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8

Measuring Meaning and Understanding in Primatological and Biological Anthropology Fieldwork: Context and Practice

Agustín Fuentes

After the study of fossils and living animals, when theories have taken definite form, then experiments should be planned. Particularly the importance of adaptive complexes and the precise nature of adaptation can be advanced far beyond the level of individual opinion.
–Sherwood Washburn 1951

We are primates, products of the evolutionary process, and the promise of primatology is a better understanding of the peculiar creature we call man.
–Sherwood Washburn 1973

Introduction

In the North American school of biological anthropology and primatology our modern call to action in fieldwork owes a great deal to the inspiration of Sherwood Washburn. His emphasis, amongst other facets, was on the importance fieldwork as a main baseline for the construction of testable hypotheses. In the Japanese tradition, as illustrated by Pam Asquith, Juichi Yamagiwa, and Nobu Katsukake in this volume, Kinji Imanishi for played a similar role. As primatologists and biological anthropologist our goals for fieldwork can be summarized by a few salient questions (Table 8.1). The answers to these questions are then the driving forces behind the creation and modification of our methodologies.

Table 8.1. Goals for fieldwork in primatology and biological anthropology.

What is human and how do we know it? Fieldwork across cultures and populations provides a view of what humans do on a regular basis – the patterns and variations in human behaviour
How do humans function? Fieldwork provides us with substantive data on human physiological structure and function that can be modeled and tested in the laboratory
Can we see Primates as primates and People as primates? By studying nonhuman primates as well as human primates (the comparative approach) we derive an understanding of both primate wide trends and primate particularities that are present only in specific lineages of primates ...this fieldwork is extremely important for the understanding of the phylogenetic origins of behaviour and structure

Because we are interested in understanding why human and nonhuman primates do what they do, and what our measurements of behaviour and structure mean, we go to the field to collect data. Because we rely on field data as the most accurate reflection of real-world functioning of human and primate bodies the meaning of our data is extremely important to our overall understanding of being human or being primate. That is, we see field data as those data that provide us with the greatest insight into how organisms actually function in the complex reality of day to day existence as opposed to the carefully controlled assessments of particular facets of behaviour and physiology that we can examine in the lab.

Field data are rarely the true measures we see them as. That is, the data seldom carry the totality of information necessary to fully interpret outcomes. This is well known to researchers and we practice and teach care when collecting field data. However, even the most skilled field practitioners can inadvertently create substantial problems during field data collection. We can fall prey to the conflating of measurement with interpretation, we can make the assumption that assessing data is equal to understanding data, and we can fail to recognize and incorporate important biases during our initial assessments of data. These problematic patterns are largely due to the attraction of seeing wild or free ranging behaviour as it happens, which immediately stimulates our desires to understand what we are seeing.

The goal of collecting field data is the measurement of behaviour and bodies, ostensibly the facts of existence. To avoid inherent bias these data should be analyzed only after they have been collected, not simultaneously with collection. However, many of us fail to remember that we cannot and should not interpret what we are seeing as we are collecting data. This is best illustrated by Phyllis Dolhinow's use of the phrase 'I would not have seen it had I not believed it' (Dolhinow 1999). That is, data are of the highest quality when we do not attempt to interpret what we are seeing as we see, and record, it. For the experienced fieldworker it is very difficult not to see the beginning of a behaviour and immediately think 'aha, I know what is going to happen next.' Even if we are correct in our assumptions (as we frequently are) this frame of mind can alter the records we are collecting. For this very reason we use structured data collection tools and methodologies that attempt to inhibit our ability to interpret while recording.

Simply because we can assess the data and discover patterns within it does not imply that we understand the underlying function and/or origin of the patterns. While understanding emerges from repeated assessment and testing, we have to be careful not to make the leap from one to the other quickly. Also, biases created by the constraints of field situations, such as temporal restrictions on field activities, personal experiences and the paradigms from which we derive our basal understandings of how patterns emerge, also impact the data we collect.

In response to these issues this chapter examines the various confounds at play in the collection and assessment of field data in primatology and biological anthropology. While nothing presented here is new, it remains a very important aspect of any discourse on fieldwork and thus in a book on fieldwork should be one of the focal points.

Basic confounds: context and methods

The first significant component of understanding and meaning in our fieldwork is context. Here by context I propose that the (sub-)discipline of the fieldworker (biological anthropology vs. primatology) and the regional and national history of that arena of inquiry has substantial impact on how one goes about asking questions. The issue of nationality is eloquently examined by Pam Asquith and Nobu Katsukake (this volume) so I will not dwell on it in this chapter. However, I will add my voice as an emphatic reminder that much of what we do is rooted in deep traditions of various field work cultures and that one should be aware of his or her cultural history as he/she embarks on data collection or reads the published work of others.

A relevant component of context is the degree to which an individual is engaged with, or by, the various perspectives that ally with biological anthropology and primatology. A biological anthropology fieldworker strongly allied with a psychological approach may seek out different data than one with an evolutionary biology perspective even if asking a similar question. Thus researchers allied with multiple fields and paradigms can embark on data collection from distinct contexts relative to one another and their datasets may look different even when examining the same subjects.

The second and probably most critical component that shapes our field data is the methods by which we collect it. This is a basic point of information that all researchers are taught, but even the most practiced fieldworker can fall into the trap of tunnel vision in regards to methodologies. Here I suggest that different methodologies provide distinct lenses with which to view behaviour and physiology and thus careful consideration and examination of the choice of lens utilized can inform us about the data produced from such activities. Different practitioners favour different methods based on their interest, training and the questions at hand, however, that does not imply that the choices are always the correct ones or that other methods applied to the same data sets will show the same outcomes.

Methodologies in primatology and biological anthropology

When considering field work in primatology and biological anthropology we can see that there are some general similarities despite working on human and nonhuman organisms. In collecting physiological measures one can overlap in the assessment tools to a large degree due to the phylogenetic relationships between primates. For example, many of the hormone metabolite tests used on nonhuman primates are those that were developed for humans and the same is true for many of the genetic markers (DiFiore and Gagneux, 2007; Lasley and Savage, 2007; Ray, 2007). However, the converse is less practiced in that those markers developed for nonhuman primates are seldom used on humans. This does give us some assuredness that we are measuring similar physiological systems, but it does not immediately tell us the meaning of those systems in the organism of interest. For example, the work of Robert Sapolsky on behaviour and endocrine function in baboons has led to similar work with humans. However, these same stress response systems appear to have some important differences in outcomes (meanings) in human and baboon societies (Sapolsky 2004).

In observational measures there are methodological similarities when collecting data to construct general or daily activity budgets (Table 8.2). The patterns and formats by which observers record ad-libitum, or by chance, notes are also quite similar between primatology and biological anthropology. Over the last forty years there has been cross-fertilization between ethnography and primatology producing a methodological practice of observation that has become incorporated into modern biological anthropological approaches to observing humans. However, these similarities (between methods of observing nonhuman primates and humans) are among fairly superficial measures (activity budgets and gross behavioural profiles) and can potentially create false similarities in interpretation of the data.

Table 8.2. The Methodological differences and similarities between primatology and biological anthropology.

Methodological differences in primatology and biological anthropology	
Primatology	**Biological Anthropology**
Scan samples	Informant focus data collection
Group scans	Third party reports
Focal scans	Interviews
All-occurrence samples	Culturally limited all-occurrence samples
Continuous	Continuous
Select variables	Select variables
Limits of observation	Limits of Cultural context
Biomarkers w/capture	Biomarkers w/o capture
Observer impact	Participant observation/impact

In the arena of differences between primatology and biological anthropology we see the greatest potential for miscommunication across research results. For example, primatologists are limited by access to the group ('solved' via habituation, which may create its own problems) and by the observation conditions (one can lose a monkey in forest cover). Observing Humans on the other hand one can encounter a variety of cultural limitations on observability and access. Thus the gender/age of the fieldworker and his/her linguistic/social engagement with the people being observed can radically impact the observations themselves. This may lead to problems when attempting to generalize or universalize results across these related spheres of interest. For example, comparing results of nonhuman primate studies reporting on male-male competitive behaviour inherently produces a distinct set of data than a similar male-male competition study in humans. In this same example we might measure reproductive investment in two very different ways; using occasional behavioural protection of an infant by a male langur monkey versus social investment in a child's education, food and housing by an adult male human. In this case we must ask ourselves: are these measures equal when attempting to assess meaning and understand a system of male mating investment? Even two studies of similar behaviour in humans by different fieldworkers can produce distinct data sets if appropriate controls are not utilized.

The use of similar biomarkers in nonhuman primates and humans can produce distinct types of data as the nonhuman primates must usually be trapped and sedated prior to drawing samples whereas the humans are not. Even the use of fecal samples produced distinct patterns as human cultural rules and contexts surrounding defecation and urination vary substantially more than nonhuman primate behavioural restrictions on such behaviour. This comes into play when moving back and forth between human and other primates in the recent discussions about field data relative to hormone levels such as testosterone, cortisol and oxytocin for example (Carter 2004; Sapolsky 2004).

Of course, language also complicates the methodological endeavour. We are more reliant on interpretive meaning of data from nonhuman primates, which is not to say that a good deal of human data are also open to broad interpretation. Rather, we are not able to question the primates in the same manner we can humans. We have no access to linguistic clues and symbolic manipulation to contextualize the behaviours we observe and record in the free ranging nonhuman primates. This is can also be a confound in interpreting human data; language and symbol greatly enhance our ability to deceive and manipulate meaning. Language, and its concomitant cognitive context, also allows us to 'think' ourselves into the mind of another human in a way that we cannot with another primate, even when we share a multitude of similar physiological and morphological patterns with those primate cousins (see related discussion in Fuentes 2006).

Finally, it is also worth noting that the human subjects protocols and the animal subjects protocols, and the ethical issues surrounding them, differ. Researchers must acquire these permissions prior to engaging in research projects, and the differences can also shape the methodologies and subsequent data generated by such projects.

Therefore data from field studies in primatology and biological anthropology can be different even when similar methods are used. This is an important consideration in trying to assess data derived from these two intertwined sub-disciplines. Similarities may be superficial such as in the case of activity budget, or they may not be (even in the same case). However, simple one-to-one correlations across primates and humans are fraught with complexity thus field data should not be taken at face value without multiple assessments of the issue at hand.

Care must be taken when comparing data derived from similar methodologies on humans and primates even when constructing primate wide trends. Caution must also be used when making comparisons (such as in mating behaviour, aggression, infant care-taking, etc ...) and using those comparisons to initiate research agendas (as per Washburn's 'New Physical Anthropology'). In other words, our use of field-work to test and construct hypotheses about primates and humans is in and of itself a complex exercise filled with both superficial and substantive similarities. The methods utilized and the contexts in which those methods are employed can have substantial impact on the resulting data and therefore on our attempts to assign meaning to the data and derive understanding from those meanings.

What constitutes natural behaviour?

In addition to the context and the methodologies we practice, the conceptualization of natural in the field, assumptions about what we see in the field, the proclivity to think in terms of species-specific behaviour, and the potential conflicts created by the differences between approaches favoring inductive and deductive approaches are relevant to thinking about the production and assessment of field data.

A major problem revolving around the concept of natural encountered in fieldwork scenarios is the notion that field observations (of humans or other primates) are recording behaviours in an environment that is minimally anthropogenically altered and/or that is disconnected from facets of the global ecosystem. This is seen in primatology with the drive to find new or novel sites at locations distant from human settlement and in human studies by a focus on tribal or forager peoples in settings remote relative to urban centres (seen also as 'remote' from modernity or even post-modernity and representing a possible 'link' to our forager past). Over the past few decades it has become readily apparent that these assumptions about isolation or minimalization of modern impact are potentially serious errors.

A second fallacy of natural is the concept that measuring one population or subpopulation can present a reliable measure of a species norm, or what is typical (read natural) behaviour for a given species. Primatology has only recently emerged from this misconception as multiple site studies on the same species continued to demonstrate remarkable variability in behavioural and physiological responses at the same time that some distinct common patterns emerge/remain (Campbell et al. 2007).

A main assumption in both the issues of natural and of species specificity is the notion that the anthropogenic impact and local histories are minimal enough to

ignore when in most cases they are not. An example from primatology exemplifies this. Sapolsky and Share (2004) provide an example from their study of two groups of baboons. This study began with observations of two groups, one who frequented a tourist lodge garbage dump and another that lived in the forest away from the human activity. The two groups had somewhat distinct behavioural and ecological profiles. Over time the higher ranking adult males from the forest group began to forage occasionally in and around the tourist lodge group. These adult males were highly aggressive and frequently would have to fight with the males and other individuals in the tourist lodge group for access to the high quality refuse at the forage site. At one point in time a quantity of tainted meat was dumped by local humans at the site resulting in the tourist lodge group and the aggressive males from the forest group suffering substantial mortality. The remaining males in the forest group happened to be rather pacific in their behavioural profile, especially relative to the highly aggressive males that died from the tainted meat forage. Over time as young males matured in the group they acquired the pacific behavioural patterns of the surviving males and the whole behavioural profile of the group shifted radically from aggressive to affiliative.

This example challenges both what is natural and what is typical simultaneously. The baboon males exhibited the capacity for both aggressive and pacific temperaments and the foraging and demographic patterns were affected by anthropogenic interactions, even in the forest group where most individuals did not interact with humans of human areas. Observers not viewing this entire sequence (local history) and only collecting data at specific 'slices' of time would report very different patterns of behaviour in the forest group and potentially offer quite different explanations of their meaning.

It is common to assume that what we see in the field reflects an adaptive response to specific selective environments. This was often seen as a simple truth: that field behaviour is natural, thus has arisen as an adaptation in response to selection pressures as organisms strive towards optimal solutions. The reality of fieldwork interceded by at least the 1980s and this simple assumption about optimality transitioned into the current paradigmatic parsimony explanation based on functional hypotheses. That is, if the behaviour we see is potentially 'costly' for the organism in terms of energy or reproductive output then it must have arisen (or is maintained) through some selective influences and, thus, may be seen as a response to selective challenges. The problem with this perspective is that it results in assuming we understand the meaning (or function) of a behaviour simply because of its occurrence in a field setting. Seeing a behaviour commonly expressed in a field setting does not necessarily tell us that the behaviour is functional, adaptive or even the best response to a given challenge. These understandings come from much greater analyses of the behaviour in and across contexts. While most fieldworkers are well aware of this confound it remains a common assumption amongst practitioners. If a behaviour is exhibited in the field in a 'relatively' normal environment, or in a particularly challenging environment, then we usually assume it is a functional response. This is a potential error of assigning meaning and assuming understanding prior to careful analyses.

While fieldworkers are less and less likely to assume species-specific behaviour and physiologies today, it does still occur and is relevant to this discussion. Multiple factors (Table 8.3) will impact what our data look like and each plays an important role in the creation of meaning and subsequent understanding. The representative sampling issue is of particular importance in regards to human biology and behaviour studies. Many psychologically allied approaches in biological anthropology particularly in mating strategies theory (Buss and Schmidt 1993, Miller 2000) and game theory/cooperation studies (see Barrett et al. 2002) rely heavily on student samples as proxy for field conditions. While this is a practical response to the difficulty of getting subjects and controlling for subjects in cross-cultural contexts this remain a real problem as college students are a relatively poor representative sample of the human species

Table 8.3. Multiple factors impact what our data look like and each plays an important role in the creation of meaning and subsequent understanding in short vs. long term studies.

Short vs. long term studies
Individuals vs. Group vs. Groups vs. Populations – representative sampling
Inter- & Intra-specific comparisons
Researcher vs. Research team vs. Multiple teams

A final additional confound, the conflict between inductive and deductive (a posteriori and a priori) approaches in fieldwork, is worth mentioning. I tend to think of this as a pseudo conflict because I believe it should not be a problem for researchers. However, under certain paradigmatic perspectives deductive approaches based on apriori hypotheses are more highly valued than inductive approaches and basic natural history/baseline data collection (van Schaik and Kappeler 2002). This is a problem as assumptions of sufficient baseline understandings change the meaning of data. If we assume we already know how a system works, then there is a higher likelihood of our data reinforcing this assumption. This is why both inductive and deductive approaches should be equally valorized as complementary approaches, not hierarchically related methodologies. The conflict interpretation is best combated by researchers being open to diverse paradigmatic perspectives, employing multi-disciplinary approaches and realizing that baseline data collection and hypothesis testing are equally valid partners rather than steps on a hierarchy of value. It is also worth noting that recent emerging complexity in both evolutionary and biocultural theory comes with a built in connectivity between apriori and aposteriori approaches (Odling-Smee et al. 2003, Stamps 2003, West-Eberhardt 2003).

A case study

Here I review a case study from primatology to demonstrate the complexities inherent in measuring meaning and understanding in fieldwork discussed above. The case of

gibbon social organization and behaviour provides a robust exemplar of how context, methods, and paradigmatic orientation combined with a significant temporal factor can influence and structure the ways in which field data are understood and assessed. Here I give a brief summary of the broader review presented in Malone and Fuentes (in press) and Fuentes (2000).

The gibbons are a group of apes found throughout parts of South and Southeast Asia. They are represented by four genera and twelve species making up the family Hylobatidae. Aside from humans they are the most geographically widespread apes. Even in the earliest stages of primatological investigations the Hylobatids, or lesser apes, played a significant role in method and theory. The fieldwork by Carpenter (1940) is amongst the first examples of modern primatological field studies and set a basal stage for the expansion of primatology as a discipline in North America. Interestingly, Sherwood Washburn was a research assistant on one of Carpenter's main research trips to Asia and the experience he had there, melding primate observation and anatomical/morphometric analyses, may have laid a portion of the impetus and theoretical perspectives eventually resulting his call for a 'New Physical Anthropology'. Although cautious in his assertions about what he saw and recorded, Carpenter did report that the gibbons appeared to occur in groups that resemble nuclear families: an adult male, an adult female and one or more offspring. Carpenter and his team observed a few groups with apparently more than two adults, but gave them little consideration (Fuentes 2000). This assumption of a 'nuclear family' grouping pattern become the normative state for baseline perceptions of gibbons from the 1940s on. It began to be associated with a set of particular characteristics, the most prominent of which is the 'pair bond' and its assumed monogamous social structure.

The 'pair bond' or predictable, stable and long-lasting relationship between two unrelated adults is, for some, the basal unit of social organization in many primate taxa. Several authors proposed the pair bond as the central aspect in models of the evolution of lemur, gibbon, and human societies (Jolly 1998; van Schaik and Kappeler 1997; Lovejoy 1981; Palombit 1999; Wrangham et al. 1999). In these cases the pair bond is associated with monogamous mating and a suite of behavioural characteristics that act to maintain the mating and social bond between the pair. The focus on the pair bond is primarily rooted in the search for its evolutionary or functional origins, the manner in which selection pressures may have produced pair bonds, their associated behavioural profiles, and how pair bonding relates to individuals' genetic fitness (see Fuentes 1999, 2002). As such, the occurrence and maintenance of the pair bond becomes a critical basal element in the theorizing on the evolution of primate, and human, social organization. This status combined with the strong positive socio-moral association of monogamy and pair bonds (frequently shorthanded as 'marriage') in North American and European society and Judeo-Christian traditions make monogamy/pair bonds very interesting to both evolutionary theorists and individuals searching for explanations of our current moral and social systems (Fuentes 2000; Hrdy, 1981; Symons 1979; Wrangham et al. 1999).

In this context study of the gibbons moves beyond being simply a primatological endeavour and plays a prominent role in the construction of broader evolutionary hypotheses for the occurrence of two adult, pair-bonded, or 'monogamous' social organization (Mitani 1984; Palombit 1999; van Schaik and Dunbar 1990; Tilson 1980; Wittenberger and Tilson 1980). From the earliest field studies to more recent fieldwork (Brockelman and Srikosamatara 1984; Chivers 1974; Ellefson 1974; Leighton 1987; Mitani 1984), the hylobatids were seen as the 'paragons of fidelity', the model family unit, and the standard bearers of the monogamous primates. This then acted to provide impetus for their study in regards to the potential insight they could provide to the functional understandings of said theoretical perspectives (monogamy, in this case).

However, as early as 1940 Carpenter warned, 'It was found that the gibbon family pattern with limited variations characterized gibbon societies. ... But this description may be an overgeneralization' (Carpenter 1940). So, while there were tentative warnings and cautionary statements regarding the use of gibbons as 'model' monogamous systems, they still were held as the prototypical primate to study to understand monogamy, with the caveat that 'some extremes to a strictly monogamous patterns have been observed, but typically in extreme circumstances' (Chivers 2005). Thus deviations from the expected grouping pattern were aberrant and not included in data based attempts to understand gibbon social and evolutionary patterns. The 'monogamous' model for the gibbons included a suite of associated characteristics that fieldworkers expected to see during their data collection. These included a species-ubiquitous pattern of pair bonded adults residing in nuclear families, monogamous mating within the pair, long term fidelity between the pair, territoriality and aggression between neighbouring groups/pairs, and expulsion of offspring by the same-sex parent at or near sexual maturity because of their potential as competitors. These assumptions combined with the broader perceptions of monogamy as a social system led to a suite of hypotheses constructed to take to the field in order to test the specific apriori models (Fuentes 2000, 2002; van Schaik and Dunbar 1997; Wittenberger and Tilson 1980).

Because of the accumulation of fieldwork knowledge over the past three decades we now have sufficient data to warrant not only Carpenter's caution, but a full reassessment of both how gibbons live and interact and how we gather and represent field datatsets. The above mentioned apriori models for gibbon social evolution and behaviour have to be modified. Substantial field work demonstrates that, currently, we cannot best describe the gibbons as living exclusively in nuclear family groups, exhibiting only monogamous mating, or necessarily exhibiting other facets in the expected behavioural profiles of monogamous social systems (Bartlett 2007; Brockelman et al. 1998; Fuentes 1999, 2000; Malone and Fuentes 2009; Reichard 2003; Reichard and Sommer 1997). Based on accumulated fieldwork we can assert that monogamy per se is not a social system (it is a mating pattern) and that two-adult groups as a species norm are very rare for primates (as in all mammals). Even when two-adult groups do occur in primate species, they vary dramatically in their behavioural and mating profiles and occurrence in a two-adult

group does not necessarily imply monogamous mating or other any specific pair bond behaviour (Fuentes 2002). In other words, the basal assumptions underlying much of the status of gibbons as the iconographic models for monogamy in primates are misunderstandings and overstatements of the meaning of previous field data and incorrect assumptions about what 'monogamy' is on the whole.

Why did perspectives on gibbons change? Why are our understandings and our interpretation of the meaning of gibbon behaviour different today? Fieldwork at both long term and short term field sites resulted in a set of data that challenge previous assumptions/assertions. Eight of twelve gibbon species have been reported to occur in larger than two adult groups (at least 18 per cent of all groups observed) (Fuentes 2000; Malone and Fuentes 2009). Observed dispersal distances from natal (birth) groups are short (averaging less than 1000m) and mating outside of the group does occur, suggesting that genetic relationships between neighbouring groups may be close (Fuentes 2000; Reichard 1995, 2003). Long term observations demonstrate that many groups are not made up of only parents and offspring (they are not nuclear families) and that groups undergo changes in adult composition as adult pairs dissolve and form over time (Brockelman et al. 1998; Reichard 2003). Different species show variation in by sex behavioural patterns, especially in regards to inter-adult intragroup interactions (Fuentes 2000). Local populations of gibbons appear to have a level of supra-group social organization, resulting in communities or neighbourhoods with complex genetic and experiential relationships between groups. This suggests that the traditional focus on the single group as the sole centre of gibbon social behaviour may cause misinterpretation of the overall context and function of the inter- and intra-group behavioural patterns observed. This challenges assumptions associated with nuclear family structure, 'monogamy,' and the underlying explanations for why gibbon groups would enter into conflict (or affiliation) with one another (Bartlett 2007).

How does this situation and change over time reflect my previous discussion of the complexities of fieldwork in measuring meaning and understanding? In turns out that the context and paradigmatic training for decades of fieldworkers provided a set of assumptions that impacted the way in which they 'saw' their data during collection and 'understood' it during analyses. This pattern combined with a heavy usage of a 'species or genus typical pattern' assumption (monogamy and its associated behavioural patterns in this case) led to the propagation of an interpretation pattern of gibbon datasets that was increasingly contradicted by those same datasets. Here we see explanatory mechanisms and social constructs suggesting that 'monogamy' per se was an adaptation exhibited by gibbons and thus gibbons lived in two-adult groups by design. Measures of gibbon behaviour were then based on the assumption of a meaning for a two-adult group: that it equalled social and genetic monogamy. This assumption carried with it specific sub-assumptions about species particular behaviour patterns and thus hypotheses were generated to test these (and not other) assumptions. It turned out that data on gibbon groups and populations are best understood by placing them in the broader context of recent studies on small group, two-adult, and pair bonded primates, a relatively new paradigmatic approach that

emerged in the 1990s due to increasingly confounding field datasets on small-grouped primates (Fuentes 1999). While the measuring of gibbon behaviour has changed with the advances in genetic analyses and the increase in long term and additional study sites, the debate on meaning is ongoing. Researchers are currently faced with multiple perspectives regarding explanatory hypotheses of gibbon social structure (Bartlett 2007; Fuentes 2000; Reichard 2003). In the case of the Hylobatids one can see that the patterns and contexts under which research took place coupled with specific apriori paradigmatic assumptions resulted in specific limitations on understanding of the gibbons and their behaviour. This case also highlights the pivotal role of accumulated fieldwork and advances in technologies to move through assumptive barriers and into broader and more accurate attempts as measuring meaning and understanding in primatology.

So what?

> In place of separate subjects, each with its own assumptions, methodology, and technical jargon, we must envisage networks of cooperative investigation, with common methods and terminology, all eventually linked up in a comprehensive process of inquiry. This, of course, will mean a radical reorganization of scientific teaching and research.
>
> J. Huxley 1963, in Washburn 1973

Given in 1963, reiterated in 1973, this quote still holds an important call for fieldworkers in primatology and biological anthropology. Our ability to collect, assess, analyze and contextualize high quality datasets is largely reliant on our abilities to follow Huxley's call and see across methodologies, paradigms and contexts in our quest for understanding. In this chapter I suggest that the incorporation or at least recognition of variable methods and an openness to multiple paradigms of analyses lead to better data which in turn facilitates better access to opportunities for understanding. It should be apparent that because of the global anthropogenic impact on our planet, and of the cultural and historical constructs we as researchers hold and encounter that we need to be as biocultural as possible in the field. That is, we need to incorporate and examine interfaces between physiological, cultural, historical and perceptual modalities as parts of dynamic systems not only as distinct and easily separable influences. I suggest that one area that may hold great promise in integrating perspectives and practice from biological anthropology and primatology is the emerging field of ethnoprimatology (Fuentes and Wolfe 2002; Riley 2006).

Because of our increasing realization of the bio-behavioural realities for humans and the need to integrate approaches we can possibly see an engagement with, and respect for, multiple methodologies as a new biological anthropology, a logical extension of Washburn's new physical anthropology. The complexities in fieldwork methodologies, contexts and interpretations fit well with emerging understandings

of complexity in evolutionary theory and especially with greater understandings of human niche construction and the integration of organism-environment relations (Fuentes 2004, 2009; Odling-Smee et al. 2003; Potts 2004). Rather than merely responding to environmental challenges organisms can also shape their environment and this in turn alters the selective pressures impacting the system. Evolution is a dynamic process and one in which humans have a significant impact relative to other organisms. Rather than assuming a cultural deconstructionist's cooption of post modernity that favours only hyper relativistic and hyper reflexive narratives we can see a move beyond post-modernity in primatology and biological anthropology wherein a certain amount of reflectivity and reflexivity combined with greater integration across disciplines and fieldwork paradigms produces novel and/or innovative engagements with our datasets and the way we both produce and understand them.

Finally, I conclude this chapter with the following points, as they are central to the future of fieldwork and the data it generates.

- Who owns the truth in the collection of field data? Can it be one or another specific paradigm or one national school over others? No. The concept of multiple truths may not be practical, but there are certainly many ways to create meaning from data and exploring a multiplicity of paths can only be beneficial to our overall abilities to derive meaning from datasets.
- How do we disseminate field data? Where do we publish, in what language do we publish and who are the gate keepers to publishing? These are questions for serious consideration and ones that are worthy of a book on their own. How and where data are presented acts to either solidify current paradigms or challenge them.
- How do we train fieldworkers? Most, if not all, of the contributors to this volume are teachers as well as researchers. We create the next generations of fieldworkers and thus the way we do so shapes the datasets that emerge. Training the practitioners of the future to respect diverse (and possibly divergent) perspectives is one of the strongest tools we have in our quest for the derivation of meaning and the process of understanding data derived from fieldwork in primatology and biological anthropology.

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9
Fieldwork as Research Process and Community Engagement:
Experiences from the Gambia and Afghanistan

Mark Eggerman and Catherine Panter-Brick

Introduction

This chapter is a reflection on the practice of fieldwork, in which we offer concrete suggestions to encourage an open-ended and iterative attitude to research. This is particularly relevant to situations in which research activities rely on close engagement with local communities for the purposes of scientific data collection and evaluation. We draw upon personal experiences from two contrasting projects. One was action research to address a particular public health problem in the Gambia, namely the prevention of childhood malaria through better usage of mosquito nets. The other was basic research on young people and their families in Afghanistan and refugee camps in Pakistan, to provide baseline data on mental health and to address an important public health issue. Both projects were cross-disciplinary in theoretical outlook and methodology, evaluating biological health outcomes within social and ecological contexts. They posed, however, very different challenges in terms of implementation. We drew on our long-term experience initiating and managing large research teams overseas: directing inter-disciplinary research on health and wellbeing in cross-cultural contexts (Panter-Brick) and international media research in the Middle East and Africa (Eggerman), to devise ways that made research more reflective, relevant and effective on the ground.