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Borgerhoff Mulder, Monique

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An Interdisciplinary Biosocial Perspective

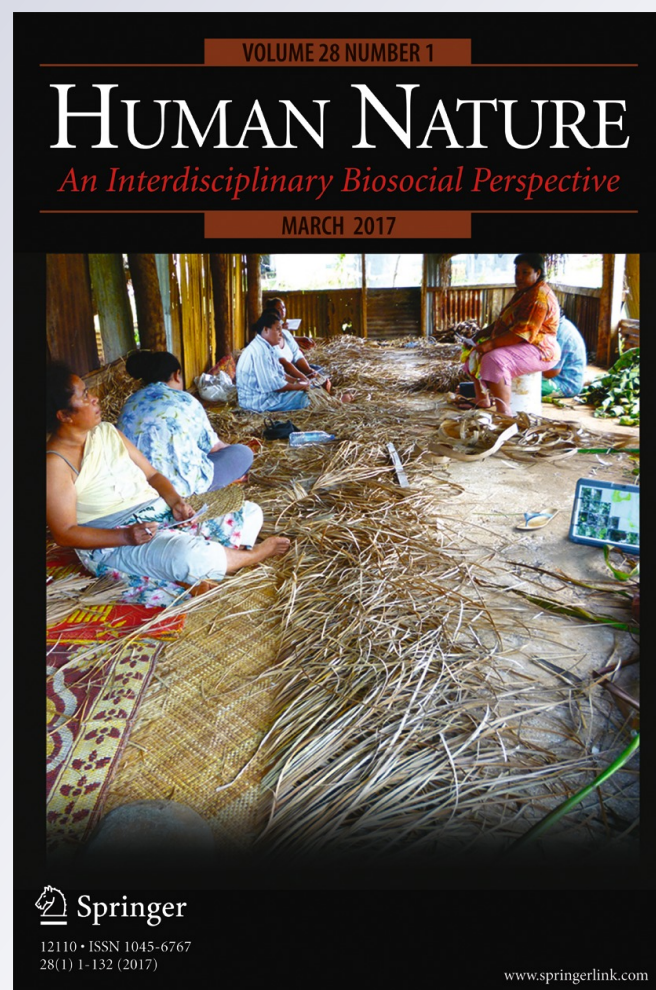
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Review of *Demography and Evolutionary Ecology of Hadza Hunter-Gatherers* by Nicholas Blurton Jones (Cambridge University Press, 2016)

Monique Borgerhoff Mulder¹

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How a New Classic Raises the Bar for Us All

This is a *tour de force* demographic study of the Hadza of Tanzania, one of the world's few remaining, relatively intact forager populations, written with humility, wit, and precision by a fieldworker with a lifetime of experience working with hunter-gatherers.

We are introduced to the Hadza, a demographically-thriving population, with a TFR of 6.1, a life expectancy at birth of 32.7 years, an infant mortality rate of 21.8%, and a survival to age 15 rate of 55%, all relatively normal values for human populations, forager or other. Indeed when compared with standard life tables, the population shows no particularly unusual demographic parameters. To the extent it is possible to reconstruct earlier cohorts, there is no evidence of abrupt demographic changes during the past 100 years; for example, the age pyramid is quite smooth, though there may be a slight shortage of women aged 40 to 50 as a result of a mid-twentieth-century drought, and echoes of failed government attempts to settle the Hadza. “Pacification” for the Hadza affected mainly their relationships with outsiders (slavers, pastoral nomads and now the government), not the internal dynamics of their society. Furthermore, comparisons of anthropometric measures made in the 1960s with those of the past two decades suggest only very slight indications of impoverishment. Blurton Jones shows convincingly the Hadza will persist demographically, if not culturally. Attraction to alternative lifeways may seal their fate in that respect. And indeed, as I was reading a chapter in a

✉ Monique Borgerhoff Mulder
mborgerhoffmulder@ucdavis.edu

¹ Department of Anthropology, One Shields Avenue, University of California-Davis, Davis, CA 95616, USA

Dar es Salaam café I noticed a headline in an English language newspaper, accompanied by a picture of a Hadza family: “Minority Ethnic Groups on the Verge of Extinction in Tanzania” (*The Citizen*, September 11, 2016). Anthropology is significantly enriched with this publication on yet another threatened cultural group, a book which will sit proudly next to (and in places outshine) the two preceding comparable classics: Howell on the !Kung and Hill and Hurtado on the Ache.

Using a rich combination of archival reports, early ethnographic accounts, simulation modeling, and his own extensive demographic files (which creatively incorporate some of the work of earlier and concurrent Hadza ethnographers), Blurton Jones produces a compelling account of a population that has been recovering from major losses (raids, in some cases an indirect consequence of the slave trade, from neighbors) incurred at the end of the nineteenth century. He brings these observations to bear directly on the “forager population paradox”—namely, the enigma of healthy hunter-gatherer fertility and mortality rates with their relatively low numbers. He uses historical accounts and simulation modeling to determine what has kept the Hadza population from exploding, taking into consideration density dependence, disease, sexually transmitted infections, famines, prey crashes, climate change, war, and raids. Although the results are necessarily somewhat inconclusive, the general message is that the Hadza’s relatively rapid rate of natural population increase is offset by losses to raiding, diseases (introduced in part by the larger, denser populations of agriculturalists and pastoralists who increasingly surround their homeland), and out-marriage. More generally, Blurton Jones follows others in subscribing to a saw-tooth characterization of forager population history, with populations growing and crashing repetitively. He extends earlier insights by suggesting that such dynamics might be exaggerated in contexts in which offspring depend on multiple helpers because adult mortality further jeopardizes child recruitment (beyond the fact that dead adults can’t produce children). An interesting suggestion to be sure, although it assumes that helpers provide care that is a complement rather than a substitute to the (missing) parent (an assumption partially supported later). He also makes the interesting suggestion that growing and crashing populations would select for reproductive strategies emphasizing “fast and early reproduction” (p. 224), and not the heavy parental investment predicted by some models of how offspring status might assure long-term fitness. I suspect that these differing relationships will depend on the extent to which ecological conditions and societal institutions allow for the monopolization and/or transmission of resources across generations.

The book nears 500 pages and is structured in two parts. The first consists of descriptive demography: age-specific fertility rates, mortality rates, crude death and birth rates, age structure, and so on. This is a Herculean task. In the nonliterate population, people move around from year to year (sometimes day to day), divorce and remarry frequently, change their names, and can be referred to differently by mother and father! The second part focuses on adaptationist questions: inevitably a chapter on optimal birth intervals (Are they shorter than those of the !Kung because of the richer ecology?), the inferred contributions of siblings and grandmothers as helpers, the role of hunting reputation in men’s reproductive strategies, paternal investment, and monogamy. Without giving away too much (and I return to these topics below for those with the patience to read on), previous reports from Blurton Jones’s collaborators are

generally corroborated, but always with interesting twists and new puzzles, and inevitably with much better data.

In this review I can't do justice to the full contribution Blurton Jones has made to anthropology, nor to the fascinating exhibits in the online supplement. In the latter, in addition to supplementary data analyses (that I must acknowledge I did not peruse in detail) he compiles results from aerial surveys documenting changes in wildlife densities in the area, copies of some of his own field notes, a rich description of Hadza interactions with their neighbors over the preceding century, and a translation of Obst's 1912 visit to the Eyasi and Yaeda valleys describing the Hadza as "interesting smoking companions" and commenting on having never seen "such concerned mothers or such active family fathers." To get a few gripes out of the way, the Cambridge editorial staff should have insisted on chapter summaries—annoyingly, some do and some don't—with one merely petering out into a section called Comments on the Supplementary Information; relocating gems is difficult if (like me) you don't use a yellow highlighter, nor was the indexing helpfully organized. Also there are shockingly no (well-deserved) credits or endorsements on the outside of the book, at least not in my copy (which I confess I stole from the book display at EHBEA, albeit with the organizers' gracious assent). And then there is the price. . . . On the plus side, Jim O'Connell's wonderful photos illustrating fundamental life-history trade-offs, often as tender moments, bring the book alive.

I will focus my comments on methodological challenges and innovations, on bulleting some particularly interesting findings, and finally on a series of topics from Part II that might especially interest readers of *Human Nature*: optimal birth intervals, the role of grannies and helpers, and finally the role of men and marriage; it is only on this last topic that I see Blurton Jones as going wrong, and even then he redeems himself! Pick and choose from this review, as you will probably end up doing from the book too, if only because of its length—even though I assure you a thorough reading from start to finish is well worth the effort.

Methodological Challenges and Innovations

Small demographic datasets force us to combine retrospective information with cross-sectional measures, what demographers call "period" versus "cohort" approaches and what many of us now call a "mixed longitudinal" study. This approach raises challenges and opportunities over which Blurton Jones ponders deeply. Anyone contemplating the use of demographic data from small populations should attend closely to the multitude of biases that can arise from simply popping interview data into an excel file, running an r script, and producing a result. The most obvious problem is that this mixed methodology (and indeed any longitudinal study) risks confounding secular change with individual age effects unless special care is taken. Furthermore, all kinds of problems arise from selection bias, the fact that the people we interview (and especially those we can find on multiple field visits) are those who are most likely to survive into the present, and those who are least likely to have left the population for other reasons. As an example of both problems, consider the use of a logistic regression (birth/no birth) on annualized data controlling for age of woman and secular year to claim (as the author does) that Hadza fertility has not changed over time (no demographic transition).

There is more to attend to. First one must include a multilevel “mother effect” to control for the fact that more-fecund women produce more children, and second, one must acknowledge that the older years of more-recent women are not sampled, prohibiting the conclusion that demographic transition is not taking place if it is taking the form of having an earlier stopping age. Third, women with particularly stable marriages or normal reproduction may be more likely to fall into our sampling methods over multiple years. Blurton Jones sets new standards in contemplating the complexities here.

Mortality profiles pose special problems, particularly with respect to determining the denominator. For the younger years we can use all the years they have been alive, as determined from reliable reproductive interviews. But what about the older years? Blurton Jones used elapsed intervals for anyone who was observed more than once, and he used those years to contribute to an age-specific mortality calculation. The decisions he makes, and why, are instructive, and in many respects will provide a recipe for others, although I did wonder why he never used his sibling interviews (SI 9.7) for this task. When an ethnographer asks living individuals (especially firstborns) about the fate of their full sibs, this offers a nice unbiased window into past mortality rates (with the exception of the risk of mortality of firstborns!)

To attempt a generalization of how Blurton Jones deals with the complexities of small-scale demographic data, I would propose that his trick has been to create multiple files and retest ideas with differently selected data. For example, for women’s fertility he has a file with annualized data (birth/no birth) each year, a file with each child showing mother’s ID, date of birth, last observation or death, to which (progressively throughout the book) variables are added that code whether the father is still married to the mother, if a stepfather is present, or if the mother is living alone; and then a third file with two measures of reproductive success: age-specific number of live births (standardized residual on age) and age-specific number of living children at last census or death. These files, and a similar suite of files on marriage, are used repeatedly in the book and allow multiple testing of models and ideas. Again, a close reading repays the effort. And although there may be more elegant and concise ways now to tackle these issues of uncertainty, you (as a reader) have no suspicion that wool is being pulled over your eyes.

Particularly Interesting Findings

Not all the selected findings I list below are original, but they are typically better documented than ever before and of general interest to evolutionary anthropology. Findings directly pertinent to current debates in human behavioral ecology are explored in greater detail in subsections below.

- No evidence of any demographic transition (change in age-specific fertility over time) occurred between 1965 and 2000, although early stopping behavior among the youngest cohort of women is not ruled out (discussed on p. 135)
- High mobility between camps makes claims that hunter-gatherer communities are “small” suspect, particularly with respect to theories of disease transmission (discussed on pp. 106–7)

- Postmarital residence patterns are bilocal with a slight matrilocality bias (p. 109)
- No association between hunter-gatherer mobility and low fertility as compared between Ache, Hadza, and !Kung (p. 135), but evidence of lowered birth rates in years with no rain and higher rates at temporary settlements show adjustable sensitivity of fertility to resource availability (p. 120).
- Almost no evidence of resource budgeting in women's reproductive histories; in puzzling over how to control for age, and the related question of whether women who reproduce fast in their early years burn out or continue to out-reproduce others as they age, there is strong evidence that strong starters keep going at their higher rate (p. 247).
- Male RS variance is greater than female RS variance at all age brackets (p. 250)
- Men's hunting reputation is associated with more wives, shorter periods to remarriage, and younger wives, but with *lower* child survival (p. 425)
- There is always a pool (10–20%) of unmarried men and women between 20 and 40 years of age (p. 247)
- The number of spouses varies between 0 and 7 for women and 0 and 9 for men (p. 292)
- No evidence for lower fertility among polygynously married women (p. 297)
- No evidence that Hadza men avoid women with dependent children as wives (p. 299); in fact, divorced women with young children remarry faster than those without children, and with no trade-off in husband quality (as measured by reputation as a good hunter)
- Larger mothers wean heavier children (p. 316), and maternal weight is a correlate of her children's adult weight and height, yet there is no indication of greater fertility among larger women (p. 328)
- No evidence of maternal depletion, nor of any effect of a husband or mother on maternal weight changes (SI)
- Children who are large for their age are less likely to die (p. 320); also, diminishing return to child survival on age-specific size (p. 322), which reveals clear opportunities for maternal trade-offs between investing further in a current offspring and conceiving and/or investing in a future child (p. 330)
- Orphans grow less well than children with living parents (SI)
- Inter-birth intervals don't lengthen over a woman's reproductive career; women just stop reproducing, around the age of 35, although this depends on the presence of teenage helpers

Optimal Birth Intervals

It's a treat to see Blurton Jones return to inter-birth intervals after all these years. His and Sibley's innovative work on this topic in the 1970s and 1980s was seminal to the founding of what was then known as human sociobiology. He starts the chapter with the story of how testing for optimal birth intervals occurred to him one strange day in London—a surprise visit from the !Kung ethnographer Richard Lee followed by an evening train ride with optimality theorist Richard Sibley to an ethology conference in Italy. In this chapter on birth intervals, Blurton Jones returns to the old question with

new nuance. If you only read one chapter of this expensive book, standing in the aisle of one of the few remaining shops that carry real books, read this one.

He starts with the litany of problems (both his own and those identified by others) to plague the original 1978 study which shows how !Kung women space their offspring optimally at four years with respect to a backload model. He then develops a suite of new methods to address each flaw—namely, controlling for the age of the index child, conducting analyses that examine the effects of both the preceding and the antecedent interval simultaneously, excluding heterogeneity among mums, and examining maternal and paternal conflicts of interest over the birth interval. I don't want to give away the whole plot—suffice it to say that both the original-style analysis and the two modern alternatives show that Hadza women space their offspring optimally, just as in the !Kung data. The observation that there are rather more short intervals than predicted leads Blurton Jones into characteristic soul-searching. Part of the problem is shown to reflect data coded as being of somewhat dicey quality (any good ethnographer should adopt the habit of coding data quality—it can be very useful later), and part of it to different interests among fathers and mothers—with the former favoring shorter intervals because their likelihood of being married to their current wife diminishes with time.

I'd wager that this anthropologist has thought more deeply about the complexities of studying inter-birth intervals than anyone else on this planet. Additionally he describes the procedures of his analyses with such clarity and detail that you could give a demographic data base to any promising undergraduate and they could follow the steps. The chapter is also a delight to read as an example of how the study of a particular question has matured over a 38 year interval.

Grannies

Unsurprisingly, grannies feature heavily, with two chapters dedicated to them. Blurton Jones has already published data showing that grandmothers choose to live where they have most effect (where there are young children and no other living granny), but in these chapters he presents us with much more. He takes seriously the standard criticism of “grandparent effect” studies, namely that grannies will move to where child outcomes are most at risk, rendering meaningless studies that compare households with and without grannies, lacking genuine counterfactuals. Accordingly he compares children with living versus dead grandmothers. He supports earlier Hadza findings that grannies enhance child growth rates, using more appropriate analyses that nest comparisons within mothers and grandmothers. He finds that children with living grannies are heavier and have higher upper arm circumference (UAC) scores than those whose grannies have died, but no effects for BMI and height. The multilevel structure of the analysis is important in that it controls, to some extent, for heterogeneity—longer-living grandmothers produce higher-quality grandchildren.

The probability of child survival is also enhanced by a living granny, as shown in both annualized regression and survival analysis, again robust to heterogeneity, maternal weight, and orphaning. Most of these analyses focus on the maternal grandmother, but paternal grandmothers are shown to play an important role in enhancing child survival if there is no living maternal granny. Despite suggestions that grandmothers may allow humans to space their births more closely, there is no Hadza evidence for

their presence shortening IBIs, although there is evidence that mothers can support “successful” short intervals (which here means keeping both children alive) more effectively if their own mothers are still alive.

In sum, the first chapter on grandmothers presents considerable evidence to support the idea that living maternal grandmothers enhance survival of children between their first and fifth birthdays, especially those born to mothers under 35 years of age. With the quality of Blurton Jones’s data under our belts, the review of comparable studies on other hunter-gatherer populations (!Kung, Ache, Aka, Martu) is somewhat inconclusive, if only because each study has different shortcomings. Indeed, he concludes that although “we have insufficient evidence to claim grandmothers are effective helpers among hunter-gatherers as a whole, there are indications that they tend to behave in ways expected to benefit their grandchildren” (p. 381). As with birth intervals, Blurton Jones has again seriously raised the bar in this area of study.

In the following chapter, Blurton Jones turns to the analysis of intergenerational conflict between parents and grandparents, and he finds surprisingly little evidence of competition (and hints of synergy). First, statistical analyses find no negative effects of grandmothers’ age-corrected fertility on daughters’ or daughters-in-law’s age-corrected fertility—if anything, the association is positive. Second, a woman’s children are more likely to survive if her own mother has small children, again counter to predictions of reproductive conflict, and suggestive of synergies rather than resource competition. Third, looking now specifically at paternal grandmothers, it is the older women who appear to be winning—namely, a woman’s children are *less* likely to survive if her husband’s mother has small children, contrary to the predicted “daughter-win” outcome of current models for how asymmetries in relatedness between grandparents and grandchildren might influence intergenerational competition. There are many possible interpretations here, and the samples are quite small, but these results prompt new ideas, grounded as ever in Hadza ecology.

In ruminating on models for intergenerational conflict, Blurton Jones comes up with even more reasons why the younger generation should be willing to put more into reproductive competition than the older generation, or why daughters should win. First, in a growing population there is stronger selection on early (rather than late) reproduction (though there may be quibbles with the logic here). Second, in a system such as the Hadza’s, with high rates of divorce and remarriage, the older woman is always related to her daughter’s children, whereas the children of the older woman may only be half siblings to her daughter (when fathered by a new man). In both cases the younger woman has more to gain from investing in reproduction, even if this is costly to her mother. So with these theoretical expectations, why do we find *no evidence* of daughters winning? Blurton Jones puzzles over this, and he offers some suggestions. First, the period of potentially serious reproductive conflict in the Hadza is short, or at least older women continue to reproduce at a time when their daughters and daughters-in-law have only a few small children and are not heavily burdened; hence the scope for competition over reproduction may be small. Second, maybe this is a data blip, perhaps a privileged ecological and/or demographic cohort in which the costs of competition are muted? Third, maybe (despite all her famous tuber-digging) granny’s real contribution lies in providing non-depreciable (or “umbrella”) care (such as babysitting) that cancels out the costs of having to dig tubers for two sets of children? This is a reformulation of an idea offered before in the Hadza context—that we should perhaps be looking at joint

reproductive payoffs (here to mothers and daughters, or daughters-in-law, working as a team) where the ecology offers scope for economies of scale. All of this discussion leads to the idea of a finely tuned maternal reproductive system that has evolved to continue reproducing into her forties only when this won't negatively affect her daughter—for example, if the older woman is in particularly good physiological condition, or if she has a lot of helpers of her own (more on team care below). It also highlights the need for future researchers to remember how difficult it is to study costs and benefits without being able to experiment.

Children as Helpers

A chapter on children as helpers presents a thoughtful set of analyses, recognizing that siblings are both helpers and additional mouths to feed. An interesting observation to emerge from the Hadza data is that a child under 5 years of age is much more likely to have a living grandmother than to have an elder (6–18 years old) (maternal) sibling—demographically this makes intuitive sense, but its implications are often forgotten in evaluating the potentials for allocare. Furthermore, there is no evidence that elder siblings affect child weights, and there are only very weak effects of older sibs (aged 6–12) on the survival of their younger sibs (an effect that disappears when grandparental status is accounted for). Rather intriguing data show that a woman's age-specific annual probability of giving birth is much enhanced by having teenagers, even controlling for woman-specific heterogeneity in birth rates, although the effect seems to be driven by early cessation of fertility among a few women with no mother, no mother-in-law, and/or no teenagers. This raises interesting questions for further studies of the end of the reproductive period and leads to the point made earlier that the maternal reproductive system may be finely tuned as to when to stop (a topic rarely investigated) as well as when to start (much modeled). The takeaway message regarding children as helpers is nevertheless that if elder children are doing anything to help their mothers reproduce, it is in providing umbrella care that allows mothers (and perhaps grandmothers) to forage more effectively. Here again the author is building on earlier suggestions from Hadza researchers that we should focus more on team care rather than on divisions of labor that maximize individual fitness. Clearly the implications of this shift in focus of analysis need more in-depth modeling.

Marriage and the Roles of Men

Unsurprisingly, given the earlier work from these Hadza researchers, there is much interest in the role of men and the related question of why women marry.

Fathers get a shortish chapter, starting out with the interesting observation that despite high divorce rates in the Hadza, 72% of children live with their father at their fifth birthday, and 49% by age 15. There is no evidence of an effect of father presence and/or his hunting reputation on young children's weight, even in contingent situations such as when no grandmother is available to care for the child. This finding holds for children less than 3 years of age, despite a previous report from this population that men with small children bring back more food. Looking at child survival, Blurton Jones finds that children of good hunters are *more*, not less, likely to die, even if the father is still married to the mother. Children aged 5–13 do nevertheless weigh more and present

as taller if they have a live-in father; interestingly, it is the presence of the father, not his identity (as determined by a father effect in a multilevel analysis), that is key. Furthermore, if the especially good hunters (those nominated more than 5 times and constituting 20% of the adult male population) are removed from the sample, there is a slight effect of father's presence on child survival. In short, there seem to be variable male strategies—good hunters, despite their higher RS, don't contribute to their children's survival, whereas “ordinary Joes” may benefit their kids somewhat. Rather incongruously, Blurton Jones draws the general conclusion that husbands have little effect on the growth and survival of their kids, whereas in fact clear effects are seen, especially as these children grow older. Although it is correct to emphasize that these father effects are not as strong as those of grandmothers, it seems an opportunity wasted not to examine multiple male strategies more closely.

In addition to showing these rather limited father effects, Blurton Jones is intrigued by findings that Hadza men with greater reproductive success are those with the best hunting reputations, who spend more time married, who divorce and remarry most quickly, and who marry younger wives. Clearly there is reproductive competition among men as exhibited through success in marriage, hunting, and most likely in building their social networks. This leads him into a strange analysis, presented in the earlier chapters of Part II, in which he claims to find no positive effects of marriage on Hadza women's fitness. From this analysis he claims no support for the conventional sociobiological bargaining model of marriage—that couples exchange paternity for paternal provisioning.

The key idea then is that marriage should lead to more successful reproduction for both sexes. This he tests with the prediction that a greater proportion of adult lifespan spent in marriage will result in higher fitness, for men because they have greater sexual access, and for women because they enjoy paternal/spousal provisioning. I have three problems with this: the reading of the data, the logic whereby the prediction is based, and the attempt to link the findings to Bateman's gradient.

The data show that Hadza men who spend much of their adult life married have the highest rates of RS (Fig. 15.8a), whereas there is no systematic relationship for Hadza women between their success in keeping children kids alive and the proportion of their adult life they have spent married (Fig. 15.8b). The finding for men could reflect reverse causality (as Blurton Jones acknowledges)—marriages break down (thereby reducing the percentage time spent married) under conditions of childlessness (or low conception rates). More seriously, though, the inference from Fig. 15.8b is flawed: it could be that women who spend most of their adult lives married can space children more closely (in fact, Hadza fathers have been shown to bring home more food when their wives are breastfeeding, and that this shortens the inter-birth interval). As such, the higher child mortality experienced by constantly married women might result from closer birth spacing. Worse, Blurton Jones seems to ignore the strong positive association for women between percentage of adult life married and overall reproductive success (Table 15.3, panel A) for both men *and* women. As I read the table, the regression coefficients are all positive, and mostly highly significant. The unstandardized b coefficient is somewhat higher for men than women, but this is unsurprising given the greater variance in male RS; furthermore there is no test for difference in slope. Given that number of living children is surely the most comprehensive indicator of overall contribution to the next generation, I can't understand why this result is

ignored. In short, I am not sure that the fact that there is no positive association between offspring survival and percent adult career spent married means women don't use marriage as a way of bettering their reproductive chances given the clear evidence that proportion of adult lifespan married is associated with women's reproductive success. But there is more that troubles me.

Why are the benefits of marriage measured through the time spent married? In my book, if husbands are important and I am stuck with a bad one, best to ditch him and skip to another, even if it costs me a little time. In other words, I am not sure that the percent of adult lifespan married is a good indicator of the importance of marriage for fitness for either sex; indeed, this is a criticism to make of a slew of recent studies that use divorce frequency to assess the importance of marriage in humans. And although Blurton Jones cautions me (personal communication, 2016) that percent of adult lifespan spent married is not a measure of staying married to one person, the two are undoubtedly correlated given the quite substantial intermarriage intervals (a mean of 4 years and median of more than 2 years for each sex [p. 296]) and the fact that most married adults (men, 58%; women, 61%) have more than one marriage.

Blurton Jones's argument bleeds here into a broader critique of the use of the Bateman Gradient (technically the effect of *mate number* on RS) that suffuses the book. Here Blurton Jones's specific position is difficult to understand. He proposes that his measure of mating success, the *proportion of adult life span spent married*, is a more appropriate measure than a simple count of mates. Further, he labels his measure the "Bateman Gradient," even though it really isn't. There is some intuitive sense to his strategy—it's good to be able to retain mates if they are high-quality ones. Furthermore, it is correct to recognize that the effect of *mate number* on RS is neither the only, nor indeed necessarily the most interesting, dimension on which the two sexes differ (a point now well recognized in sexual selection theory). That said, the ability to stay married over your adult lifetime is an odd proxy for "mating success." In fact, his usage of the "Bateman Gradient," insofar as high values would tend to characterize men and women who have all their children with a *single* spouse, is almost directly opposite to how evolutionary biologists use the term (high values for individuals acquiring multiple mates)! This means that when he brings his results to bear on interpretations of others' work (including my own), it's not surprising there is quite some confusion.

But, as I noted earlier, even here Blurton Jones partially redeems himself, exhibiting his characteristic wisdom and balance. After noting that his findings question the "exchange of paternity for care/resources" model for marriage, he quickly limits the remit of this statement, claiming only that "the Hadza show us that marriage *can* exist without such an effect" (p. 308, emphasis added), referring to a father-present effect on child survival. In other words, he is not challenging the findings of other studies (and in turn I feel rather bad about challenging his, but I think it is worth pointing out that his inferences are possibly flawed). Furthermore, he makes the far more incisive point that marriage may not be important for the provisioning of children among the Hadza because women have free access to abundant and nutritious plant foods, and single women have access to meat from large game caught by any man in camp. Hadza women enjoy remarkable economic autonomy. And more broadly, our differences point to the huge scope that might lie in investigating far more closely, from an evolutionary perspective, the fact that marriage may have entirely different functions in different

ecologies and production systems, following intellectual ancestors such as Meyer Fortes, Edmund Leach, and Rodney Needham.

Parting Nuggets

Rather hidden in the discussion of grannies and helpers are a few suggestions, undoubtedly controversial, with which I would like to conclude: (a) On contemplating the failure to find competition between mothers and daughters over reproduction, a rather remarkable observation is made. Studies of hunter-gatherers, long hailed as providing the best evidence with which to test models of the evolution of human life history, have some serious unexpected shortcomings that don't affect studies of farmers and herders. The proposal here is that the extreme flexibility of residence patterns, and the extensive patterns of food sharing, may serve to *conceal the trade-offs* we are interested in, trade-offs that are more sharply evidenced in societies where residence and food sharing is highly constrained by lineage principles (p. 397). (b) Hunter-gatherers cannot be viewed as cooperative breeders (at least in the strict sense, though we should not forget some subscribe to broader definitions) insofar as there is little evidence (at least among the Hadza) of reproductive conflict, there are few costs of changing groups, and the extensive food-sharing ethic reduces the risk of serious food shortfalls. In this way, Blurton Jones sees no evidence of the isolated reproductive units, riven by inequities, that are typically modeled and observed in studies of cooperative breeding in nonhumans. Blurton Jones's claim here will undoubtedly raise some eyebrows, though he is undeniably right in noting that under such conditions the consequences of evolutionary conflicts will be different (p. 397). Finally, (c) for anyone interested in variability among hunter-gatherers, the final chapter raises some hares worth coursing: how in warm climates do forest, savanna, and desert environments impact social structure, linearity, residence rules, intergroup relations, fertility, and male provisioning? It would be nice to see this material expanded for a peer-reviewed journal and (as Blurton Jones notes) tested by "an enterprising student with access to the several relevant data bases that now exist" (p. 442).

Monique Borgerhoff Mulder is professor of anthropology at UC Davis. Her research focuses on sexual selection, life history theory, and natural resource management. This work is based at field sites primarily in East Africa and examines implications for both evolutionary theory and development policy. She directs research at Savannas Forever Tanzania, coordinates applied interventions in western Tanzania, and is currently exploring cultural evolutionary models for carbon emission reductions in Zanzibar.