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Motherhood, methods, and monkeys: an intertwined professional and personal life

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Ever since I was a little girl, I wanted to be a behavioral ecologist, or live in Africa and study monkeys, or… No, my sense of careers was much more limited, as were my experiences with wildlife.

Born in New York City in 1940, I was raised after infancy in suburban Maryland just outside Washington, D.C. in a sequence of small apartments/flats that were imbedded in stretches of such apartments, with scattered patches of grass lawn and occasional playgrounds populated with swings and slides and climbing bars. With almost no public transportation, no family car until my late preteen years and bicycle riding restricted to sidewalks, I had little exposure there to the world of animals that would become so important in my life, although I believe that I was always an observer of people. Ecological context was reinforced by family: my mother was highly allergic to virtually all mammals (except us), my father was extremely and increasingly overprotective, and both were New Yorkers to the core, by both background and temperament. No, I did not start from an attraction to nature or animals or animal behavior. I started from a love of reading and math and science, a love of puzzles, a thirst for playing with ideas and relationships and alternatives, all of which were more a part of my upbringing and inclinations than was the world of nature. The animals, behavior, the passion for African savannahs and North American woods would come later. Interestingly, my sister eventually became a farmer in rural West Virginia; what a puzzle our environmental choices were to our city and suburban-focused parents!

Leaders in Animal Behavior: The Second Generation, ed. L. C. Drickamer & D. A. Dewsbury. Published by Cambridge University Press.

Academic excellence was highly valued by my parents both in principle and in practice, but the message was a mixed one for a girl: a college education was a must, but a must in the nineteenth century model, in order to be a better wife and mother, and also to have an employment skill 'in case' something happened to one's husband. At the same time, I later came to feel that, unlike my younger sister, I was treated as 'first son', the one who was expected to inherit parental values and traditions. Confused and dissatisfied and with little sense of the route to shaping my future, but in classic 1950s fashion, I lived an unquestioning and relatively unexamined life in many respects. Yet I loved math and science, was eager to travel, and so somewhere developed the idea, I think from an elementary school teacher who had previously taught at Army bases around the world, that I could earn a degree in math, and then travel the world by teaching math everywhere. Although my father had the not uncommon view that teaching was a lower calling than 'doing', I had no idea what else I might do with a math or science degree, especially if I also wanted to travel. My maternal grandmother somewhat reluctantly granted my radical high school graduation wish for a slide rule, agreeing to the purchase provided that I would buy a slide rule that was small enough to hide in a purse, a considerable concession for her I suspect, and one that conferred a great feeling of possibility to me.

University was affordable for my family if I went to a nearby public college where I could live at home, and so when my father was transferred to Los Angeles as I finished high school, my journey as math major started at UCLA after a summer job at the National Institutes of Health (NIH) in Bethesda, Maryland. I returned to that job after a first year at UCLA, meeting there a tall, handsome, and captivating biologist, the person who would become the love of my life and introduce me to a totally different world. Stuart Altmann was completing his Harvard PhD thesis in biology after spending two years studying rhesus monkey behavior on Cayo Santiago, an island off Puerto Rico, working for NIH. 'I went to the zoo with a zoologist' read my simple message home after our first date. When, during our subsequent short-term coast-to-coast 'courtship' we decided to marry, Stuart told my parents that I wouldn't have much money but would experience a lot of travel! They took a while to recover from their shock and dismay, not at Stuart but at such an early marriage. When we started a family two years later, my mother's wail was 'a baby before a BA?' To this day, however, I remain amazed that at 18 I made the best decision of my life, one I've never regretted. That I was able to raise two truly amazing children and then also have a wonderfully satisfying career, strikes me as such marvelous luck. It did take luck on top of commitment and passion and a few very important sources of encouragement, including Stuart, to surmount the primarily gender-related obstacles that I often found discouraging and demoralizing. Serendipity played a major role in my professional and personal development over the next 20 years.

I had enjoyed and was challenged by some of my UCLA courses and had a range of interesting part-time math tutoring jobs, but the University was not the right academic environment for me, particularly as the math department was open about not wanting women math majors as they would be a waste of time; I was ready for change. With little money and only a single academic year before Stuart would submit his thesis, I enrolled as a part-time math undergraduate at MIT, obtained a part-time job doing computer programming for Beatrice Whiting at Harvard, and helped Stuart with data analyses for his thesis (S. Altmann 1962), spending many a delightful brown-bag lunch among the ants with Stuart's advisor, Ed Wilson. Thus began my education outside the classroom, my exposure to a number of outstanding academics, and my experience of a number of the research threads that would ultimately be woven into the tapestry of my academic life. Beatrice Whiting and her husband John designed and directed an ambitious six-culture study of child social behavior (Whiting & Whiting 1975). In 1959, this project was radical in a number of ways that I only fully appreciated later. Six field teams spread around the world to collect data in standardized ways to address hypotheses about the process of socialization and the cultural contingencies involved. Such a level of hypothesis testing, systematic data protocols, planning, and coordination were unprecedented at a time and in a field in which individualism and exploration still predominated. The premise and execution of this distributed, simultaneous field work is all the more striking when one considers the status of international communication in the late 1950s. Moreover, taking advantage of the recent development of computers, data were going to be entered (onto computer punch cards!), verified and subjected to exploratory analysis as they came in from the field. This resulted in my introduction to computer programming and provided my first experience with this emerging technology several years before I would otherwise have had that exposure. Only half jokingly, the Whitings, who knew Stuart's thesis project, suggested that perhaps we should add the Cayo Santiago rhesus monkeys as a seventh culture to see what would be interesting in the comparison.

As a result of working with data from both the six-culture study and Stuart's rhesus monkey project, my first introduction to behavioral field research was one that treated as normal a focus on research design, data analysis, hypothesis-testing, and data comparability, not common at the time. I also developed from the beginning the idea that discipline boundaries were permeable, that good, useful ideas and techniques were to be found in different fields, and that humans were an interesting species to compare in rigorous ways to nonhumans. This was an essential mindset for me a decade later when I began the investigations of behavioral sampling methods that led to my observational methodology paper (Altmann 1974). Perhaps it was just as well, at least for me, that I was unaware that the highly respected woman who was the lead director of the six-culture study and who mentored me for that academic year by outstanding example did not have tenure, and would not receive it until she was almost ready to retire decades later. However, this oblivion, like that regarding school racial segregation until I was in secondary school, is shocking and embarrassing for me to imagine now, especially since during those same times many of us were aware of, and grew up experiencing, religious prejudice and segregation.

My exposure to computer programming was unusual at the time and was reinforced in the summer of 1960 by working at NIH again, this time in Wilfred Rall's group in the Office of Mathematical Research, which was exploring mathematical models of neural networks. There, I again encountered mind-stretching and stimulating science from mentors who encouraged me simply by inclusion, thereby communicating implicitly that I was doing what I was meant to do.

Once again I transferred universities, this time as Stuart assumed a faculty position in Zoology at the University of Alberta in Edmonton. The math department there was welcoming and challenging, and I was invited to join the honors program for my final year. However, I declined because by then I was expecting our first child and would need to attend school part-time, an option that I learned wasn't available for an honors degree student. Despite this disappointment, finishing my degree part-time in the evening and spending most of my time parenting was not all disadvantages. I cannot imagine, either emotionally or intellectually, having missed the intimate and intense involvement in raising my children, at once the most challenging and fulfilling experience of my life. At the same time, my intellectual yearnings required more. My enjoyment of mathematics was greatest in pure math, and my success and pleasure was greatest when I lost all sense of time or place for innumerable uninterrupted hours or even days, when I was in a state that has been termed 'Flow' (sensu Csíkszentmihályi 1990), something that at least for me was incompatible with being the primary child-care person at home. Although I was unaware of it at the time, my parenting experience was probably a significant fork in the road that eventually led me to focus on behavior rather than mathematics – yet it would be over a decade before my professional direction was really set.

A little over a year later, in 1963, our toddler Michael and I joined Stuart for what was to be a 15-month study of primate communication in a natural setting, and the plan was that as Stuart and a PhD student from another university conducted behavioral observations I would promptly tabulate and analyze data they collected, a radical idea at the time and still relatively uncommon. However, this was the explorer's era in tropical field research, even more so in studies of large mammals, and many scientists wanted 'their own species'. Because a previous 10-month field study had been conducted on baboons in a park on the outskirts of Nairobi in Kenya and another short study had been conducted in South Africa, the PhD student was concerned that baboons had 'been done'. The student dropped out of the project at the last minute, and I joined Stuart in observations. What an amazing, life-changing year that was in so many ways.

At 23 and with camping experience that was limited to a very happy summer week spent in Girl Scout camp in the mountains of Virginia during several successive middle school years, plus a camping honeymoon that involved two weeks of camping from Los Angeles to Cambridge, Massachusetts, I had no expectation, positive or negative, of what life would be like when we left Edmonton for East Africa, with stops particularly for Stuart to meet with Robert Hinde in Cambridge, Detlev Ploog and Konrad Lorenz in Munich, and Hans Kummer in Zurich. In each case, Stuart's stimulating science discussions were complemented by more personal ones as the wives and children welcomed Michael and me with great generosity. Vreni Kummer provided my first advice and hints of experiences to come 'living in the bush'. It's difficult to describe how unusual and welcome such warmth by strangers was, especially in Europe of 1963, a time when hoteliers were even surprised that we travelled with a child, particularly with no nanny for caretaking of him and to feed him out of sight rather than in the public dining room!

In Nairobi, we were again fortunate, in that Irv and Nancy DeVore were back in Kenya for a brief return to the site of their Nairobi Park baboon research. Before Irv's first trip to study baboons, he had visited Cayo Santiago to learn Stuart's behavioral data collection methods on the rhesus monkeys, and the DeVores visited us at Harvard on their way back from Kenya at the end of their study. Now, we visited Nairobi Park together, and they were generous with advice on everything from baboon observations to shopping. Although Irv and Nancy were confident that we would settle in Nairobi Park with all its advantages of the nearby city and habituated animals, they also suggested some other Protected areas we might visit in Kenya and northern Tanzania (then Tanganyika) in our search for field sites. These included Amboseli, in Kenya, where Irv had spent a few weeks during his field study. With advice from various local wildlife biologists as well, we developed a route and shopping list, then began preparing for two months of independent living and travel on the primarily unpaved roads. Our goal was to survey olive and yellow baboon populations of southern Kenya and northern Tanzania in search of a field site that had relatively undisturbed populations that would be sufficiently visible for detailed behavioral studies. We bought a used long-wheelbase Land Rover and outfitted it simply to store all we would need while being able to use it for toddler's playroom and adults' rooftop observation site during the day, sleeping quarters at night (Altmann & Altmann 1970). While vehicle modifications were being fabricated, and when transport was available, we began to develop an eye for baboons and baboon behavior through observations at Nairobi Park where the tourist-familiar baboons were readily observable.

Despite Nairobi's inviting climate and gardens full of native and exotic flora, we were eager to leave the city for our 'safari', and we began our travels shortly after Michael's second birthday, as soon as the vehicle was ready. First on our route was Amboseli Reserve, at that time a dusty seven hour drive to the base of Mt. Kilimanjaro. From mile-high, lush Nairobi, we descended across the Athi plain on the unpaved main Nairobi–Arusha road to the border town of Namanga, where we continued to descend, on a less traveled route, into the Amboseli basin, centered on the now-seasonal remains of the Pleistocene Lake Amboseli. Here in the Amboseli–Longido basin was my introduction to tropical ecosystems and for me, unlike for most tropical biologists, it was on the East African savannahs that I 'imprinted'.

Amboseli was home to a large population of the least disturbed and most observable baboons that we were to see anywhere over the next two months. As highly terrestrial, omnivorous, and highly social primates, baboons, even more than other cercopithicines, were, and remain today, ecologically as well as geographically widespread, frequently coming into contact and conflict with humans and their livelihoods, especially in agricultural regions, increasingly so as agriculture increasingly expands at the expense of other land use. As a result, baboon (sub)species throughout Africa often either flee humans or approach them, particularly near tourist or park ranger areas, where baboons come to see humans as sources of food. Then, as now, African wildlife in many areas are preserved and most unaffected by humans in the arid and semi-arid regions that are unsuited for agriculture and are inhabited instead by pastoralists such as the Maasai. UC-Berkeley PhD student Tom Struhsaker had surveyed areas in Kenya and Uganda for study sites for his vervet monkey research, and he, too, had settled in Amboseli for similar reasons (Struhsaker 1967). In 1963–4, Amboseli was a

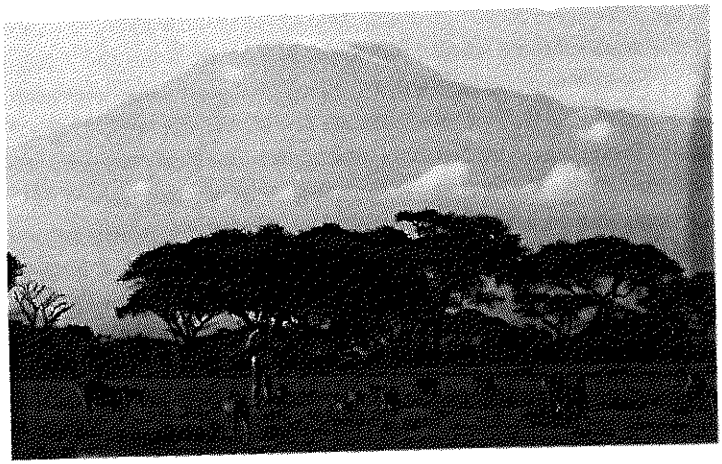

Figure 2.1. Baboons foraging on the savannahs of Amboseli, Kenya, at the base of Tanzania's Mt. Kilimanjaro. The animals were already relatively tolerant of observers in or on the roof of a vehicle when we first went to Amboseli, but they initially fled from people on foot. Since the mid-1970s all our observations have been conducted on foot.

Figure 2.2. Baboons are one of the most highly social of primate species. Even in this semi-arid environment where as much as three-quarters of the daytime is spent foraging, time is always taken for social grooming and resting.

large Maasai Reserve, devoid of tourist accommodation except for a small tented camp, and open to visitors only in the dry season because the access road, which was transected by a seasonal river, unpredictably flooded during rains. We had the whole area almost to ourselves much of the year and had no communication or supply resources except for monthly trips to

Nairobi or Arusha, which we alternated with Tom, not very far from whose tent we pitched our own two tents. The isolation also meant that we were on our own for all tasks from water-hauling and diaper washing through cooking. For much of the year, we couldn't find any help that would do these tasks, no-one who would live in a tent where lions, elephants, buffalo, and leopard were regular visitors. All in all, this was a year of maximum multi-tasking as childcare and camp living activities were juggled along with searching for and observing baboons, good preparation for the rest of my life. By her own example, my mother had taught me that if a mother's employment wasn't essential for family survival, one would gain opportunities to pursue non-domestic activities by being faster and more efficient at the domestic ones, and it was a message I internalized early on.

That first year in Kenya was an immersion introduction to fieldwork, animal behavior, and baboon ecology for me, while baboons and the other wildlife were the focus of our family's life, because the year was also one in which we had more life-threatening medical crises than we were to have in total since then, many of my animal behavior memories from that period remain diffuse and fragmentary to this day. My field 'year' suddenly was cut short in April when our son suffered from a complete paralysis from the neck down. We barely succeeded in crossing the river to reach Nairobi, where he ultimately was kept near an iron lung for the next two months until he recovered enough for me to return with him to the Clinical Centre at the National Institutes of Health in the US. After four months of separation and with much relief, our family reunited when Stuart joined us from Kenya in August. Anyone who has experienced the thrill of a child's first steps can perhaps imagine the even greater thrill of a child's first steps after a life-threatening paralysis.

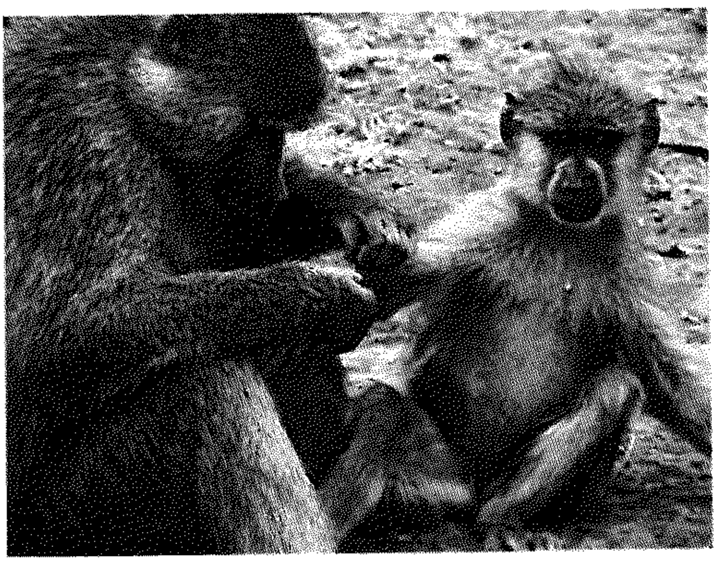

Figure 2.3. Baboon females form close, persistent ties, particularly with mothers, daughters, and other close relatives in this matrilocal species. Mothers' initial grooming of her infants of both sexes develops within a couple of years into reciprocal relationships with daughters though not with sons.

What was to be our last year in Canada was a busy one personally and professionally. I gave my first scientific presentation in December, on progression order in baboons, in a symposium Stuart organized on *Communication and Social Interactions in Primates*. I then spent much of my available work time that spring helping edit the resultant University of Chicago symposium volume (Altmann 1967), both of which were important 'firsts' for me. Reviewing and editing a diverse range of papers introduced me to the literature in a new way and helped me continue to develop my interest in methodology. The new year also saw Stuart hospitalized for a back injury and me for a mid-pregnancy miscarriage, finally bringing to a close our 'medical year' that had begun in Kenya. That summer, we prepared to leave Edmonton, headed to the suburbs of Atlanta, Georgia, and to Emory University's Yerkes Primate Research Center. We made the move in our sort of style – Stuart, Michael, and I, with camping supplies for a few weeks, piled into or on top of our royal blue Volkswagen 'beetle' complete with bright red aerodynamic wooden roof carrier that Stuart constructed for the journey. What a sight we must have been at the campsites along the way as we and our belongings spilled out in the evening and folded precisely in again, usually the next morning – we couldn't take too long on this journey as I was mid-pregnancy again, and the 'beetle' had no spare space.

We located a small wedge-shaped and steep wooded lot near Emory and began having a house built to nestle in the hillside. A few months after our daughter Rachel was born, we happily moved into the 'construction zone'. I initially joined Stuart part-time analyzing the data from our Kenya year. But within a year the initial promise that I could work with Stuart was rescinded as Emory extended their nepotism rules to the Primate Center. Stuart offered to fight this decision, but various prior exclusions left me demoralized and unwilling. Instead, this seemed an ideal time to explore my early interest in teaching mathematics beyond the tutoring that I had always done to varying degrees ever since I began university.

I soon learned that I was in the right place and time from the perspective of the nearby public school system as well. The racially segregated school system was finally about to integrate, under pressure locally from civil rights organizations and many African–American parents, and nationally from the federal government. One type of 'carrot' provided by the federal government came in the form of additional funds for math programs in the historically black schools. Because math teachers were already in very short supply in the state, temporary teaching certificates were available to those who had a math degree but not an education degree. I first did substitute teaching in the recently integrated high school. When I applied for a regular job and distressed the administrators by answering honestly their questions about religion and frequency of church attendance, they promptly decided that the way to deal with several 'problems' at once was to offer this Jewish math major a one-year job developing and implementing a remedial math program in the town's historically black elementary school. I enthusiastically jumped at the opportunity for social and professional reasons. I spent 1967–8 in one of the most stimulating, educationally satisfying, and exhausting jobs of my career as five of us joined long-standing black colleagues, replacing those of their colleagues who integrated the previously white schools during one of the most historic civil rights and political years of the 1960s. At the request of the parents of my poverty-level students I started an evening math class for parents so they could help their children with homework, and I taught school-time enrichment for the top math students in my son's school. I subsequently completed my Master of Arts in Teaching (MAT) degree in math, continued tutoring, and worked part-time supervising math student teachers. At every turn and task, though, I found myself observing behavior and crossing disciplines, as when I found that teaching or tutoring math often involved teaching language and logic skills, or noticing handwriting impediments to arithmetic performance in low-performance students, or noticing the extent to which students of all ages devised much more creative ways to solve math problems or even do basic arithmetic tasks than the single proscriptions taught in the schools. Neither I nor my students ever fit neatly into the boxes to which we were assigned, something that I didn't originally recognize or find comfortable but with which I was increasingly becoming comfortable. My slow and somewhat twisty path was perhaps the right and even necessary one for me.

I'm not sure what route my career might have taken if we had stayed in Georgia, but in 1969, Stuart received a research grant that enabled us to return to Kenya for a few months. Soon afterwards, he also accepted a faculty offer from the University of Chicago. At the same time, Stuart made a suggestion that ultimately helped tip my career direction back toward behavioral studies. He thought researchers would find useful a guide to ways of analyzing behavioral data that had been gathered in different ways. I was intrigued by a convergence of my interests. With an impending move, I was not in a position to take on employment beyond occasional tutoring, so I dove into the observational behavior literature to get a feel for the current state of the topic. Being an outsider to the field, this was something that I particularly needed. For the same reason, I could also bring a fresh perspective. By the time we settled in Chicago in autumn 1970, I was 'hooked'. We returned to Amboseli in July of 1971 to begin habituating a group of baboons, Alto's Group, one of those that we had censused and followed for some months in 1969. Alto's Group became the study group for Glenn Hausfater's thesis research in which he tested Stuart's earlier queueing model of male priority-of-access to mating opportunities (Hausfater 1975). Thus, what later became a long-term research program began with a focused short-term study, as has been the case for most if not all research projects that developed into long-term ones and that have provided models for recent ones that have been started with explicit long-term goals.

With our younger child, Rachel, now in school for a few morning hours each weekday, I was able to focus some time on reviewing the methods in observational studies that ranged across species from ants and wasps to humans, and from an array of disciplines including psychology, education, anthropology, and zoology. A few striking findings were soon evident. The first was that for only a small proportion of studies, and a particularly small proportion of field studies, were the descriptions of the behavioral sampling adequate to identify the key characteristics that one would need to make analysis decisions. Second was that well-described sampling schemes were virtually never justified in terms of their ability to answer the behavioral questions in which the authors were interested. And finally, the strengths and weaknesses in these respects were distributed across academic disciplines, taxa being studied, and researcher seniority. Consequently, the topic of my emerging project shifted from data analysis to the nature of relating behavioral questions to data collection, and my original cross-disciplinary tendencies became reinforced. Although writing has always been difficult for me, the project was such a perfect fit and so fascinating, and Stuart was so encouraging and willing to comment on successive drafts, that my earlier discouragement and fears gradually receded. When I had a draft of *Observational Study of Behavior: Sampling Methods* (J. Altmann 1974) ready to share more broadly, I sent it to many animal behaviorists, and was gratified when several generously gave me valuable feedback. Stuart declined my offer of co-authorship, years later telling me that he thought I would only receive adequate recognition if I was the sole author; with hindsight, I think he was probably right.

The sampling paper was in many ways the right paper at the right time. A need was there, and the paper's use spread rapidly even before publication. The approach was a very simple, first-principles one. It was not mathematical other than basic ideas of sampling spaces, estimation, and question formulation, but even that was unusual at the time, and unfortunately, sometimes missing or misunderstood even now. It was intended as a guide to thinking and planning and design, with the introduction as important as the subsequent evaluations of existing sampling methods. A recent small volume by Marian Stamp Dawkins provides a marvelous expansion on this theme (Dawkins 2007).

By the time I submitted the sampling paper to *Behaviour* in 1973, Rachel was in school 'full day', and to some extent thanks to my work on the sampling paper, I decided that I wanted to enter a PhD program, one that would lead to a career in behavioral ecology. I also chose to focus my research on baboon females and their young infants. This was a difficult decision, because in naturalistic studies, particularly of primates, this was considered primarily a women's topic, especially in an era that focused on male aggression and reproduction as the evolutionarily important topics in selection, in differential reproduction, and in survival. This perspective didn't make sense to me; rather, I thought it was clear that in natural environments the survival and reproductive challenges faced by female mammals were at least as great as those of males, especially so in species such as primates that experienced disproportionately long periods of gestation, lactation, and infant carrying during long daily travels while foraging. The period of investment in offspring constitutes a high-risk bottleneck period in terms of selection and one on which evolutionary biologists should be focusing rather than avoiding. Indeed, times were soon to change thanks to the increasing influence of Bob Trivers' work (Trivers 1972). None the less, research into behavioral aspects of evolution continued for decades to be hampered and misled by a focus on sexual vs. natural selection and by considering number of mates the key measure of differential reproduction. These misconceptions are only now being redressed, finally bringing studies together into an evolutionary framework.

The strongest biology program in the Chicago area was the one in which Stuart was a faculty member, and I wouldn't consider it both for that reason and because I wanted a return to school to further enhance the complementarity of our skills, not to increase the overlap. Most fortunately, the University of Chicago also has an explicitly interdisciplinary program in Human Development that had modeled in many ways on the one at Harvard in which

Figure 2.4. Individual mother–infant pairs have long been observed in close association with one of a group's adult males in a number of baboon populations. However, the functions of these associations, particularly for males, remained in dispute. Within the past decade, paternity analyses combined with behavioral data, demonstrated that the associated male is often the infant's father and that males in this polygynandrous species play an important supportive role in thir juvenile offspring's social interactions.

I worked with Beatrice Whiting over a decade earlier, one that sought to bring the perspectives of psychology, sociology, anthropology, and biology to bear on an understanding of the human lifecourse. With considerable enthusiasm on the part of several faculty members and puzzlement or skepticism on the part of others about the relevance of my goal to develop a thesis project on baboon ontogeny and parenting, I was admitted and joined the program in 1973. The faculty accepted my in-press sampling paper as meeting the requirement for a preliminary research project, making an exception regarding prior work for which I was grateful. When I graduated a little over five years later, I was also gratified that by then I had won over enough of the wonderfully diverse faculty that they nominated my thesis for the annual Social Science thesis prize, which I received.

Possibly more than for other students, the process of conducting my thesis research was a life-changing one, much more so than one might guess from an apparent continuity: I studied behavioral ecology of baboons before and continued to study them afterwards. It was in these years, though, that in many ways I found my niche and my 'voice', that I developed my focus on demography, on life histories in both the biological and the social science meanings of that phrase, on the integration of ecological context, family structure, and individual lives. It was during these years that I first traveled and spent time in the field without the family, albeit briefly. And though I was always respected intellectually by Stuart, I was understandably just

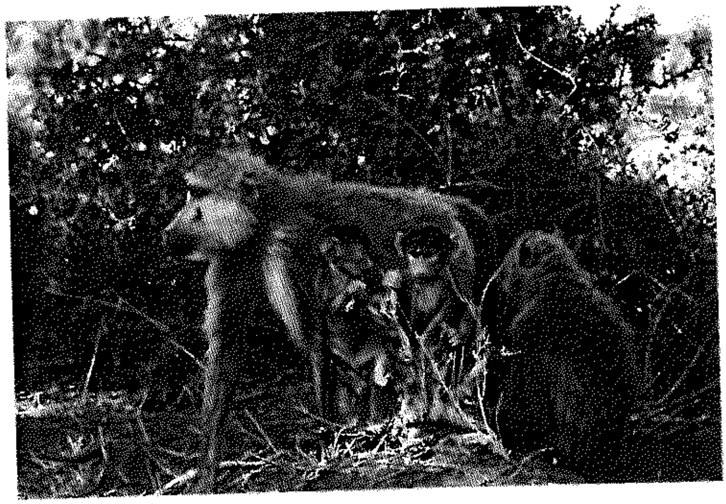

Figure 2.5. Although infants are primarily dependent on their mothers for nutrition, protection, and transport for most of a year, the pair is from the onset deeply imbedded in a highly social milieu.

starting to be recognized by others as a scientist, including being elected as editor from the American side of the journal *Animal Behaviour*. Like many students, I had trouble putting closure on my thesis; fortunately, taking over the journal editorship and my son Michael's upcoming high school graduation in 1979 provided two great incentives to finish. I wanted to complete my degree before Michael's graduation. In addition, I took over the journal at a stage when it would require a huge amount of effort to get out of a tough place. Editing the journal was another whole education in itself, in both science and sociology of science. It was easily the most time-consuming and rewarding professional service I've performed.

My study on the ecology of motherhood focused on the allocation challenges faced by simultaneous investment in survival, maintenance, and reproduction, particularly for non-seasonal breeders with high offspring investment in slowly maturing young. My research reinforced my original idea that studying mothers and infants was not the 'cute', soft female topic that it was often considered but rather the situation of particularly great opportunity for selection, especially so in challenging environments such as the arid African savannahs. I was intrigued by the extent to which the social environments of mothers benefitted or hindered their ability to succeed in negotiating this perilous period. Equally striking was the degree to which both parent and offspring were each dependent on the other for success. Although the interests of parent and offspring were not identical (Trivers 1972), their ability to find creative, mutually beneficial solutions to potential conflicts of interest seemed at the core of success in the primate lineage, perhaps a major factor in evolution of human ontogeny and sociality.

I faced a dilemma as I thought about publication of my thesis. I knew that each aspect of my study would attract more readership and a broader audience, especially from those not studying primates, if it was developed as one or more separate technical papers, and I felt that each was a broadly important topic. Yet, the intertwining of one with another, the contingencies between ecological and social context for example, cried out for an integrated approach, one that did not treat each aspect as an independent, neatly discrete box, lined up in a string of separate topical papers. I tried to do the separate papers but the farther along I went, the more I felt the pull for the integrated whole just too compelling. I've sometimes regretted that I didn't find a way to do both, perhaps in rapid succession, and if I were a speedier writer or more efficient or with a more exclusive research life, or if I hadn't been trying to maintain the long-term field project, perhaps I could have, but that was not to be. Life for scientists as well as our study subjects is often one of tradeoffs and imperfect decisions. In any case, the decision was made and the thesis was written as a book and quickly published under the title *Baboon Mothers and Infants* (J. Altmann 1980) (the publisher rejected 'ecology' in the title and wanted the addition of 'baboon'); it was broadly reviewed and well received. Although I had a baby before my BA, I did (barely) manage to finish my PhD before that 'baby' graduated from high school and before the second one entered.

Once again, I was at a crossroads. Editing *Animal Behaviour* provided a great challenge, especially so in an era of pre-electronic communication, but no salary, and I also needed to seek funding for the baboon research. Stuart, Glenn, and I had co-directed the Amboseli Baboon Project for the first decade as the Project gradually became one with long-term goals. Stuart's focus was primarily on feeding and foraging, Glenn's on dominance and reproduction in males, mine on demography, ontogeny, and female life-histories. We each had several other topics of interest as well as a number of shared ones. Most years, our family spent at least part of the year in Amboseli, as did Glenn, and each year one or two PhD students or post-doctoral fellows spent a year on individual short-term specific studies while maintaining demographic, reproductive, and agonistic data. After the first few years, we gradually introduced a growing set of common procedures and data sets that these participants were expected to collect in exchange for being able to draw on data from previous years and focus most of their time on specific independent projects. In 1982, the resultant Monitoring Guide was announced in society newsletters and made available to interested researchers on request, and in recent years the current version, authored by myself and Susan Alberts, has been available for downloading on our Amboseli Baboon Project website, www.princeton.edu/~baboon.

However, by the early 1980s, Glenn wanted to be independent of Baboon Project commitments, and Stuart wanted to focus on his book about infant feeding and nutrition rather than devote time to the fieldwork or data collection or long-term aspects of the Project maintenance or database management. My research questions and interests, on the other hand, had been and were increasingly longitudinal, lifespan, and intergenerational in nature.

When I completed my degree in Human Development, I could have tipped toward studies of either humans or baboons, but in the absence of funded local academic opportunities in human development and with the likelihood of more problems than advantages entailed by relocation for a faculty position, I leaned toward staying with the Amboseli baboons. I was already deeply invested in what had become one of the first, and is now one of the most

Figure 2.6. Amboseli observers endeavor to stay only as close to their subjects as needed for particular data collection, no closer than the most shy member of the group will tolerate. Here, Raphael Mututua successfully makes himself the most boring primate in the animals' world.

intensive and extensive, long-term field studies of any large mammal. My thesis research and initial demographic and developmental analyses only whetted my appetite for answering lifespan and intergenerational questions about the relationship of individual and group behavior to survival and reproduction. On another level, I realized that Mt Kilimanjaro, the African savannahs, and 'a feeling for the organism', had become too much a part of my being to leave them. Fortunately, Stuart and I had spent our adult lives living on one income, his, with my very modest contribution rarely covering more than flights to Kenya for the children, so we were great at economizing. We were satisfied continuing to manage that way, and as Michael entered university, we were aided greatly by the University of Chicago's generous offspring college tuition plan – Chicago and baboons it would be if I could obtain the research funding. The next year, I was very fortunate to receive a multi-year research grant, one that provided both research funds and some salary to follow up on findings and questions raised in *Baboon Mothers and Infants*; I was thrilled.

In addition to the early 1980s bringing a transition to sole responsibility for the baboon fieldwork and the growing data sets, it also marked a transition in primary field personnel that has now persisted for over two decades and has provided the field base for the next generation of our fieldwork. This transition involved intensive training of and participation by Kenyans at all levels and primary focus on multi-year personnel and projects. After a few false starts with other local assistants, in 1981 we began training Raphael Mututua, a recent O-level graduate, who 28 years later is a marvelous Project Manager as well as data gatherer. In 1989, Raphael was joined by Serah Sayiallel and in 1995 by Kinyua Warutere, the three constituting a 'dream team' with a continuity across years that has been essential to all our

Figure 2.7. Raphael Mututua, Serah Sayiallel, and Kinyua Warutere, the Amboseli field 'dream team'. Each follows a different study group most days. However, for several days at the end of each month, they spend a few hours together with each group (five groups since 1999 for example) to reinforce inter-observer agreement and to solve any emerging problems.

Amboseli research projects, but especially our ability to develop life-history studies of males that are now becoming as rich as those we had been conducting on females, especially rare in a male-dispersing species. The new efforts to study male life histories were also made possible by two American assistants, first Amy Samuels throughout the early and mid-1980s. By the mid 1980s, I also was able to attract the first of our Kenyan graduate students, Philip Muruthi, who received his MSc at the University of Nairobi, joined Princeton University for his PhD, and then returned to do conservation work in Kenya. Philip remains a valued colleague and a mentor to many young African wildlife scientists.

The 1980s were exciting times for the Amboseli project despite a number of significant threats to the fieldwork that ultimately were resolved. Even a single decade of continuous data on identifiable individuals was revealing insights into life-history plasticity and its sensitivity to both social and ecological environments, themes that became increasingly major ones in my thinking. The baboons also increasingly found themselves in a changing environment; as individuals and group, they exhibited interesting behavioral responses to those changes. In addition, partly in response to reviewer concerns, we made the demanding – and expensive – expansion of our studies from a single social group to multiple ones, starting by adding Hook's Group to intensive monitoring. This enabled us to study variability over space, foraging, and demographic contexts as well as over time.

Despite the early emphasis of primate field studies on feeding, fighting, and mating, by the late 1980s much more had been revealed about individual differences among females throughout their lives than about males. As with males of most species and with

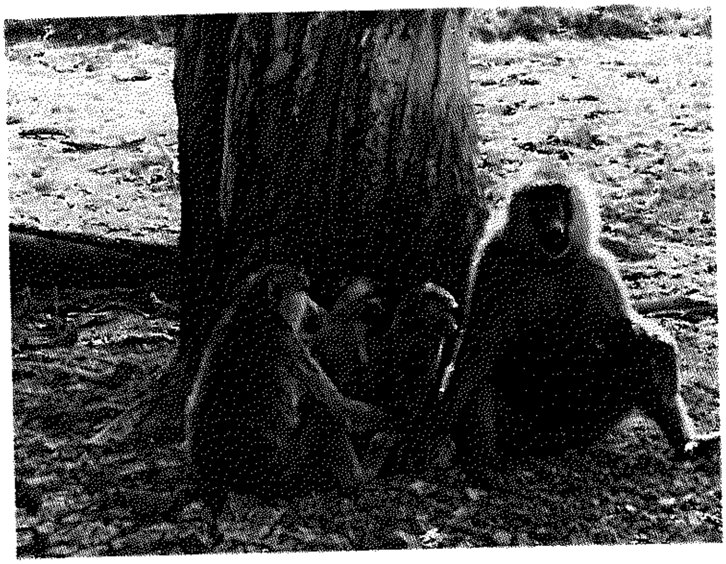

Figure 2.8. Baboon males not only exhibit natal dispersal at about 7-8 years when they become fully adult; repeated, secondary dispersal is also common during adulthood. This poses a special challenge for them in the need to establish new social affiliative and agonistic relationships.

the dispersing sex of all species, theory and data remained basically cross-sectional, snapshots in time, rather than representing lifetime differences and age-specific data based on individuals over the life-course. This is a glaring gap to a great extent because individual differences in male life histories and reproductive success are much less stable across the lifespan than are those of females and, therefore, cannot be estimated well by short-term data. For boom-and-bust life courses such as those that characterize males of many species, including baboons, short-term studies overestimate male variance in fitness and overestimate the impact of factors such as dominance and number of mates obtained. The practical challenges of redressing this gap are great, however, especially in species with slow life histories and male dispersal. Our expansion to multiple study groups and the permanent trained presence of local assistants facilitated being able to monitor males for much more of their adulthood. This set the stage for considering in-depth studies of male life histories, exactly the topic on which a new PhD student wanted to focus.

In 1984, I joined the new Conservation Biology Department at Brookfield Zoo, where I was fully salaried for the first time. I also received modest but crucial field support for Amboseli, particularly for training Kenyan students. Shortly thereafter, I was offered a faculty position in Ecology and Evolution at the University of Chicago, so I arranged a split position between the Zoo and the University. Susan Alberts, who had spent a post-college year with me in Amboseli on a Watson Fellowship, was completing an MSc at UCLA, and was interested in tackling the 'male problem' for her PhD research. The topic, the time, and the person were right. Susan entered the Chicago PhD program soon thereafter. She proceeded to conduct a beautiful behavioral study of male maturation, dispersal, and mating, using both her own detailed several years of field data and our now 15-year longitudinal data set. Of even more long-term significance, Susan became committed to finding a way to obtain paternity information to combine with the male behavioral data, and we both began exploring a way to do that. At the same time, I was increasingly interested in elucidating the relationship between physiology and behavior without disturbance. Again, serendipity was on our side, and just as Susan was writing up her thesis, we established collaborations for physiology with Robert Sapolsky and for paternity determination with Bob Lacey at Brookfield Zoo, and Bob Wayne at the Zoological Society of London. The result was a launching in 1989 of morphological, physiological, and genetic sampling with Robert using the least invasive immobilization technique (blowpipe darting) that was available at the time. These collaborations produced a number of novel papers throughout the 1990s, including ones relating social factors to glucocorticoids, morphological comparisons of 'lodge' and wild-feeding baboons, and confirmation that at least under some conditions mating behavior distributions predict paternity distributions (see bibliography at www.princeton.edu/~baboon). These projects and testing of some longstanding hypotheses were made possible by a combination of our long-term data and methodological developments in field and laboratory.

These initial investigations provided compelling motivation to continue studies of hormones and behavior and of molecular ecology, as we had just scratched the surface. To answer the next set of questions we were interested in would require genetic samples from individuals whom we did not dart: younger individuals, more shy new immigrant males, and females throughout pregnancy. In terms of physiology, we also wanted to answer questions about more integrated, chronic hormone levels, which would require fully non-invasive sampling, such as urinary or fecal sampling. Fortunately, a few research groups around the world were starting to validate fecal steroid methods in primates, and a few others were starting to develop methods to use fecal sources of DNA for parentage studies. Meanwhile, Susan began her post-doctoral research in molecular population genetics to test a range of questions in behavioral ecology including ones for which the integration of intensive life-history and behavioral data with known parentage were the essential ingredients, and to pursue other questions in molecular ecology that extended use of genetic data beyond parentage. Our collaboration, rather than ending with her thesis in 1992, intensified, with both of us working on aspects of behavior, survival, and reproduction, and with each of us eventually developing a complementary laboratory component.

By the late 1990s, Michael and Rachel had established their own families and careers elsewhere, Stuart had transitioned to emeritus status, I accepted a faculty position at Princeton, and we made our first move in almost three decades. I felt very fortunate. I will probably always miss Chicago, some special colleagues there, and the University-wide and interinstitutional Committee on Evolutionary Biology that I had chaired for seven years. At the same time, I love where I am; what more can one ask for? In Princeton I established a fecal steroid hormone lab; Susan joined the faculty at Duke University, where she established a genetic lab. With 'startup' funds from both our universities followed by five-year support for multidisciplinary studies at the Directorate Level at the National Science Foundation, we were able to take this multipronged approach, something that is not possible through NSF individual program units that fund behavioral ecology.

About the same time, we started implementing a long-term plan, first for equal partnership and then for Susan's assuming the leadership in Amboseli. Long-term longitudinal field studies are in themselves rare, but I found none with a continuity and transition plan, yet the need, the obligation, felt compelling. In the decade since then, at least a few others have been developed or are under consideration, some explicitly stimulated by our effort.

This first decade of the twenty-first century has been at least as exciting as any previous one. I find it interesting and very satisfying that I am no longer asked when 'baboons will be done', such a frequent question in the first decades of the baboon project. If anything, the research potential of the Amboseli baboon population continues to increase rather than to decrease or even reach a plateau. This is partly due to capitalizing on emerging techniques and to emerging environmental challenges the baboons encounter. However, it is also due to the increasing value of the data that originally motivated the long-term project: systematically collected life-history, demographic data on known individuals throughout their lifespan, within the context of variable social and ecological environments. With cumulative years, we have learned and continue to identify what patterns and aspects of lives are relatively stable and exhibit little variance across time and space, even across individuals in some cases, and conversely what aspects are highly age-dependent, contingent on ecological and social factors, and now also genetic ones. Intensive and extensive research on the Amboseli baboon population has also enabled us to begin to identify physiological, behavioral, and life-history tradeoffs, conditions under which those tradeoffs occur, and the extent to which some individuals are relatively unconstrained in their options whereas others are much more constrained. In addition, we can now investigate patterns and variability late in life, during aging, at the opposite end of the lifespan where my studies started almost four decades ago. Another reason for increasing value and excitement is our ability to 'get under the skin' now, to answer some questions about physiology and genetics and to do so primarily while retaining hands-off methodologies.

No one biological system or population is ideal for all questions in behavioral ecology, nor does one provide findings that will hold over all species or even over space and time for that species. But I'm amazed and gratified by what the baboons have enabled us to do to answer a range of fundamental, exciting questions and by their potential to do so into the future. I've felt fortunate, and continue to be amazed at how much fun one can have planning research, observing behavior, analyzing data, and emerging with confirmation of ideas, or having them blown out of the water, or something complex in between!

The themes of survival, reproduction, the importance of ecological and social context, maternal effects, ontogeny, demography, and the relationship of relatedness and social structure to demographic structure, all were topics of study in the first decade of the baboon project. All were ones for which as many questions remained as were answered. Even as some completely new themes have been developed as well, most first publications became one end of a thread that has continued into later decades, often to the present, and that developed more complex colors and textures, ones increasingly interwoven with each other that continued or re-emerged in exciting directions with the help of new methodologies, diverse collaborations, and changing developments in the fields of ecology, evolution, and behavior (see the Amboseli Baboon Research Project website, www.princeton.edu/~baboon).

Why invest in a long-term study, one that spans multiple generations, several decades? Why stay with a single species, a single population, especially one with a slow life history? Even I have periodically asked myself that question through the years, especially when renewal of local research permits or grant funding has been at risk, but also when the weight of managing a full-time project and staff halfway around the world, plus the US components, becomes overwhelming. Yet I know at the core that not just my life but our understanding of ecology, evolution, and behavior would be much more limited if all rigorous research were conducted within a narrow range of perhaps easier possibilities.

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