacement Behavior for Academia?"

1. Definition and utility of longitudinal studies

Since its inception, conservation biology was characterized as a "crisis discipline" with the mission of providing the scientific basis for conservation decisions, responsive to immediate needs but with an eye toward the distant future (Soulé, 1985). In many regions, this ideal is overshadowed by the grim reality that data are often still inadequate, or worse, ignored (James et al., 1999; Rabinowitz, 1995; Sodhi et al., 2004). Confronted with the leading edge of Anthropocene mass extinction, conservation efforts often resemble attempts to plug the proverbial broken dam, which springs new leaks as soon as one is fixed. Conservation research as well as management decisions tend to occur in reaction to imminent threat. However, a longer perspective is required if our collective aim is to facilitate the persistence of species, including our own.

Currently conservation and ecological research published in leading journals is biased toward study sites based in wealthy and temperate countries (Martin et al., 2012). Yet in Asia, home to both the largest human populations and some of the most threatened ecosystems, the rapid pace of environmental change is most pronounced (Laurance, 2004a,b) and has been termed an "impending disaster" with respect to the loss of biodiversity through both deforestation and defaunation (Harrison, 2011; Jepson et al., 2010; Kurten, 2013; Laurance, 1999; Sodhi et al., 2004). Here I discuss reasons why we need greater investments in longitudinal research efforts on Asian fauna and the current challenges for doing so, with examples based on experience and literature.

Longitudinal research constitutes efforts to study a particular system in a repeatable and systematic way that are spread out over two or more time points, as opposed to one-time investments. By this I do not mean multiple independent studies but systematic, sustained efforts to understand the behavior of a system using a specific protocol. Some monitoring programs may constitute longitudinal research, if they are conducted in a consistent and scientifically justifiable way. The importance of longitudinal ecological studies is well-understood, especially with regard to understanding the long-term dynamics of ecosystems (Condit, 1995; Kim, 2006). The 50-acre plots which form the network of forests monitored through the Center for Tropical Forest Science and Forest Global Earth Observatories (CTFS-ForestGEO) are classic examples of long-term monitoring efforts. In Asia there are already a number of International Long Term Ecological Research (ILTER) sites. More generally, understanding the basic biology of long-lived species, especially if they reproduce slowly, requires great time investment (Condon et al., 1994). This is no less true of wildlife populations than for the ecosystems they are part of. Clutton-Brock and Sheldon (2010) identified at least six benefits of long-term individual-based research on wildlife: the ability to analyze age structure, link life history stages, link generations, quantify social structure, derive lifetime fitness measures and replicate estimates of selection. By individual-based I mean studies tracking the life histories of identified individuals in the population. At least the first three are directly relevant to conservation, and the others necessary for basic understanding of wildlife populations and their life histories. While long-term, individual-based studies may be considered a gold standard, longitudinal studies need not necessarily be long-term to be of value, so long as they are *of sufficient duration to capture a process of interest*. Ideally, this will entail multi-year timescales that adequately reflect the time needed for biological processes to occur or for the impacts of disturbance or management to manifest and be evaluated (Chapman et al., 2000; Yamaura et al., 2009). Longer studies are not always better than shorter ones, given that resources are limited. Where sufficient data are available, conservation funding and effort would be better invested in transferring available expertise to policy makers law enforcement, and ultimately into action (Knight et al., 2010, 2008). Studies also need not be based on known individuals, e.g. studies based simply on counts or occupancy, which can nevertheless provide important data on population trends.

Longitudinal studies are requisite for understanding population trends. North American hunting and fisheries records inform harvest quotas, a paradigm example of the practical application of longitudinal research in managing wildlife. There

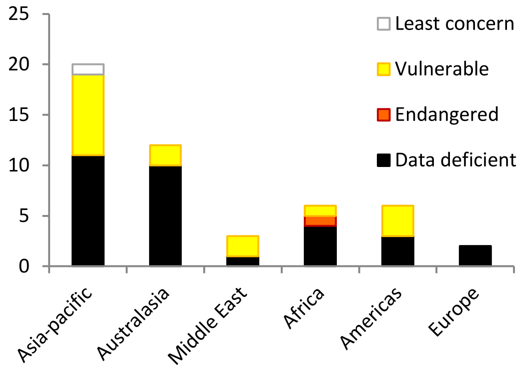

Fig. 1. IUCN Red List status of *Hippocampus spp*. Asian-pacific waters contain both the greatest diversity of known species and the most data deficient species, followed closely by Australia and New Zealand. Note that species occurring in more than one region were counted in each region.

are few, if any, such controls or standards for the multitude of species annually being harvested and sold locally or exported from Asia, especially from the southeast (Nijman, 2009). The CITES framework is a major international regulatory mechanism for wildlife harvest and trade. Periodic assessments released via the IUCN Red List form the basis of CITES guidelines. They are meant to monitor population trends but are only as good as the individual reports on which they are based. Many may be found only in gray literature that is not peer-reviewed, and thus not subject to critical examination. Coverage in the Red List is biased toward vertebrates (Stuart et al., 2010) and even then data are deficient for some heavily poached species that are cryptic, such as pangolins (*Manis spp.*), forest ungulates, and many fishes. Using CITES trade records, Nijman (2009) estimated that upwards of 30 million individuals consisting of approximately 300 species of animals were traded legally or illegally from Southeast Asia during the period 1998–2007. This included 16 million sea horses (*Hippocampus spp.*) exported by Thailand, Vietnam and China, most frequently as dried specimens. Of 54 species currently recognized in the genus Hippocampus, 40 species are on the Red List, and of these 20 are found natively in the Asian region. The majority of species are classified as "data deficient", (27) and the rest as "vulnerable" (11) or endangered (1), other than *H. sindonis*, which curiously is classified as "least concern" although there are no estimates of abundance and population trends are unknown (Fig. 1). Those assessments citing data from trade records and surveys reported declines of at least 30%. However, it is likely that there are even greater numbers being traded illegally (Nijman, 2009; Wiswedel, 2012) or failing to be appear in international trade records because they are sold or consumed locally. This state of affairs is clearly inadequate to prevent overexploitation. The impact of harvest on non-vertebrates may be still more difficult to assess. Sampling locations in which local abundances are tracked are urgently needed. The Long Term Ecological Research sites are one model for doing so, such as the reef site located off the island of Moorea in French Polynesia. Nationally sponsored LTER networks have the mandate, infrastructure and financial capacity required to host programs which track ecosystems over time, typically using standardized plots and protocols. Studies of wildlife could in principle be integrated into such sites. National Red Lists are another mechanism through which to monitor and protect species, which could be adopted by more countries in Asia. In Nordic countries, for instance, up to 40% of multicellular species are assessed every 5 to 10 years (Gärdenfors, 2010). While this may not be currently feasible in Asia for financial and practical reasons, especially in areas that are species rich but economically poor, a rational approach would be to sample taxonomic groups prioritizing species of particular concern or those that are functionally important in their ecosystems (Collen and Baillie, 2010). Ultimately, longitudinal data on population trends are crucial for quantifying what actually constitutes sustainable harvest. They have the additional benefit of informing public awareness about the threats to species—purchasing dried sea horses as souvenirs, decorations, and medicine may not currently be perceived to be as destructive as purchasing ivory trinkets or rhino horn, but it very likely should be.

One might expect longitudinal studies of iconic species to be more commonplace. Elephants represent species with an extended life history that challenges monitoring efforts. Nevertheless the difficulty of censusing Asian elephants in the wild, coupled with a general reluctance to invest resources in doing so rigorously within range states, has resulted in spotty data on the abundance and distribution of the species (Choudhury et al., 2008). In contrast, there are several well-established programs for monitoring African elephant populations (*Loxodonta spp.*), largely driven by non-governmental actors, some of which have spanned decades (Moss, 2001; Turkalo et al., 2013; Wittemyer et al., 2013) and have contributed greatly to conservation efforts and public awareness, most recently with respect to understanding the impact of illegal killings (Wittemyer et al., 2014). Comparable datasets for Asian elephants (*Elephas maximus*) are largely absent. The governments of India and Sri Lanka have deployed vast amounts of resources on the statistically unsound *ad hoc* approach of counting elephants at water sources, over periods of less than a week. On the other hand, independent research at a site in southern Sri Lanka over a period of six years has yielded high-resolution data on population size as well as demographic variables by observing individually-identified animals using a standard protocol (Fig. 2) (de Silva et al., 2013, 2011). The study first showed that local abundance was at least twice as high as officially recognized by wildlife authorities. Moreover, by comparing longer datasets (20 months) with artificially restricted datasets (3–13 months), errors in the latter were

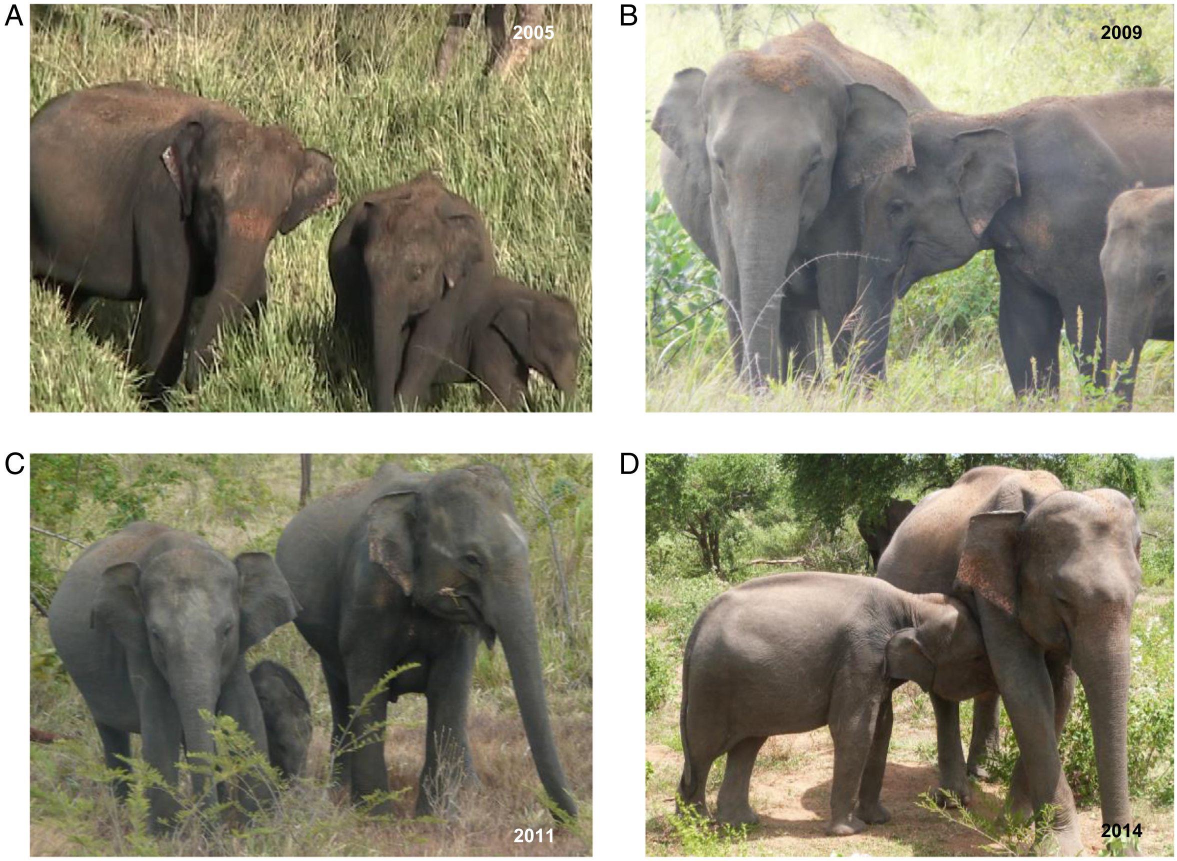

Fig. 2. An elephant family over time. A. Female [065], her putative juvenile female calf [866] and her infant calf [c065-05] in 2005 at the start of the study in 2005. B. The same trio in 2009. C. [866] has her first calf in 2011 [c866-11], accompanied by [c065-05]. D. [866] nurses her calf [c866-11] in 2014. Note the rapid growth by age three. Observations such as these allow accurate estimates of primiparity and birth intervals and serve as reference points for size-based estimates of age of unknown individuals. Longitudinal observations yield high-resolution demographic data and allow researchers to follow individual life histories which could serve to calibrate estimates in other sites where such observations are not feasible. Photos: Shermin de Silva.

highlighted. They included underestimates of density and overall abundance, as well as of survival probabilities and birth intervals. At least half the breeding females were shown to have birth intervals exceeding six years, longer than those reported based in short-term studies, indicating reproduction occurs much more slowly than previously appreciated (de Silva et al., 2013). These are necessary variables for modeling and understanding population dynamics, and the impact of spatial constraints or human activity. For instance, the impact of illegal capture of elephant calves for captive use is likely to be underappreciated. Captures could have severe consequences for populations that are already limited by the number of available breeding females, if equivalent or longer inter-birth intervals are typical. As there are several subspecies of Asian elephant occupying a wide range of habitats, studies of other populations are needed to understand location-specific vital rates and vulnerabilities. Surveys conducted over multiple years designed to estimate density using fecal DNA or to at least appropriately estimate occupancy, would yield more reliable data on elephant populations where continuous observation is unfeasible (Jathanna et al., 2015).

Addressing human-elephant conflict is another domain in which sustained research effort can be crucial for understanding underlying causes as well as efficacy of mitigation strategies and developing appropriate management plans. Studies spanning even a few years can be highly informative of the adequacy of extant protected areas, management plans, or risky locations acting as population sinks. Tracking the movement of Bornean elephants in Sabah highlighted areas where land-use strategies and planned changes impact this threatened population (Alfred et al., 2012; Othman et al., 2013). One tracked individual was among 14 animals killed through poisoning, flagging an area of conflict. While there is an abundance of mitigation methods deployed both by nongovernmental actors and wildlife managers (Desai and Riddle, 2015; Perera, 2009) few studies if any systematically evaluate conflict levels before and after interventions, and maintain follow-up contact with affected human communities while also assessing impact on elephant populations. For instance, the expenditure on large-scale electric fences along protected area borders in Sri Lanka as a means of mitigating conflict with elephants has not been accompanied by any investment in evaluating whether such measures work, why, and over what time scale. Experience and debates in other contexts indicate them to have mixed results, being particularly detrimental if bisecting important habitat (Woodroffe et al., 2014).

Longitudinal observations reveal inter- and intra-individual variation in movement patterns and habitat selection that are vital for defining the spaces that require protection or special management. This is especially critical for migratory species or those with breeding constraints. Clades in which there have been longitudinal and even long-term study efforts in Asia include primates (Osterberg et al., 2015), sea turtles (Shanker and Pilcher, 2003) and birds. Birds are particularly well-studied, possibly due to the presence of professional as well as amateur enthusiasts. A study of range shifts for several bird species in Japan over a 20 year period using the Living Planet Index (LPI) related two concurrent socioeconomically linked processes: the decline of domestic logging together with concomitant increases in logging in Southeast Asia, and changes to bird populations (Yamaura et al., 2009). As domestic timber production declined, native forests matured while demand was met through imports from Southeast Asia (Dauvergne, 1997). Simultaneously, bird species within Japan associated with early successional forests declined, including those with long-distance overwintering sites in Southeast Asia, putatively due to the degradation of habitat there (Yamaura et al., 2009). High-resolution data from long-term species-specific monitoring programs can be especially useful for predictive models if other environmental covariates are also recorded.

Another study tracking the movements of three individual Asian great bustards (*Otis tarda*) over 2–3 years during their annual migrations between northern Mongolia and Shaanxi China revealed that individuals breeding near the same location do not follow exactly the same paths as one another, nor do particular subjects maintain fidelity to stop-over sites across years (Kessler et al., 2013). One female showed a divergence of as much as 170 km in the route she followed across two consecutive years while another has shown over 300 km (Fig. 3). By monitoring a cohort of birds until their deaths, the researchers are assembling a dataset to quantify causes and rates of adult mortality, including due to hunting. Such data are a necessary starting point to quantify threats and prioritize conservation action along flyways and migratory routes. Researchers have also been able to develop deeper ties with rural communities hosting remnant populations of this species, including a two-way transfer of information about this rare bird. Ongoing over close to ten years, collaborative scientific projects such as these yield additional dividends in building bridges between local and external students, researchers, and their respective organizations or institutions. When research is conducted longitudinally, with collaboration between wildlife agencies, NGOs and researchers at the outset, results are more likely to inform management (Milner-Gulland et al., 2010).

2. Practical challenges

Despite the obvious value of longitudinal research, there are barriers to implementation.

Research, however painstakingly conducted, may be seen as an academic exercise of little value for practical management (Cook et al., 2013; Whitten et al., 2001). The extinction of the Javan rhinoceros (*Rhinoceros sondaicus*, Fig. 4(A)) from Vietnam is an extreme example of the gulf between conservation research and decision makers (Brook et al., 2014). A relict population numbering perhaps 10–15 individuals was discovered in Cat Tien National Park in 1989 through a reported hunting incident and subsequent interviews (Schaller et al., 1990). Using progressively more sophisticated methods ranging from field transects and trackway measurements to fecal genotyping techniques the estimate was revised to 8–12 individuals by 1991 (Santiapillai et al., 1993), 7–8 by 1999 (Polet et al., 1999), 4–6 in 2002 (Fernando and Melnick, 2003), and 3–4 by 2006 (Nguyen and Polet, 2007). In 2012 the last remaining individual was definitively found to have been hunted (Brook et al., 2012). These efforts epitomize the worst case scenario bemoaned by Whitten et al. over ten years earlier that researchers are merely "documenting paths to extinction". At present, the Sumatran rhino (*Dicerorhinus sumatrensis*, Fig. 4B) in Borneo is in a similarly critical situation after costly failures in relocating individuals resulting in the deaths of several (Rabinowitz, 1995) with continued political challenges to studying the species let alone executing interventions. Indecisiveness by authorities even when presented with data (Brook et al., 2014; Rabinowitz, 1995) and conflicting agendas among actors from different jurisdictions (e.g. Indonesia vs. Malaysia) stymie progress even with the best available data. However, building partnerships and trust entail long-term engagement and critically re-examining the established *modus operandi* of researchers and conservationists when interacting with wildlife authorities. Accountability for successes as well as failures must be shared, recognizing that data do not motivate action in themselves.

A major reason for the disconnect is frequent mismatch between conservation needs, research conducted, and avenues of dissemination (Knight et al., 2008; Milner-Gulland et al., 2010; Sodhi and Liow, 2000). A review of literature concerning all extant species of rhinoceros identified five 'phases' of crisis and their accompanying bodies of research (Linklater, 2003). In the early stages in which population declines are first registered and management steps are taken, there was little work published either *in situ* or *ex situ*. In the intermediate stage, where drastic declines may be halted by bringing individuals into captive breeding situations, a shift occurred in which research expanded and began to focus on laboratory-intensive genetic and epidemiological research in either captive or wild subjects. In the later phases of population recovery and stabilization (the latter being largely hypothetical with respect to some species) the focus progressed toward primarily *ex situ* research on husbandry and breeding efforts. It is difficult to escape the conclusion that when monitoring and protecting wild populations on the ground proves challenging, attention and expenditure shifts toward captive efforts (Linklater, 2003; Rabinowitz, 1995). This is unfortunate, because although captive breeding may be a last resort for species in steep decline, it is both expensive and rife with its own challenges (Snyder et al., 1996). Ultimately its utility for maintaining wild populations is minimal if individuals *in the wild* remain unprotected. Allocating resources to additional research on a species that is already in the extinction vortex may not be as impactful as strengthening the capacity to protect those species in the wild.

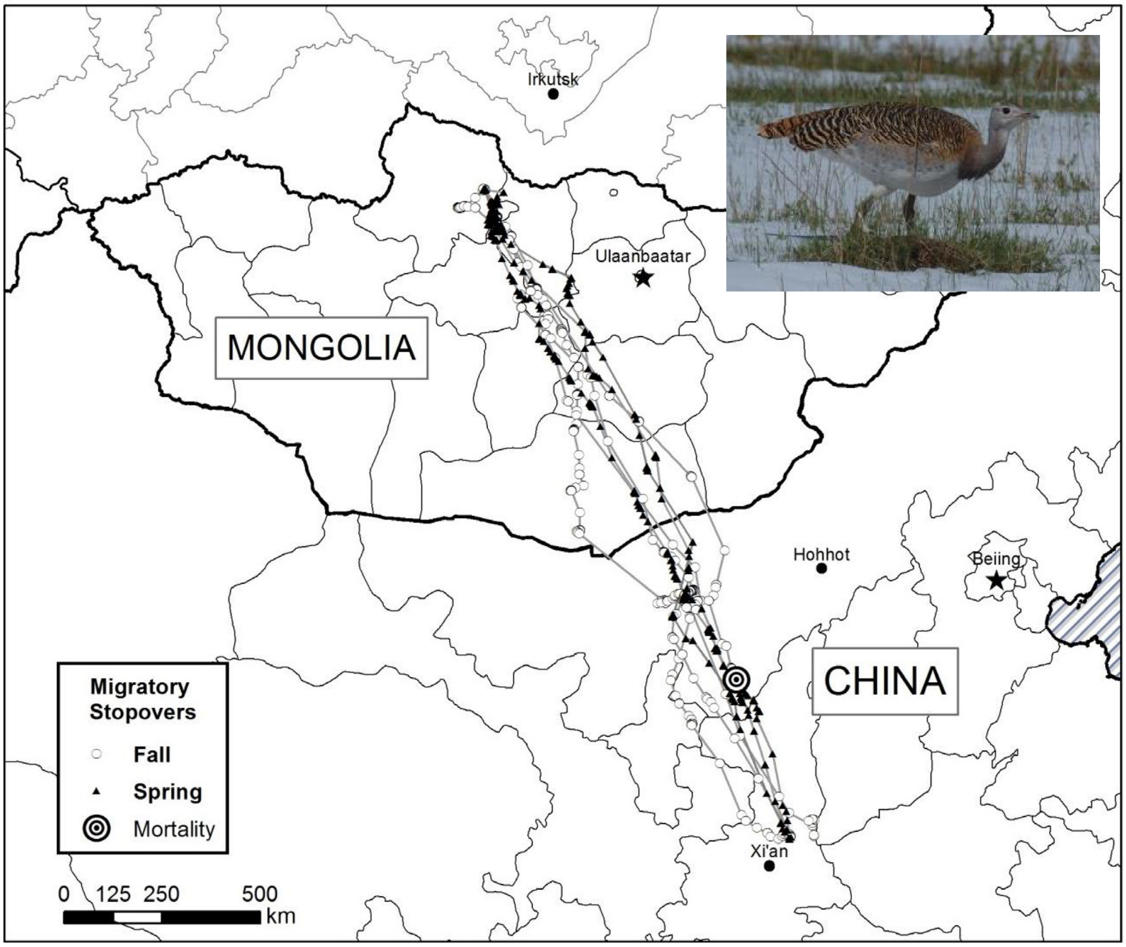

Fig. 3. Seven migratory movements from a single female Asian Great Bustard. Monitoring over multiple years allowed researchers to determine that migratory routes of individuals diverged by up to 350 km east-to-west, and that stopovers are not repeated between years. Map courtesy of Aimee Kessler and photograph by Martin Gilbert (all rights reserved).

Longitudinal studies would be of more use early on before the situation becomes critical, and could be instrumental in detecting early signs of trouble.

The preceding example may beg the question of why it is necessary to conduct and publish peer-reviewed literature at all. In the case of rhinoceros, publications appearing in peer-reviewed scientific sources were heavily outnumbered by those appearing in NGO reports and other gray literature (Linklater, 2003). If similar patterns hold true for other taxa, surveys of peer-reviewed literature (e.g. Martin et al., 2012) may miss important work. The rationale behind this discrepancy may be that if findings can be made available to wildlife managers directly, perhaps the emphasis should be on translation and implementation of recommendations. This attitude may partially explain why Asian biota and researchers are in general underrepresented in the scientific conservation literature in contrast to non peer-reviewed literature (Sodhi and Liow, 2000; Dudgeon, 2003; Corlett, 2011). The average time between data collection and publication can last nearly four years in conservation biology (Fazey et al., 2005), which may be necessary for assuring quality but is hardly adequate for providing timely information. Additionally, lack of access to the scientific literature, which is dominated by "the English speaking elite of the rich world" and the mismatch between what is considered interesting by academic peers and editors vs. conservation practitioners in developing countries are contributing factors (Milner-Gulland et al., 2010). Few journals will publish simple monitoring results, and the academic rewards for doing are negligible. However, higher level meta-analyses or indices such as the Living Planet Index and Red List fundamentally depend on monitoring data, which should therefore be peer-reviewed to ensure quality. There is thus a need for both evaluation and dissemination of longitudinal datasets.

These gaps reinforce the impression that the scientific practice of conservation research is largely removed from needs on the ground and warrant serious attention by the community as a whole not only within Asia, but those journal editors and researchers beyond who maintain standards for review and publication (Milner-Gulland et al., 2010). Despite these issues, I would maintain longitudinal studies be conducted with the aim of peer-reviewed publication because it entails more stringent quality control. This does not preclude concurrently providing the most up-to-date recommendations to

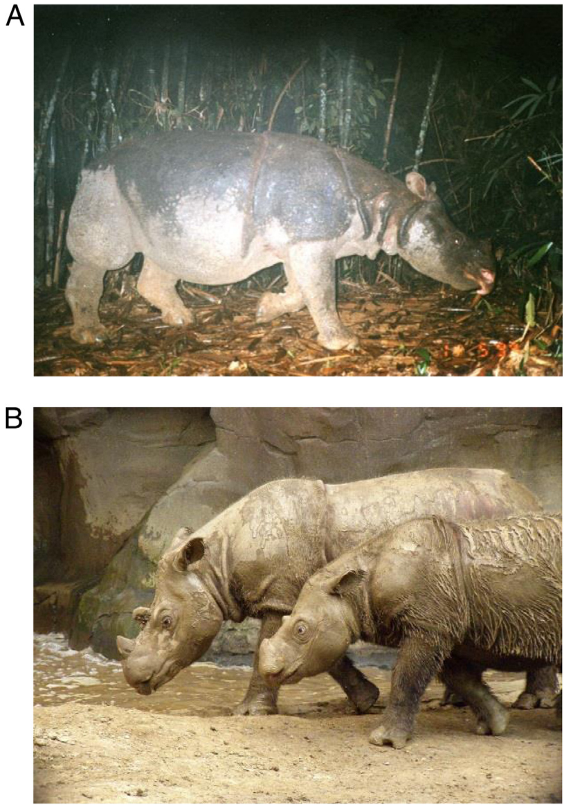

Fig. 4. Southeast Asian rhinoceros are critically endangered. A. Javan rhinoceros (*Rhinoceros sondaicus*), photograph by WWF of Greater Mekong (all rights reserved). B. Sumatran rhinoceros (*Dicerorhinus sumatrensis*), photograph by Charles W. Hardin (cc-by 2.0).

authorities through other avenues of dissemination, preferably in person. Most importantly, peer-reviewed publication makes the work searchable, accessible, and citeable. While there are few journals which publish baseline monitoring data, those in possession of long-term datasets can take advantage of developments in data repositories such as Dryad and pre-print servers such as BioRxiv or PeerJ to make their work accessible by the wider community, thus maximizing the return on investments already made.

Once conservation interventions are conducted, efficacy must be evaluated and subsequent actions modified accordingly. Conservation biology has been criticized for lack of rigor in doing so compared to other fields with similar applied aims, such as medicine (Ferraro and Pattanayak, 2006). Failures as well as successes must be documented and learned from, building on strategies that work without unnecessarily wasting resources on those that do not, and most importantly, understanding why. Publication helps. Following this, broader evaluations may be conducted through meta-analyses. But it is important remember that individual programs may take some time to demonstrate success or failure. Thus longitudinal monitoring of some form is essential. Unfortunately, while follow-up studies are built in to research on medical interventions, they do not seem to be a regular practice when assessing the health of wildlife populations or the effectiveness of management interventions.

The framework for doing so already exists. The paradigm of "adaptive management" (Higham et al., 2008; Rehme et al., 2011) takes a dynamic approach to decision making, with periodic assessments and revisions inherent to the process. But it is not yet widely known or embraced by wildlife managers in Asia. Doing so may be fundamentally impeded by lack of resources or political vision. Managers may resist admitting to failures, especially when under political pressure to take the most expedient course of action. At worst, some states may actually misrepresent the efficacy of actions taken (Shanker and Pilcher, 2003). Funding may be available for conducting the initial intervention, but not for subsequent periods of monitoring, maintenance and evaluation. While it may be more appealing to fund active interventions or pilot projects rather than long periods of monitoring and evaluation, expenditures are wasted if efforts fail repeatedly but are not documented, assimilated, and improved. Adaptive management requires a mindset that is open to learning from experience, and would be a useful paradigm under which to design and conduct longitudinal studies as well as fund them. Failures should not be hidden, but acknowledged so that other alternatives may be tried.

The uncertainty of funding is a perennial challenge for maintaining projects, by no means unique to Asia or to conservation as a discipline. Funder priorities ultimately shape the efforts undertaken, throughout the globe (McNeely et al., 2009). Notably though, the majority of conservation funding is spent within economically rich countries whereas the expenditure per square kilometer in tropical countries is less than a quarter of the global average (James et al., 1999). In developed countries, funding for monitoring and managing wildlife or ecosystems may come from both scientific funding bodies and through state sponsored programs, such as the ecosystem monitoring initiatives and species recovery programs of North America. Among many Asian countries, monitoring and managing wildlife may be lower down on the agenda relative to traditional priorities of economic development, such as building infrastructure or attracting industrial investments. "Geographically flexible funding" from multilateral agencies may make up for some of these deficits, but are far more limited (Brooks et al., 2006) and are themselves accessible largely, if not exclusively, by governments. The distribution of multilateral funds primarily via government mechanisms raises a critical question: how effective are they in regions where governance is weak or fragmented? There is a negative correlation between tropical deforestation rates and the degree of democratic governance within a country, with weak governance implicated as a major driving factor (Didia, 1995; Laurance, 2004a,b), to say nothing of war zones (Hanson et al., 2009), corruption (Ferraro, 2005; Laurance, 2004a,b; Smith et al., 2003; Smith and Walpole, 2005), and powerful industrial lobbies. Witness the ongoing disaster of peatland forests being burned in Indonesia, despite the efforts of the ASEAN Center for Biodiversity and commitments by member states to meet the millennium development goal of reducing biodiversity loss by 2010 and *specific measures to combat transboundary haze pollution* (ASEAN Centre for Biodiversity, 2015). Indeed the two main oil-palm producing nations Indonesia and Malaysia are actively lobbying to undermine zero-deforestation commitments (Laurance, 2015). In other cases short-term or politically-motivated strategies may be prioritized over tackling root causes (Rabinowitz, 1995; Sodhi and Liow, 2000). Even if funding bodies stipulate formal requirements of collaboration and transparency between governmental and non-governmental agents as a condition for receiving funds, such as the Stakeholder Implementation Plan mandated by the Global Environmental Facility, where there is corruption and weak governance these requirements may not be met. It would seem prudent to also diversify the types of actors eligible to access such funds directly, without governmental gate-keepers, thereby ensuring more diverse stakeholder participation and facilitating honest independent evaluations of conservation or management strategies. This will necessarily impose an inconvenience on funding bodies, which will need to vet a broader applicant pool, but may be more cost-effective in terms of returns for funds spent. The Wildlife Without Borders species programs managed by the US Fish & Wildlife Service are fairly unique in exemplifying such a mechanism, which has in some cases provided core support for wildlife conservation and research efforts throughout the world. The support available would be greatly increased if other developed nations adopted similar models. A final recourse is to rely on individual donors or private foundations, which may necessitate substantial time investments in terms of search, proposal submission and follow up reporting, for relatively low returns. In practice such sponsors rarely commit to basic monitoring or longitudinal studies, thus programs will not be able to predict their financial capacity from one year to the next.

The instability of funding and limitations of national resources allocated to wildlife monitoring and management has several consequences. It may indirectly influence where researchers choose to work, resulting in the observed biases toward rich temperate countries (Martin et al., 2012). It may be difficult to maintain basic research infrastructure. Low resource investments on wildlife research centers in the developing nations of Asia translate into both less core funding as well as reduced capacity to attract local and foreign researchers and students which could supplement finances. In a survey of field stations in the tropics (Latin America, Africa and Asia), Asia ranked the highest in terms of average income, but was behind Latin America in terms of median income. Asian stations reported that the largest source of funding originated from the host country (37%), while self-generated funds (e.g. through fees for services) constituted the least (12%) followed by grants, donations, and endowments (10%) (Whitesell et al., 2002). This contrasted with respondents from Latin America where up to 40% of funding was self-generated, reflecting the greater volume of researchers moving through the sites. Most critically, it limits the human resources that can be dedicated to longitudinal efforts. Maintaining such programs requires training and supporting field personnel, whether employed by government agencies, NGOs or academic researchers. If funding is insecure, it is difficult to attract and maintain a sufficiently skilled labor force to conduct the needed legwork on the ground. Moreover, scientific capacity may not be prioritized or valued among individuals employed in the government sector, in contrast to those in developed nations where agencies managing wildlife maintain their own staff scientists. This reduces both the emphasis on rigorous longitudinal research as well as shifts the burden of monitoring wildlife to private endeavors, contributing to the gap between researchers and the individuals tasked with managing wildlife.

3. Conclusions

Wildlife species in Asia are currently being removed or otherwise impacted by human activities at an unprecedented rate. If conservation efforts are to be based on evidence, we need sustained efforts to obtain baseline data for wildlife of concern such that shifts can be detected and serve as the basis for scientifically informed management. The impacts of trade or management should be assessed, and strategies revised accordingly as needed. Collecting such data may bring few academic rewards and may be difficult to disseminate, despite their necessity. Funders, be they private donors, states, or multilateral initiatives, need to make investments that are renewable rather than one-time, less influenced by temporary trends, and accessible to non-governmental (e.g. academic, nonprofit, individual) stakeholders. Those collecting longitudinal data should strive to publish them either in traditional venues or data repositories. While there are many established sites for conducting longitudinal research at the ecosystem level, there are few explicitly longitudinal initiatives focused on wildlife populations in Asia. Perhaps it is time to change this paradigm.

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