

Foreword

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There is a new confidence in the social and biological sciences in the value of our work, and nowhere is this more apparent than in the emergence of “evidence-based” fields. In the wake of the successes of evidence-based medicine (Sackett et al. 1996), in conservation biology we now have evidence-based conservation (Sutherland et al. 2004), in economics something fast approaching evidence-based development economics (Banerjee and Duflo 2012), and in policy the stirrings of similar movement (Biglan and Cody 2013). Sharing a commitment to systematic comparison, whether this be based on randomized controlled interventions (Cohen and Dupas 2010) or, where experimental manipulation is impossible, tightly controlled comparison (Andam et al. 2008), systematic reviews and meta-analyses (e.g. Brooks et al. 2012), these fields provide rigorously-assessed, and often widely vetted, knowledge for deployment in direct action.

So where are we with respect to an evidence-based evolutionary anthropology? The dozen chapters in this stimulating collection offer some intriguing pointers towards where we should be going. Clearly “behavioural change”, the holy grail of so many conservation and public health projects, cannot be attained, either through educational interventions or restructured incentives, without a genuine understanding of how and why humans behave as they do. How can you change a person’s firewood collection practices or health-seeking strategy if you do not understand the dynamics entailed in how ecology and individual circumstance shape opportunities and constraints, how opportunities and constraints shape preferences, and how preferences influence decisions? Furthermore, it is equally critical to understand the principle avenues whereby behaviour and ideas are transmitted between individuals if campaigns, pamphlets and educational outreach are to have any effects.

As many of the contributors to this volume make abundantly clear, behavioural ecology (the dominant framework of evolutionary anthropologists), and the evolutionary social sciences more generally, offer a powerful framework for tackling these issues, given their commitment to both distinguishing explanations of ultimate function from those of proximate mechanism and determining universal rules

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that underlie the diverse patterns of behaviour within and between different human populations. By examining decision-making within the matrix of the costs and benefits that structure the marginal returns to fitness associated with any particular action (or inaction), behavioural ecologists can develop hypotheses for how and why humans behave as they do, given a particular set of environmental opportunities and constraints. As a rigorously empirical discipline, these predictions are then tested with data from multiple populations in different parts of the world, with the objective of revising model assumptions (Borgerhoff Mulder and Schacht 2012). Furthermore, by using evolutionary models to identify, explore and describe theoretically-informed proximate triggers of behaviour, evolutionary social scientists should be in a position to design effective policy interventions. And then, with additional input from cultural evolutionary theory (Mesoudi 2011), they can identify key influences on how certain patterns of behaviour are transmitted within and between generations.

From the chapters in this volume we now know, for example, of extrinsic mortality's role in shaping reproductive and health decisions, of the salience of habit forming, of the highly contingent patterning of cooperation and punishment across human populations, and of the dangers of using simple models based on the assumption that individuals strive to maximize resource acquisition when we design strategies for reducing poverty. Are we ready to make evidence-based recommendations? Certainly some of the contributors to this volume are willing to move in that direction, always with caution given both past and present misuses and/or misinterpretations of evolutionary reasoning. Where we are typically lacking, however, is in clear policy recommendations. It is one thing to say 'We now know X, and this should guide policy' and quite another to say 'Knowing X leads us to recommend policy Y'.

To do this we need more evolutionary political science—a field largely missing from this collection because it is still so young. For example, it is certainly useful to know that the poor invest little in health care (their own and that of their children) not simply because of their limited finances but because of their high vulnerability to extrinsic sources of mortality and their consequential heavy discounting of future states of health. But this is only a start, and we need more ideas: to get policies which motivate people living in deprived neighbourhoods to find innovative ways of reducing extrinsic risk, which provide tax monies for the material and social capital to do this, or which offer incentives for those who wish to find new homes in less risky environments. These are fundamental policy shifts that change the structural parameters of inequality. They require ideological shifts in tolerating inequality that seem to occur more naturally in some contexts than others (Borgerhoff Mulder et al. 2009), but we still do not understand exactly why. Scientists are rarely good politicians. But by blending the insights of models, experiments, and systematic empirical comparisons, in the way Ostrom (2007) pioneered in the field of natural resource management, we need to start thinking about establishing a more comprehensive evidence-based social science.

So, to avoid being hoisted on my own petard, how do we as evolutionary anthropologists do this? The suggestions are deceptively simple. First, we should strive for

greater communication with on-the-ground organizations dedicated to improving public health, alleviating poverty and finding sustainable use of natural resources; this is critical because these organizations typically have much greater access to policy-makers than do academics. We should also aim to integrate academic research more closely with project evaluation, as is occurring so successfully in development economics (e.g. Palm et al. 2005). It is also very important to make the results of our research accessible to those in the executive branches of government who can make best use of them. That said, being effective in any of these goals is difficult, and we still have many lessons to learn. Hopefully these chapters will attract new interest and fresh talent.

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