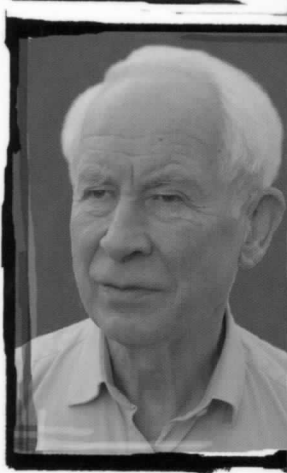


Leaders in **Animal Behavior**

The Second Generation



Edited by

Lee Drickamer and
Donald Dewsbury

CAMBRIDGE

LEADERS IN ANIMAL BEHAVIOR

The Second Generation

Edited by

Lee C. Drickamer

Northern Arizona University

Donald A. Dewsbury

University of Florida

Preface

Introduction

Richard D. Vetter

Multi-channel, multi-modal, and multi-scale communication in the social behavior of the honeybee

and perspective

Richard D. Vetter

Learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury

How do we know what we know? The role of the hippocampus in learning and memory

Frederick C. Dewsbury



CAMBRIDGE
UNIVERSITY PRESS

2010

Myths, monkeys, and motherhood: a compromising life

SARAH BLAFFER HRDY



Definition of an Anthropologist: “(Someone) who studies human nature
in all its diversity.”
Carmelo Lisón-Tolosana (1966)

Maternal effects (1946–64)

From a young age, I was interested in *why* humans do what they do. With little exposure to science, certainly no inkling that there might be people in the world who studied other animals in order to better understand our species, I decided to become a novelist. Born in

Texas in 1946, right at the start of the postwar baby boom, I was the third of five children – four daughters and finally the long-awaited son. My father's father, R. L. Blaffer, had come to Texas from Hamburg via New Orleans in 1901 at the time oil was discovered at Spindletop. He recognized that fortunes would be made in the oil business. He married Sarah Campbell from Lampasas, whose father was in that business. I was named for her, Sarah Campbell Blaffer II. My mother's father's ancestors, the Hardins, French Huguenots from Tennessee, arrived earlier, in 1825, before Texas was even a state. They settled in East Texas in what later became Hardin and Liberty Counties.

I know more about the Hardins than I otherwise might because after my father died, my mother wrote a book, *Seven Pines*, based on old family letters. Like her mother before her, my mother was a compulsive scholar and stickler for accuracy. My grandmother Davis was a woman of tremendous determination, one of the first women from Texas to attend Wellesley College. She had a passionate love of literature, and after college ran a bookstore in Dallas while waiting for my grandfather Davis, a Yale graduate and a bank president, to marry her. After marriage, with the same determination, she assumed the role of strait-laced *grande dame*, but never lost her love of books. When widowed, she went to graduate school in English, and later to Paris to learn bookbinding.

When my own mother died, I inherited part of grandmother Davis' extraordinary library, including the bound copy of her 1944 Master's thesis about the poet and actress Adah Isaacs Menken, a self-made woman if ever there was one. Menken made up just about everything, not just her poetry, but also her parentage, date of birth, and the authorship of her poetry, since some verses were plagiarized. My great-grandfather's mother had met Menken when she passed through East Texas with Victor Franconi's Hippodrome in 1850. This "liberated-to-the-point-of-scandalous" woman made an impression so deep that my grandmother, and later my mother, were still talking about her when I was growing up. Years later when I decided to study anthropology, when I first went to India, it was my mother who funded the work, and later, when against family opposition I married a fellow anthropologist, my mother and maternal grandmother stood up for me.

In *Mother Nature: A history of mothers, infants and natural selection* I acknowledged my debt to these remarkable women. "There is an old saying that *sons branch out, but one woman leads to another*," I wrote.

Perhaps its author was aware of sex-specific parental effects. In my case, this book owes its existence to my mother, Camilla Davis Blaffer Trammell, and to her mother, Kate Wilson Davis, for passing on to me their dogged temperaments (probably genetic) combined with a love of learning (more likely a maternal effect). Both women were closet bluestockings ... [although like] other women of their class and time they were determined to 'marry well.' How else to achieve an acceptable social status? Alternative options in those days were not obvious. Yet these women imparted to me their love of books and ideas, and stood up to support an iconoclastic kinswoman in her defection from tribal customs. ...

Born in Dallas, I grew up in Houston when it was still a fairly sleepy city with graceful oaks, long lazy gar and alligators swimming in the bayous, and cattle grazing along Buffalo

Speedway. Prevailing values were distinctly "Southern", generating genteel manners, extreme segregation, and patriarchal institutions. It was no accident that I would later become interested in the evolutionary and historical origins of patrilocal marriage, male-biased inheritance, female sexuality and peoples' obsessive concerns with controlling it. Elsewhere the women's movement may have been getting under way, but no one I knew talked about it.

Reared by a succession of nannies, I was a case study in "insecure attachment" and, except with friends, quite shy. I was simultaneously bookish, rambunctious, and imaginative. I dreaded school and was inattentive, doodling and daydreaming through classes. I loved horses though, devouring Walter Farley novels, and in ninth grade worked my way through Moyra Williams' *Horse Psychology*. Together with Dusty, a hunter of undistinguished conformation but with a tidy way of folding his front legs when he jumped, we traveled to horseshows around Texas and as far away as Tennessee. When I was fifteen my mother arranged for me to go to a school in Maryland known for its riding program.

Fortunately, St. Timothy's was something more. Back home the term "bluestocking" was a pejorative, but this small, girls-only school took women's education seriously. Miss Watkins, the headmistress, was sensible and humane. Given my shaky academic record, I was promptly assigned to the remedial quarter of my class. We called ourselves "the dupes." Foreseeing trouble, Miss Watkins also assigned herself as my advisor. During monthly meetings I did my best to reveal as little as possible, but Miss Watkins somehow knew it all. She expressed unwavering confidence in my abilities.

For all its good points, like most girls' schools, St Timothy's did not offer much science. However there was Mrs. Cross' biology course in which I was promptly nicknamed by my classmates "queen of the Bio dupes." Too lacking in either worldliness or self-awareness to view doing well in school as a route to anything, I was motivated purely by a lust to learn. This included a passion for *Scientific American* (at that time a magazine even a neophyte could enjoy), an avocation with important consequences later on. Academic prizes and medals came as sur-prizes, as if by some odd happenstance. It was like being nearly six foot (which by then I was) and not realizing that other people considered me tall. I honestly had no idea I was becoming a scholar. Years later, groping to explain how feminist ideas began percolating into my writing, I compared myself to "some savage on the fringe of civilization" dimly and awkwardly rediscovering the wheel (Hrady 1986: 151). It was like that.

"The Savage Mind" (1964-69)

In 1964, I enrolled in Wellesley College where my mother and grandmother had gone. My favorite courses were creative writing, Mary Lefkowitz's Greek mythology, and a course in geology from the novelist Erskine Caldwell's son because the language used to describe the deep history of the earth struck me as beautiful. At the end of my sophomore year, I transferred to Radcliffe, the women's part of Harvard. To this day I don't understand how I managed it. As far as my father was concerned, Cambridge was a den of immorality and radicalism. Fortunately I was the third daughter, the heiress to spare, and by then there was a fourth, followed finally by the long-desired son. No one, I suspect, paid much attention.

Tuition bills were simply sent to my father's secretary. The handful of transfer students were supposed to arrive early. Unable to find the off-campus house to which I had been assigned, and not knowing whom to ask, I spent my first night in Cambridge at the Central Square YWCA, where someone swiped the copy of Dostoyevsky's *The Idiot* which had been given me by a friend for luck.

My reason for transferring was simple. I had begun a novel about modern Mexicans of Mayan descent who were torn between their contemporary worlds and ancient heritages. It occurred to me that it would be helpful to actually learn something about Mayans and their mythology. I decided to study under Evon Vogt, world's expert on Mayan cosmology. This meant entering Harvard as a junior, changing my major from English to anthropology, and learning about "structuralism."

Beginning with *Le Cru et le Cuit*, in 1964 (the year I entered college), one by one, volumes in Claude Lévi-Strauss' ambitious and highly speculative Introduction to the Science of Mythology were published. After *The Raw and the Cooked* came *The Origin of Table Manners*, *From Honey to Ashes*, and finally, *Man Naked*. I devoured the work of the structuralists, especially Mary Douglas' elegant 1966 classic, *Purity and Danger*. Prior to that, I had thought of myths as Jungian archetypes or perhaps grist for Freudian mills. Then Professor Vogt (everyone called him "Vogtie"), exposed me to this grand explanatory framework. According to Lévi-Strauss, folktales were the products of human minds attempting to make sense of the animals, plants, seasons, and social relations in the worlds they lived in. Using techniques inspired by linguistics, Lévi-Strauss compiled vast networks of myths from North and South America and then broke these complex narratives into their component parts, seeking recurring patterns and the logic that linked them. He used his version of the comparative method to reveal the "mental adaptations" of those devising the stories. The emerging categories ("living" versus "dead," Natural versus Cultural, etc.) often involved binary oppositions, which Lévi-Strauss considered fundamental to the architecture of human cognition, the structuralists' version of "core knowledge."

When critics pointed out that Lévi-Strauss' interpretation of how "savage minds" worked was tainted by his own Sorbonne-educated French mind, he famously retorted that since he was dealing with universals, it scarcely mattered. It made no difference whether "the thought processes of the South American Indians take shape though the medium of my thought, or whether mine takes place through the medium of theirs." This was 1966. I was hooked.

For an undergraduate interested in Maya-speaking peoples, the Harvard Chiapas Project offered unparalleled opportunities. Every undergraduate or Ph.D. student working on the Maya (and there were dozens of us) deposited copies of our fieldnotes and publications in the Harvard-Chiapas files, a common library open to all, with an avuncular but firm Vogtie riding herd on rampaging egos. It was a model of scholarly collaboration.

My undergraduate honors thesis, published in 1972 as *The Black-Man of Zinacantan: A Central American Legend*, was a structural analysis of folktales about "anomalous" animals, creatures that failed to fit established categories of living or dead, natural or cultural. I was interested in learning how and why human imaginations invented demons. I had at my disposal several hundred folktales collected from Tzotzil-speaking Maya in

Chiapas by Robert Laughlin, now at the Smithsonian, as well as stories from across Central America collected by Vogtie's students, including myself. By that time I was spending summers as a volunteer medical technician on projects in Honduras and Guatemala sponsored by Radcliffe's Education for Action. I knew only a smattering of the various Mayan dialects, but my Spanish was good. Women would come to the clinic early in the morning before it opened and share stories. My day job was teaching hygiene, setting up vaccination clinics, and arranging matters for weekly visits by a visiting dentist or doctor. But my predawn and evening hours (just the times when they were most prone to prowl) were reserved for asking questions about *espantos* (literally "spooks"), learning about the natural history of imaginary creatures, about *characotel*, the man who turns into a dog at night to go off and make mischief, *auitzotl*, the spectral water animal that lurks near river crossings to consume the nails and hair of drunkards, or *x'pakinte*, really tree bark dressed as a woman to deceive men. Most fascinating of all was the creature called *h'ik'al*, a tiny, black, super-sexed winged demon who punishes women careless of their sexuality – women who deviate from normative sex roles. Transgressors would be seized, carried off by *h'ik'al* to his cave, raped with his super-long penis. Impregnated victims would swell up and then give birth, night after night, until they died. No wonder women cowered at the prospect of night-time assignments. People were so terrified of *h'ik'al* that they avoided mentioning his name, for fear he might come.

The Black-man of Zinacantan was a conventional exercise in structural analysis except that work by Maya archeologists and cryptographers allowed for comparisons far back in time. Combined with what ethnographers, ethnobotanists and ethnozoologists were learning, knowledge about ancient Maya belief systems added a new dimension. Transcriptions of codices compiled by early Spanish explorers were also available at Harvard's Tozzer Library and at the University of Texas in Austin, so I was able to trace the origins of the *h'ik'al*, a contemporary chastiser of sexual sins, back to the ancient Maya bat demon *Camazotz*. To test the validity of my interpretations, I generated predictions about how (if my analysis was correct) contemporary Maya subjects should respond to different propositions derived from them. It was the beginning of an abiding interest in "deep history" – though sociobiology's explanatory framework would push the time depth back millions of years, and broaden the comparisons to include other species.

Formative detours

In truth, my metamorphosis from structuralist to sociobiologist was actually more convoluted. After the first semester of senior year, I took time off. My return coincided with Woodstock and Kent State. I graduated as a member of the infamous "Class of 1969." It was my classmates, including friends from Education for Action, who took over University Hall in protest against the Vietnam War. I will never know whether I would have joined them because that fateful day was also my first up after spending two weeks bed-ridden with mononucleosis. Eager to understand what was happening, I went to Harvard Yard. My classmates were already inside the building, but I saw Professor Irven DeVore, a fellow

Texan whom I knew slightly. I had taken his undergraduate course on primate behavior and was dating one of his advisees. It was a beautiful spring day, and when I asked Irv what was happening, he replied, "I'm not sure. But in my day we would have called it a panty raid."

That June I graduated Phi Beta Kappa, receiving my degree *summa cum laude*, but skipped Commencement. None of my family planned to come – which was in fact a relief, since I was not sure how my volatile and conservative father would react to the political turmoil. The main reason, though, was my inability to make up my mind about red arm bands. Some classmates planned to wear them to signal protest against the war. I opposed the war but felt politics and scholarship should be kept separate – a tenet of anthropology in those days, albeit not today. My indecision was symptomatic of inner conflicts I experienced throughout 1968–69.

I had spent the "time-off" traveling first to Chiapas to see for myself a real-life *karnaval* ceremony in which a man would paint himself black and act like a mischievous *h'ik'al*, then to Yucatan and Central America to collect more tales, then on to Kenya, Tanzania, Zambia, South Africa, and Ethiopia. On my return, before returning to college, I worked at a meat processing plant in Watertown. "Jack-Pack", as it was called, specialized in "proportioned meats" for restaurants, ensuring that every diner received the same size portion. I wanted to learn what it was like to make a living. Clearly, I was beginning to deviate from the anticipated trajectory of a former debutante. My growing (still limited) political awareness brought with it unease on various fronts, especially over what was happening in Central America.

Recollecting my naiveté while working among villagers in Honduras and Guatemala, now shamed me. I recognized that the Guatemalan military had been using our services as part of its own public relations campaign. How could I have failed to register why the machine guns on the backs of the jeeps transporting the medical volunteers were there? A moderately accomplished artist, I used to make teaching posters for my hygiene students, big colored demonstrations about parasites and why it was important to wear shoes, boil water, eat a balanced diet. In retrospect, my admonitions to people who could not possibly afford to follow my advice were painful. I loved anthropology, but my travels in Chiapas and Guatemala had convinced me that if I lived among people there I would have to become some sort of revolutionary, which I had neither the desire nor the temperament to do. Better change course while I still could.

It was in this frame of mind that I decided to give away my anthropology books and apply to a graduate program in communications at Stanford University to learn to make educational films for people in developing countries. To prepare, I enrolled in a seminar on television. One partner on the required film project was Al Gore, already way ahead of the rest of us in his understanding of television and its potential.

So far as I was concerned, Stanford's communications program had little to offer. I began auditing Paul Ehrlich's population ecology course. He had just published *The Population Bomb* (1968), and as I listened, I was reminded of John Calhoun's *Scientific American* article about "Population density and social pathology." It had made a vivid impression when I first read it early one Sunday morning before church in an empty dining hall at St Timothy's

School. When kept at high densities, Norway rats experience a “behavioral sink,” exhibiting such pathological-seeming behaviors as maternal neglect, infanticide, and cannibalism (Calhoun 1962). I was also reminded of something I had read in Professor DeVore’s undergraduate course. Primate behavior then was still too young a science to have a textbook, so we used edited collections of field studies, including one by Yukimaru Sugiyama and his colleagues. They had reported infanticide among the high density population of langurs at Dharwar Forest in South India. It occurred to me that these monkeys might provide a model for the behavioral effects of crowding. By switching to nonhuman primates, I could also avoid (or so I thought) ethical issues raised by studying people.

Naive again – on so many counts, I made up my mind to go to India to study infanticide in crowded monkeys. Before the end of the term, I dropped all my courses, even those for which I had completed the work, to make certain that I would have no stake in staying. I applied to graduate school at the University of California-Berkeley, where langurs were already being studied, and to Harvard. Harvard (but not Berkeley) admitted me in the middle of the year. Thus in January 1970 I joined Harvard’s fledgling program in behavioral biology within the Anthropology Department. (At that time, primate behavior, if taught at all, was taught by anthropologists.)

I had no idea how I was going to learn to study monkeys. Serendipitously, that summer the former husband of one of my aunts asked me to join him and two of my favorite cousins on a hunting safari in the Masai Mara and Kenya’s northern frontier district. Although not a hunter, tagging along seemed the opportunity of a lifetime. On the way to Nairobi, I bought my first pair of binoculars at the Frankfurt airport, the battered Leica 10×40s I still use today. When the safari was over, I stayed on. Louis Leakey sent me to the Tigoni Primate Center outside of Nairobi, where a young primatologist named Neil Chalmers was in charge. In this way my uncle’s invitation provided my first chances to see monkeys in the wild and to briefly study them in captivity.

In addition to running the colony and playing housemother to a succession of young women Leakey deposited, Neil was doing research on comparative infant development in African cercopithecine monkeys. I did my best to be useful and in return received as good an introduction to Old World monkeys as would have been possible anywhere in the world at that time. Neil loaned me Pru and John Napier’s *Handbook of Living Primates* and generously oversaw my pilot study of allomaternal behavior (then still called “aunting”) among caged patas monkeys.

Choosing a study site – and a mate

By the time I set out for India in June 1971, I still had no training in field methods beyond the weeks observing monkeys at Tigoni and reading a copy of Jeanne and Stuart Altmann’s just-published *Baboon Ecology*. However, as I fulfilled course requirements, I took every opportunity to work langurs in. I convinced the always amiable Professor William Howells to let me write about colobine taxonomy (in chronic and bewildering flux) in his course on fossil man. But when I turned in a paper on “Infant biting and deserting among langurs” to

the cocky graduate student, Robert Trivers, who was co-teaching the *Evolution of Sex Differences* with Irv DeVore, he acidly pointed out that "this paper has nothing to do with sex." Trivers and I got off to a less-than-promising start. Even so, I had a dawning awareness he might be someone worth learning from. In fact, of course, Trivers would be the most inspirational teacher I ever had. Like a shaman, he dove deep inside himself, resurfacing with extraordinary insights – often at great personal cost, occasionally requiring hospitalization. By the end of my first field season at Mount Abu, I would be ready to set aside the social pathology hypothesis I started with. I was just beginning to understand how important Trivers' stunningly original ideas about the connection between parental investment and Darwinian sexual selection were for understanding infanticide. Let me explain how I got to Abu.

At the end of spring term, financed by my mother, I headed for the forest of Dharwar in south India where Yukimaru Sugiyama had first reported infanticide among langurs. By then a new report of infanticide had been published by S. M. Mohnot (1971), who was studying langur behavior near Jodhpur, in Rajasthan, north India, so Jodhpur became my first stop. Another Harvard anthropologist, Dan Hrdy, met me there. Something else happened that year in Professor Howell's course on fossil man – I fell in love.

Dan had a travel fellowship and planned to spend that summer in Peru. When he decided he could just as well use the fellowship to work in India, I was glad. A year later, we were married in Kathmandu, Nepal, meeting up there as Dan was on his way to the Pacific to join the Harvard Solomon Islands Project and I was on my way back to Abu. The ceremony was held in the garden of the American consul to Nepal, Carleton Coon, son of the anthropologist of that same name. I wore white cotton slacks and the Coon children put plastic monkeys on top of the wedding cake, painting estrous swellings on the female of the pair with red nail polish. Dan and I only narrowly avoided missing the ceremony. The day before we had gone by motorcycle toward the border between Nepal and China to look for langurs. On our return we encountered torrential rains and took shelter in a cave beside the road. A shepherd was already there, urgently trying to tell us something about the location of our motorcycle. Minutes after Dan moved the only possible transportation back to Kathmandu, a flashflood swept across the spot where it had been parked. Later, as we slid and swerved down the steep, muddy road back to the capital, me clinging to Dan's back as he muttered something about "If I had known what a backseat driver you were. ...". When the news reached Irv DeVore back in Cambridge, it evoked one of his more memorable one-liners: "I expect he married her for her vowels."

I fantasized that Dan would become a professor of anthropology somewhere. Together we would lead a life of shared research. But prompted by medical anthropologist Al Damon ("Why don't you go to medical school, young man, and really learn something?") Dan enrolled in the Harvard-MIT joint M.D.-Ph.D. program. Instead of an anthropologist, I had married an infectious disease doctor. We published only one scientific paper together. But over time, as our mutual devotion deepened, maintaining our partnership became one of my life's main goals, requiring compromises I did not then anticipate. Thirty-six years into the

enterprise, I can only second what I wrote in the dedication to *Mother Nature*: selecting Dan was “the wisest choice this female ever made.”

So, headed for Dharwar, how did we end up at Abu? Return with me to Jodhpur, a bustling North Indian city set in the middle of the Great Indian desert, unlikely habitat for leaf-eating colobine monkeys. A population of around 1,000 langurs survives there, thanks to 13 committees composed of devout Hindus who supplement their diets with fresh produce on a daily basis. The designation “Hanuman” derives from the monkey who helped Lord Rama in *The Ramayana*, a Hindu sacred text, while “langur” comes from the Sanskrit *langulin*, “having a long tail”. At Jodhpur, S. M. Mohnot and his mentor, Professor M. L. Roonwal, a towering figure in Indian zoology, greeted us warmly.

Early in the morning (by noon it would be 120 degrees), S. M. took me to the outskirts of Jodhpur. After scouring the rocky crags, I encountered my first langur, a female about the size of a springer spaniel with the slender-waisted elegance of a greyhound, an extraordinarily elegant silver-grey creature with a black face and dainty black gloves, inexplicably separated from her troop, making her way back to them as I scrambled behind. It was S. M. who advised me to go to Mount Abu. He promised that langurs at Abu would be no less crowded than at Dharwar, and at 4,600 feet, Abu would be healthier. We headed for Mount Abu to check it out.

The langurs of Abu (1971–1980)

Even beyond the sheer beauty of this town atop the Aravalli hills, Abu had much to recommend it. Langurs there were spread along a gradient from town-dwelling groups already habituated to humans, to wild groups on the forested hillsides. While Dan surveyed langur populations at Dharwar and elsewhere, I remained at Abu to map home ranges and learn to identify individuals. Because time was short, I focused on groups near town, including an unusually small group with a single adult male accompanying six females. There were four females with infants, a very distinctive female missing part of her forearm accompanied by twins, and a very old-looking, childless and peripherized female that I named “Sol” (Hrady 1974, 1977). Although I did not know it at the time, its small size and location made this group particularly prone to male takeovers. It would be the following year before I actually witnessed an adult male repeatedly stalk, attack, and wound infants in this troop, but observations that first summer were already leading me to reassess my starting hypothesis.

The langurs at Abu lived at relatively high density, in close proximity to humans, yet intra-troop relations were calm, just as Phyllis Jay had reported in her pioneering studies of the “peaceful” langurs she watched at northern Indian sites at Orcha and Kaukori. She had been aware of observations of fighting among langur males recorded by nineteenth century and early twentieth century naturalists, but dismissed them as “anecdotal, often bizarre, certainly not typical behavior” (Jay 1963: 8). At Abu, males were tolerant if aloof, and extremely protective of infants in their troop. Inter-troop encounters were tense affairs with mostly ritualized aggression. Even though males became agitated by the approach of

all-male bands, nothing suggested "pathological" aggression. Yet in August of that year, a month after the monsoon began, the resident male in the Bazaar troop was replaced by a new and very distinctive looking male with a chunk missing from his left ear. All six infants were suddenly gone. Two people living in Hillside Troop's range independently told me that they had each seen something inexplicable, a monkey killing an infant. It dawned on me that infanticide might be more widespread and normal than I had assumed. Infanticide was not just occurring at Dharwar but also at Jodhpur and probably Mt. Abu. Fall term was about to begin, but I knew I had to return.

Becoming a sociobiologist

Spring of 1972, Ed Wilson and Irv DeVore co-taught a seminar posing the question: could there be a science of sociobiology? Wilson had just completed *The Insect Societies*. A great visionary of boundless optimism, Wilson was preparing to lay out his ambitious blueprint for integrating ecology, demography, genetics, development, behavior, and evolutionary theory in one grand explanatory framework, *Sociobiology: The new synthesis* (1975). His sense of mission was infectious. These were heady times to be anywhere near the Life Sciences at Harvard. In that seminar, I also made several lifelong friends. Peter Rodman, just back from fieldwork in Borneo, was using the seminar to write up his data on male and female foraging strategies among wild orang utans, while Martha McClintock, still primarily interested in the effects of pheromones on reproduction, was taking the opportunity to survey the (mostly rodent) literature on how latitude affects reproduction. My paper explored how "Hamilton's Rule" could help explain the evolution of allomaternal care in primates.

Still a prophet unrecognized in his own country, British evolutionary theorist W. D. Hamilton's ideas were being reverently explicated by just-minted Ph.D. Robert Trivers. Between 1971 and 1974, Hamilton's Harvard "bull-dog" was in the throes of producing his own classic trilogy on reciprocal altruism, parental investment, and parent-offspring conflict, articles that would transform the way I (and many others) thought about the evolution of social relationships.

My seminar paper was titled "The care and exploitation of Infants by conspecifics other than the mother." It was about costs and benefits of shared care from the perspectives of the various parties concerned: mothers, infants, and allomothers. By semester's end, I had not finished, but Wilson urged me on. When completed he submitted the manuscript on my behalf to Robert Hinde at *Advances in the Study of Behavior*. Written in 1972, this was my first scientific paper, although a delay in publication meant it did not appear until 1976. The acknowledgement read: "Without the advice and encouragement of Professor E. O. Wilson, I never could have completed this paper. Without the input of Dr. R. L. Trivers, it would not have been worth writing; in his writings and private discussions he has exposed me to a theoretical construct that I believe begins to make sense of the problems with which anthropologists must deal." By 1972 then, I already felt a profound debt to Trivers and Wilson and considered myself a sociobiologist.

The infanticide controversy (1974 to the present)

I returned to Mount Abu as soon as term ended. I would do so nine times between 1971 and 1980. Findings from 1,500 hours of observations from the first five field seasons were summarized in *The Langurs of Abu: Female and male strategies of reproduction* (1977). In chapter 8, "Infanticidal males and female counter-strategists," I explained how – far from being pathological – infant-killings were the outcome of goal-directed male behavior. After invading a troop from outside, a langur male would target unweaned infants and relentlessly stalk them in a process that sometimes continued over days. Male attacks on infants were not seen at other times, and a strange infant kidnapped from another group would not be attacked, so long as it was carried by a familiar female – that is, one with whom the male had mated. Although the tenure of resident males was highly variable, the average was about 27.5 months. I hypothesized that by eliminating his competitor's offspring, an usurping male enhanced his own opportunities to breed since females who lost infants resumed cycling sooner than if they had continued to lactate.

In 1974 I had proposed that infanticide at Abu could be explained as a variant of classic Darwinian sexual selection. By canceling the female's last mate choice, the new male reduced the reproductive success of a rival while improving his own chances to mate. The hypothesis generated very specific predictions. Following Trivers (1972), attackers should belong to the sex investing least in offspring (in this case, male). Victims should be unrelated, and also unweaned, with the effect of compressing a female's fertility into the limited period when the killer had access to her. Not only were my observations of attacks on infants consistent with these predictions, but new observations from other species were conforming as well. By early 1977, even before the book appeared, I was sufficiently confident that my hypothesis applied more broadly to publish an article in *American Scientist* entitled "Infanticide as a primate reproductive strategy." I proposed that infanticide by males was a highly conserved behavioral trait widespread in the subfamily Colobinae, but also cropping up (perhaps through convergent evolution?) throughout the Primate Order in prosimians, Old and New World monkeys, and great apes. The ensuing controversy caught me by surprise.

At the time I embarked on my research on langurs, primatologists (who, remember, initially came mostly from the social rather than the biological sciences) were profoundly influenced by social theorists like Durkheim and Radcliffe-Brown. Primate social organization was assumed to be a "functionally integrated structure" in which each individual had a role to play in the life of the group and all group members functioned together to ensure the group's survival. Thus early reports of infanticide by male langurs had been dismissed as "dysgenic." My proposal in the January–February 1977 issue of the *American Scientist* provoked a series of rebuttals, beginning in the May–June issue. They began when Phyllis (Jay) Dolhinow, the first Western primatologist to study langurs in the wild, wrote that "It comes as a great surprise that infanticide might be considered a normal adaptive evolutionary strategy..." because "Normal" langurs "do not kill infants." Because it "shows destruction not adaptation" the behavior I described had to be abnormal. Furthermore,

Dolhinow asserted, "Incredible powers of memory and reason are attributed to the langur monkey (how else could a male recognize paternity and recall events that occurred six months or more in the past?)" (1977: 266). These were early salvos in a long-running debate, which persisted long after the sexual selection hypothesis and other adaptive explanations for infanticide were accepted by biologists. Within anthropology, they continue to this day.

Looking back, I divide the saga into two phases. The first phase began with exchanges involving Dolhinow, her mentor Sherwood Washburn, and their students from Berkeley, followed by critical articles by other social scientists like political scientist Glendon Schubert (1982) and the eminent physical anthropologist Christian Vogel of Göttingen University, who in 1982 published "Der Hanuman-Langur (*Presbytis entellus*),¹ ein Parade-Exempel für die theoretischen Konzepte der 'Soziobiologie'?" These critiques occurred at an early stage in the study of this phenomenon, and partly grew out of the ongoing paradigm shift from selection at the level of groups to selection on individuals. In my opinion these were useful, ultimately constructive, debates.

On both sides, everyone agreed that we needed more and better data. Over the course of the controversy I learned to be more self-critical about assumptions. I still remember sitting down to correct by hand each reprint from my 1974 paper before mailing them out. In Table VI, where I summarized available data on "Political changes and infanticide at Dharwar, Johdpur and Mt. Abu" I added to the caption of the last column "Infants Killed or Missing." Although I had counted any infant attacked by a male who subsequently disappeared as killed by *that* male, this was only a probability, not a fact.

The criticisms also made me think harder about my main underlying premise. Though only expressed under specific circumstances, I assumed that infanticidal responses such as infant-biting were heritable traits. To this day, however, there is no definitive evidence of a genetic basis for this behavior in primates, although by the time I wrote the Preface to the new paperback edition of the *Langurs of Abu* (1980), evidence for heritability was emerging for rodents and in time grew stronger (Parmigiani and vom Saal 1994).

The controversy also pressured me to think more about "human disturbance." In collaboration with Jim Moore and two Berkeley-trained primatologists, Naomi Bishop (one of Dolhinow's students who had studied langurs at very low densities high in the Himalayas) and Jane Teas, we devised "Measures of human disturbance in the habitats of South Asian monkeys" (Bishop *et al.* 1981), published the same year that Oxford's Paul Newton reported infanticide among langurs in a disturbance-free North Indian tiger sanctuary. Finally, the controversy made me think much more critically about whether an infanticidal heritage among monkeys had *anything* to do with infanticide in our own species.

Whereas in nonhuman primates infanticide typically involves unrelated males (or occasionally, as in chimps or marmosets, rival females), human infanticide most often involves the closest of relatives, an infant's own mother. Yet with very few exceptions, maternal infanticide does not occur in wild monkeys and apes. In 1979 I devised a classification of

¹ *Presbytis entellus* was the nomenclature in use at the time, though taxonomists have now returned to the older name, *Semnopithecus entellus*.

infanticide according to different explanatory hypotheses, each generating its own set of (testable) predictions regarding the age of the victim; the age, sex and degree of relatedness of the infant to the killer; nature of gain (if any) to the killer, and so forth. The five main classes were: *Sexual Selection*; *Exploitation of the Infant as a Resource*; *Competition for Resources*; *Parental Manipulation*; and *Social Pathology*. Cases of infanticide in humans are far more likely to fit predictions from "Parental Manipulation" or "Resource Competition" than "Sexual Selection" even though, as Margo Wilson and Martin Daly would soon conclusively demonstrate, infants with unrelated males in the household were at greater risk. It was not until 1999, in *Mother Nature*, where I reviewed anthropological and historical evidence on maternal retrenchment, abandonment, and infanticide in humans, that I felt able to discuss how infant-killing by unrelated men fit in. Even then I tread cautiously and was tentative (in Chapter 10). My point here is that by and large these early exchanges were part-and-parcel of healthy scientific debate. They made me more cautious.

Without my realizing it, the second, far less constructive, phase of the debate began with the publication of *Infanticide: Comparative and Evolutionary Perspectives* (Hausfater & Hrdy 1984). The volume grew out of the First International Conference on Infanticide in Animals and Man, funded by the Wenner-Gren Foundation and held at Cornell University, August 16–22, 1982. The primate section consisted of chapters by primatologists from zoology as well as anthropology, including Hausfater himself (an Altmann student), Carolyn Crockett, Lysa Leland, Tom Struhsaker, Anthony Collins, Jane Goodall, Dian Fossey, and Yukimaru Sugiyama. Dolhinow declined, but her student Jane Boggess provided a detailed critique of the sexual selection hypothesis. Zoologists Craig Packer and Anne Pusey summarized evidence from lions and other carnivores. Ornithologist Doug Mock discussed siblicide in birds, and there were also chapters on exploitation of infants as a resource (mostly through cannibalism) in fish and invertebrates. Questions of proximate causation were discussed in chapters on controlled experiments with rodents by Fred Vom Saal, Bob Elwood, Bruce Svare and Craig Kinsley, Jay Labov, and others. The final section contained overviews by demographers and historians, as well a sociobiological overview on human infanticide by Daly and Wilson. I held up publication of the volume to include Bugos and McCarthy's extraordinary case study of maternal infanticide among the Ayoreo of Paraguay. It provided the first empirical demonstration that probability of neonaticide declines with maternal age and reproductive value. In retrospect, it is fortunate we included it, as shortly afterwards anthropologists began refusing to sanction publication of data on infanticide in traditional societies (discussed in Hrdy 1999: 293–7).

My 1984 Preface reveals how convinced I was that the volume would end the controversy. "Over the past decade," I wrote,

the intellectual pendulum... has swung from an earlier view that infanticide could not possibly represent anything other than abnormal and maladaptive behavior to the current view that in many populations infanticide is a normal and individually adaptive activity. ... Quite possibly, readers ten years from now may take for granted the occurrence of infanticide in various animal species and may even be unaware of the controversies. ..." (p. xi).

So far as biologists were concerned, that is what happened. The book was well reviewed, selected by *Choice* as one of the best academic books of that year, and my research on infanticide played a role in my election to the California Academy of Sciences in 1985 and to the National Academy of Sciences in 1990, as well as to the American Academy of Arts and Sciences in 1992. So far as anthropology was concerned, my optimism proved ill-founded.

In 1993 a long critique of my hypothesis that infanticide could be an adaptive reproductive strategy appeared in the *American Anthropologist* (Bartlett *et al.* 1993). An abridged version, "Infant killing as an evolutionary strategy: reality or myth?", by the same authors (anthropologist Robert Sussman, his student Thad Bartlett, and geneticist James Cheverud) followed. They claimed that "Most witnessed cases of infant killing appear to be simply genetically inconsequential epiphenomena of aggressive episodes" (1995:150). Publication was accompanied by a press release summarizing interviews with Dolhinow and other critics, generating articles in the popular press with titles like "Monkey 'murderers' may be falsely accused" (e.g. Mestel 1995).

The critics deemed comparative evidence from rodents irrelevant. This meant there was *no* evidence for a genetic basis for infanticidal behaviors, since everyone agreed that we had not shown this for primates. By then papers from the second international conference on infanticide held in 1990 at the Ettore Majorana Center in Erice, Sicily, had begun to circulate (Parmigiani and vom Saal 1994), including Volker Sommer's summary of 18 years of data from Jodhpur based on a long-term collaboration between Christian Vogel's team from Göttingen with Mohnot and others at the University of Jodhpur. That population of roughly 1,000 langurs had been continuously monitored by more than ten full-time grad students and post-docs, resulting in tens of thousands of observation hours. They had observed numerous takeovers accompanied by 13 "witnessed", 7 "likely", and 21 "presumed" cases of infanticide. In 95% of cases where paternity could be assigned, the killer had not been in a position to be the father. (DNA analyses demonstrating that males were not attacking their own infants would not be available until later; Borries *et al.* 1999). After reading his student Volker Sommer's Ph.D. thesis, even Vogel had reversed his earlier position, becoming Europe's strongest advocate of my hypothesis to explain infanticide by males.

Nevertheless, the article in the *American Anthropologist* included a half-page pie chart showing that 43.75% of all observed cases of infanticide derived from *Presbytis entellus*, while other species such as red colobus and blue monkeys living in the Kibale Forest of Uganda accounted for only a fraction of observed infanticide (Bartlett *et al.* 1993, Fig. 1). No mention was made of how many more hours many more individuals at Jodhpur had been monitored for far longer with excellent visibility. Instead, the authors suggested that the disproportionate number of cases meant there was something abnormal about langurs, and Jodhpur in particular. Furthermore, they claimed, killers were often fathers of their victims (Sussman *et al.* 1995: 149). This of course is not what the Jodhpur data they cited showed.

By then I was working on human inheritance patterns. Reluctantly, I joined Carel van Schaik and Charles Janson, then actively working on infanticide, to publish a brief reply (Hrdy *et al.* 1995). Against Carel's advice (and I regret not taking it) I inserted what I intended as a conciliatory passage about two different approaches to science. In the first

more cautious and deductive approach, researchers proceed from known facts without imaginative leaps. In the other, relying on “strong inference”, a hypothesis is devised even before all facts are in and researchers then test the predictions it generates (Platt 1964). “The continuing debate over infanticide among primates” I wrote,

reflects two different world views, both of them defensible. ... While some are interested in emphasizing the uniqueness of each case – a valid position – others are driven to seek general patterns and to use theory to explain them. ... The latter derive their greatest pleasure from noting that so many findings could have been correctly predicted on the basis of pitifully incomplete data sets merely by relying on logic, comparison, and extrapolations guided by evolutionary theory.

I had not meant to imply that I thought evidence was irrelevant. Nevertheless, for years afterwards, I would encounter in the pages of the *American Anthropologist* and elsewhere statements to the effect that Hrdy believes in “powerful models regardless of data” (Fuentes 2002: 696). Reading these still evokes a visceral, sick feeling.

Historically the American Anthropological Association’s flagship journal has not devoted much space to nonhuman primates, and virtually none to nonprimates. But once Sussman became editor, for the first time, the journal published an article by Canadian zoologist Anne Innis Dagg titled “Infanticide by male lions hypothesis: a fallacy influencing research into human behavior” (1999). The manuscript had previously been turned down by biology journals. When Sussman learned of it he phoned the author and told Dagg that if she added some references to primates, it would be publishable in the *American Anthropologist*. As Dagg subsequently told a reporter for *Lingua Franca*, she was “astonished” but pleased (Shea 1999: 25). Zoologist Craig Packer immediately replied that “Infanticide is no fallacy” (2000), as did a consortium of (mostly) primatologists led by Joan Silk and Craig Stanford, although I do not believe it was ever published. So the controversy rolled on – but without me.

Once before, in 1976 when the American Anthropological Association entertained a motion to “ban” sociobiology, I had resigned my membership in disgust, but later rejoined. This time, I felt as if I occupied some separate reality with nothing more that I could usefully or appropriately say. I returned to the debate over the sexual selection hypothesis again only once, just long enough to write the preface for the third volume on infanticide, this one edited by Carel Van Schaik and Charles Janson (2000). The title, *Infanticide by Males and Its Implications*, signaled their intention to ignore the controversy in anthropology and finally move on. It was a source of pleasure to me that two fine, innovative chapters were written by young anthropologists who had taken a 1984 seminar on infanticide I gave at UC-Davis in my first year teaching there. One, by Leslie Digby, reviewed infanticide by female mammals and its implications for the evolution of social systems; the other, by Ryne Palombit, explored “Infanticide and the evolution of male–female bonds in animals.”

Fieldwork, politics and lost opportunities

I discussed the infanticide controversy at some length because it may be of more interest to animal behaviorists than other aspects of my anthropological career. It was a minor skirmish

in the broader critiques of science by post-modern deconstructionists known as the "Science Wars" and in the larger controversy surrounding sociobiology. During much of this time, I was still going back and forth to India. Expenses rarely amounted to more than my air, rail, and bus fares, and the cost of renting a little bungalow within walking distance of the langurs. Only in the final years, as the research became better known, did we receive significant outside funding from the Smithsonian and National Science Foundation. This funding, along with an ambitious expansion of the project to include a number of researchers, turned out to be the kiss of death. By the end of the first funded year we found ourselves embroiled in a different sort of controversy, one which would bring fieldwork in India to a close.

The field of primatology was developing fast. My research on langurs was not keeping pace. In June of 1979, Irv DeVore and Dan submitted a joint Anthropology Department/Harvard Medical School proposal to do an "Integrated Field Study of the Behavior and Biology of the Hanuman Langur." (As an unsalaried post-doctoral researcher, I lacked standing to be a principal investigator.) It was an ambitious and, for its time, innovative proposal. We planned to integrate behavioral observations with epidemiology and genetics. While Sylvia Howe focused on maternal and allomaternal behavior, I would study female sexual behavior and Irv's grad student Jim Moore would tackle the roving "all-male bands" – langur male bands containing anywhere from 2 to 60 or more males, sometimes temporarily joined by females (Figure 13.1). Although fascinating, the fast-moving male bands were difficult to study. A pilot study on the steep hillsides around Abu indicated that Jim had the physical stamina to keep up with them. Meanwhile, Dan (by then working on double-stranded RNA viruses) and Rob Negrin from Harvard Medical School would study the epidemiology of rotavirus in langurs and the other animals (including humans) in their ecosystem. For this research, we would all collect stools and also briefly trap animals for measurements, tooth casts, and blood samples. Preliminary research (D. Hrdy *et al.* 1975) indicated that there were sufficient blood protein polymorphisms in langurs so that we could use blood samples combined with behavioral observations to work out relatedness and do some paternity exclusions. (This was in the days before we had less invasive methods for DNA analyses.) Had we succeeded, this would have been the first primate field study to integrate behavioral and genetic data.

From the outset there were problems. Our collaborators at Jodhpur University were eager to have us work among the provisioned langurs at Jodhpur. However, Jim and I worried about criticisms over human disturbance. Furthermore, Vogel and his team were understandably not happy to have us trap Jodhpur langurs that they were studying, nor did we want to. To Jim and me, Abu was the obvious choice. We had long-term records yet there was still much to learn, especially about the troops and male bands out on the relatively undisturbed hillsides. Sylvia Howe, on the other hand, wanted to work at Ranthambhore, a tiger sanctuary with a charismatic and highly effective field director (Fateh Singh Rathore) eager to help with the research.

Other problems had to do with public relations. In the ongoing political drama in South Asia, the U.S. had just announced that it would not continue supplying fuel to the Indian nuclear power plant at Tharapur, thus appearing to "tilt towards Pakistan." There was also

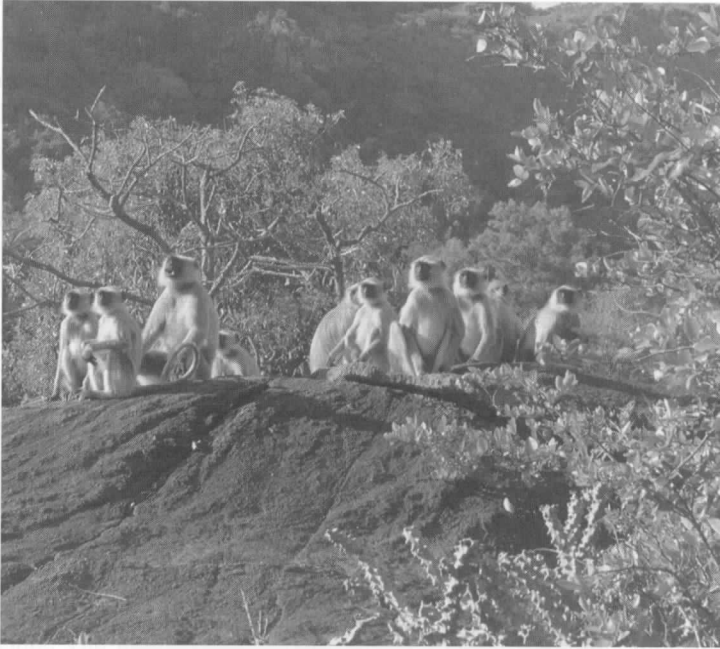


Figure 13.1. Langurs are found in breeding troops and in roving “all-male” bands containing anywhere from 2 to sixty or more males, sometimes, as in this rare photograph, temporarily joined by mothers who join with ousted males to avoid usurping males back in their “home troop”. (S. B. Hrdy/Anthro-Photo.)

tension over U.S. efforts to remove a ban on exporting monkeys for medical research. Photographs of macaques being used in U.S. radiation experiments had just been prominently published throughout India. All this meant that Americans seeking to trap monkeys that Hindus considered sacred, even if briefly and with no harm to them, had some explaining to do. Our problems made us vulnerable, but with tact and the support of our Indian colleagues, we should be able to satisfactorily explain our activities (Figure 13.2). However, there was another obstacle no one anticipated.

Our research permissions were granted at the federal level, through the Ministry of Education and Culture and the Ministry of Agriculture. Within Rajasthan, our contacts were with local forest officials at Abu and Ranthambhore. However, the Chief Wildlife Warden at the state level was the former director of the New Delhi Zoological Park, a politically well-connected expert on tigers, known for his tiger photographs. Unknown to us, this was the same individual who had undermined efforts to study Indian wildlife by other American researchers, including George Schaller (which is why Schaller did his classic study of snow leopards in Nepal instead of India). Only later, during the months Dan was fruitlessly commuting back and forth between Delhi and Jaipur in an effort to have our research permissions reinstated, with time on my hands to read old files in the American Embassy, did I learn about these previous – virtually all aborted – American projects, and notice the recurring patterns.



Figure 13.2. Studying animals regarded as sacred meant that many langurs were already habituated to people. But it also meant we had a lot of explaining to do if we wanted to mark or trap them. Here a langur visits one of the sadhus living in the hillsides around Mount Abu. (S. B. Hrdy/Anthro Photo.)

Why should we have wandered so blindly into this morass? I wanted to write about it. However, Smithsonian officials were concerned that I would further complicate the situation and requested I not do so. I complied, but in retrospect think it was a mistake. Many individuals in India and its government care deeply about Indian wildlife. Had what was going on become more generally known, the situation might have improved.

I first met the Chief Wildlife Warden of Rajasthan during his visit to Mount Abu in February of 1980. He was all charm. Later, after dinner at a local friend's home, we spoke late into the night and he told me that he had many admirers and many enemies but I was his "only friend." Still, I could not help but notice that whatever paperwork we provided, he always requested something more. He never confronted me directly. The full range of his mercurial personality was reserved for the students. When interacting with Sylvia Howe, he would alternate between great warmth, scathing attacks, and threats to have us all thrown in jail. It became increasingly clear that, for whatever reasons, this man did not want foreigners studying wildlife he regarded as *his*. When federal and local officials continued to support our work, sensational stories, originating from Jaipur, authored by a journalist friend of the Chief Warden, began to appear in the Indian press. Dan was accused of working for the

American "Defense Pathology" organization, with hints of involvement in germ warfare (which fortunately, after the matter came up in Parliament, the Indian Ministry of Defense opted to ignore, pointing out that no such organization existed). I was accused of wanting to export monkeys for profit. In a quote that I am sure was distorted and taken out of context, Professor Vogel was quoted as saying that our work would render langurs dangerous to humans.

About that time, a troop of langurs was trapped and then bludgeoned to death by villagers, probably in response to crop-raiding. On September 21, 1980, a story datelined Jaipur and titled "100 Langurs Killed" appeared in *The Statesman*. At the bottom appeared a gratuitous passage about how "American researchers on the Hanuman langur... had created a row... The issue was raised in Parliament and also in the State Assembly." The hint dropped there was picked up in the *National Herald* on September 23, which ran a story headlined **"FOREIGN HAND IN LANGUR KILLINGS?"** From then on, our fate was sealed. Though never actually revoked, our research permissions were suspended. Given the cultural sensitivity of the buttons pushed – U.S. conspiracies to get around bans on exporting monkeys, foreign agents, germ warfare, the murder of sacred monkeys – no official dared help us. About this time, Christian Vogel's team ran into similar problems, but the German government made foreign aid contingent on one German researcher a year being able to work at Jodhpur. The Indo-German langur project at least limped on, ultimately yielding important knowledge.

For Dan, me, and the students stranded in India but unable to watch monkeys, it was a nightmare of many months' duration. Hardest to bear was the sheer waste of it. No one gained. On June 10 of that year, before the storm but as clouds were massing, Irv DeVore wrote a masterful letter to Professor Roonwal, "grand-old-man" to "grand-old-man". Irv acknowledged current and past difficulties – international, local, and personal – then got to his point. If this American effort – by far the most carefully orchestrated of several previous efforts – failed, it was unlikely there would be another in the foreseeable future. After three decades, I still stay in touch with Indian colleagues, and every so often am reminded of Irv's letter when I read a proposal from a young Indian primatologist for whom, without much contact with developments outside, time seems to have stood still. Even though India has produced some of the world's finest scientists in highly competitive fields, and in spite of the existence in India of a rich array of lorises (two species), macaques (7), langurs (5), or the Hoolock gibbon (Roonwal & Mohnot 1977), few of these remarkable creatures have been well studied in the wild.

Occasionally a journalist writes about me and mentions my early research on "lemurs" (sic!).

Given how widespread langurs are, ranging from sea level in the south to high altitudes in the Himalayas, and given how relatively terrestrial and easy to observe these elegant and fascinating monkeys are, it is staggering how little we know about even this best-studied of all Indian primates. African baboons, also widespread and terrestrial, have become the best-known primates in the world. Yet many people don't even know what langur monkeys are. Several Indian species are liable to disappear before they are ever studied.

Raising Darwinian consciousness

It was the phenomenon of infanticide that drew me to study langurs. Increasingly, however, I found myself drawn into the “great Colobine soap opera” unfolding before me in female-centered social groups. Nor could I help empathizing with the plight of fellow females. Every 27 months or so, a strange male would burst into a mother’s world and stalk her infant. If he succeeded in killing it, within days, the mother would sexually solicit the killer. So why didn’t female langurs, *Lysistrata*-like, simply sexually boycott infanticidal males, eliminating this noxious trait from the gene pool? That mothers did not forced me to rethink selection pressures on females (Figure 13.3).

Most Darwinians still took for granted that natural selection weighed more heavily on males than on females. Even top textbooks still presumed that “most adult females... are likely to be breeding at or close to the theoretical limit” while “among males by contrast there is the probability of doing better” (Daly & Wilson 1978: 59). The *Langurs of Abu: Female and male strategies of reproduction* was the first book on wild primates to devote equal attention to the reproductive strategies of *both* sexes. Yet, as I ruefully noted in the Preface to the 1980 paperback edition, responses focused almost entirely on males. Male behavior, I complained, has this power to rivet attention.

The chapter on female–female competition described a novel form of female dominance hierarchy, one in which young females gradually rose in rank, occupying the top positions in the hierarchy when they were at peak reproductive value, and then declining with age. It was because I was unsure about this interpretation that after submitting my thesis in 1975, I had rushed back to Abu. When I left the season before, three subadult females in Toad Rock Troop – if my model based on reproductive value was correct – were poised to rise to the top of the female hierarchy. But would they? On my return, I was amazed by how unanimously



Figure 13.3. A “sisterhood” of langurs resting in a *Grevillea* tree at Abu. (S. Hrdy/Anthro-Photo.)

the monkeys confirmed my predictions. All three young adult females routinely displaced older females who weighed more, monopolizing the top three rungs of the female hierarchy. As usual, the oldest females remained at the bottom (Hrdy 1977, Fig. 6.9). Only later would members of the Indo-German team confirm the existence of this same pattern in langurs elsewhere, first at Jodhpur, and then at the new langur study site Paul Winkler and Volker Sommer founded in Nepal (see Borries *et al.* 1991). It was a never-before-documented type of female dominance hierarchy, consistent with George Williams' ideas about the importance of reproductive value. I waited in vain for some feedback.

Similarly, the chapter on infant-sharing provided the most detailed examination to that point of exactly which females engaged in allomaternal care and why. Yet almost all the commentary focused on males (Figure 13.4). In the preface to the paperback edition, I actually pleaded with readers, expressing the hope that "this paperback edition will ... call attention to a facet of the behavior of female primates too lightly brushed aside the first time around: The extent to which females are competitive creatures of strategy whose preoccupations extend far beyond 'mothering' and the traditional boundaries of 'maternal behavior'" (1980:vi). I knew that we could not understand reproductive strategies of either sex without taking into account the other, but in reaction to the androcentric response to *The Langurs of Abu*, I decided to make females the focus of my next book. There was also another reason. The intellectual ferment that was transforming the intellectual landscape within the social sciences, and which was also just beginning to transform the genderscape at American universities, was trickling into my consciousness.

Without my being very aware of it, the Women's Movement had been picking up steam. In fields such as history and anthropology, reactions against "top-down" history, along with



Figure 13.4. Two mothers spar at the interface between their home ranges. When the *Langurs of Abu* was first published, I received a letter from sociologist Jesse Bernard telling me that this was the first image she had ever seen of female–female aggression in nonhuman primates. Overall though, most responses focused on male behavior. (D. Hrdy/Anthro-photo.)

a new interest in marginalized peoples, were already under way. In the emerging field of women's studies, critiques of science were actually pitting "feminist" scholars *against* science, sociobiology in particular. This was partly in response to what was happening at roughly the same time in the life sciences. Genetics was moving to center stage, including among those seeking to understand the evolution of sex differences. The "gendering" of the social sciences was on a collision course with "the gening of America" and especially with what social scientists regarded as the twin evils of "genetic determinism" and "reductionism" inherent in sociobiology. It was a time of tremendous intellectual tension. I was caught smack in the middle.

To me the outstanding problem was Darwin's brilliantly original theory of sexual selection, so powerfully explanatory in some spheres (as when applied to explain the evolution of infanticide by males), yet so short of the mark in explaining how females also strive for reproductive success. Important assumptions underlying the theory, especially those being used to extrapolate to humans, were based on misleading Victorian stereotypes. After Ernst Mayr alerted Trivers to the "key" reference for his 1972 paper, the rediscovery of Bateman's *Drosophila* experiments highlighted this supposed dichotomy between an "undiscriminating eagerness" to mate in males, and "discriminating passivity" in females. Bateman had extrapolated from observations of a single species of fruit-fly to humans. From there, often invalid stereotypes about "The Reluctant Female" and "The Ardent Male" passed unchallenged into sociobiology (see, for example, Daly & Wilson 1978: 55), and later (long after animal behaviorists knew better), into evolutionary psychology.

In *The Woman that Never Evolved* (Hrdy 1981) I surveyed both power relations between males and females and the role played by female-female competition. I identified polyandrous tendencies universally present among primates and explored reasons why females in so many species of monkeys and apes expend so much energy and take such risks to mate with more males than are needed to ensure conceptions—one of the reasons male primates have to try so hard to control females (my first foray into the origins of patriarchy). I reviewed emerging evidence for cyclical libido as well as lapses from it (as in situation-dependent sexual receptivity) and explored selection pressures shaping sexual swellings at estrus, functional clitorises and erratic orgasmic reward systems, explaining why primates (humans included) eschewed conspicuous advertisements at ovulation. My goal was to demonstrate that the sexually passive female in Darwinian stereotypes could not have evolved within the Order Primates. But this was still some years before our current open discussion of female sexuality. Even the great Masters and Johnson only broached topics like female orgasm wearing white lab coats and grave faces. I was more than a little anxious. When a friend cautioned me that "it sounds like a woman looking between her legs with a mirror" I deleted the subtitle "*a primatologist examines her sex*".

The book was well received, selected by the *New York Times Book Review* as one of the Notable Books of the Year. Furthermore, the backlash I anticipated from feminists never came. Through a stroke of good fortune, an early review appeared in the radical Washington, D.C., newsletter *Off Our Backs*. The reviewer had background in biology, grasped my intentions, and declared that "every aspect" of the book reflects a feminist perspective. This

gave some of sociobiology's fiercest feminist critics cause for pause. Thereafter, the most common responses from those in women's studies and related fields were polite nods, but initially not a whole lot more. By now, however, gender studies programs were more open towards biologically based explanations of behavior.

This transformation was beginning to be apparent during the period between 1995, when I first spoke in the Gender and Science program at Princeton, and 1999 when I spoke there again. On the first occasion I gave a talk called "Raising Darwin's consciousness: female sexuality and the prehuman origins of patriarchy." Questions following my lecture were tinged with hostility. I was pointedly asked why evolutionists were so fixated on reproduction? Didn't our bias "privilege" heterosexuality? When in 1999 I lectured there again, this time on maternal love and ambivalence (for feminists an even more politically charged topic than sexuality is) I sensed genuine interest in the biology of women as well as recognition of a need to engage with evolutionary concepts.

I was hardly the only Darwinian interested in revising longstanding biases. Later, I learned more about women writers – marginalized from science and largely ignored – who a century earlier had politely proposed that evolutionary theories would benefit from greater consideration of selection pressures on females (Hrdy 1999: 12–23). Women's contributions have this way of fading from view. All the more reason not to forget this humbling history. Even though some contemporary biologists remain resistant to the notion that evolutionary theory ever had a problem with androcentric bias, that concepts needed to be revised, or that inclusion of women researchers had anything to do with fixing them ("Females were just harder to study," a prominent British biologist told me with a perfectly straight face), what stands out most in my memory is that from the early 1980s on there was a lot of support from such legendary evolutionists as George Williams and Bill Hamilton. George went beyond moral support, lending his eminence, offering to co-author a critique of "Darwin and the Double Standard" with a younger, far less distinguished colleague (Hrdy & Williams 1983). Later, John Maynard Smith and Bill Eberhard would also urge more open discussion about how "inadvertent machismo" affected the way sexual selection theory was applied. The 1994 Symposium on Evolutionary Biology and Feminism, conceived by Patricia Adair Gowaty and sponsored by the Society for the Study of Evolution, marked the culmination of this trend (Gowaty 1997).

Far from resistance, from the 1980s onward there was a small stampede among animal behaviorists to study female reproductive strategies, especially phenomena related to female mate choice. One of the ironies of the charge "sexist" so thoughtlessly leveled against sociobiology in the 1970s was that it was sociobiology's relentless focus on selection at the level of individuals that, after more than a century of neglect, ushered in an expansion of evolutionary theory to include both sexes.

Correctives proposed by women had nothing to do with females "doing science differently." In an essay entitled "Empathy, polyandry and the myth of the coy female" I traced the impact that questions raised and pursued by women researchers have had in behavioral biology, especially regarding sexual selection. Far from rejecting Darwinian theory, we sought to refine and expand it so as to better encompass selection pressures on both sexes.

The essay appeared in a volume on *Feminist Approaches to Science* (1986). Although I was explicitly *not* using feminist perspectives to arrive at any ideologically preordained conclusion, the presence of the “F-word” ensured that few biologists ever read it. I was grateful therefore when, two decades later, Elliott Sober reprinted it in the third edition of his *Conceptual Issues in Evolutionary Biology* (2006).

Women, science and compromises

Up to this point, the mentors mentioned have all been men. There is a reason. The year I graduated from Radcliffe, I only recall one woman professor, Cora DuBois. She retired that year. By 1970, there was not a single female full professor in Harvard’s Faculty of Arts and Sciences, leading in April of that year (according to a June 11, 1970, story in the *Crimson*) to Harvard becoming the first school in the nation to come under (preliminary) federal investigation for sex discrimination. Yet there *were* talented women about. One day while lost in the labyrinthine basement of the Peabody Museum, as if in a fairytale, I encountered an elf-like, very intense, chain-smoking woman in a tiny office down there. She was Tatiana Proskouriakoff, the museum’s “honorary curator” of Mayan art, at that time probably the greatest living Mayanist. (Proskouriakoff discovered that hieroglyphs carved on monuments actually recorded historical events.)

So far as primatology was concerned, I had to look far beyond Harvard for female role models, to Jane Lancaster, and especially Alison Jolly, then in an adjunct position at Sussex. I vividly remember holding Alison’s path-breaking 1966 monograph on *Lemur Behavior* and scrutinizing the photograph on the back, staring into the woman’s eyes. Later, when Jolly visited Harvard, I surprised myself by impulsively blurting out from within a throng of graduate students clustered about her: “But what is your life like?” I admired her as a researcher, writer, and as a person, and also knew that she had a husband and children. How did she manage? There was no real opportunity for her to answer then. Later I would learn that (like me) she never held a full-time tenured professorship. Alison was probably my primary role model.

In 1975, the year I received my Ph.D., molecular biologist Nancy Hopkins published an essay entitled *The high price of success in science*. Nancy went on to become a professor at MIT, as famous for her activism on behalf of women scientists as for her research on genes involved in cancer. Except for a brief union in graduate school, she never married, becoming a self-described “nun of science” until suddenly at age 60 she fell in love and married a financier (Sipher 2007). Hopkins questioned whether it was possible for a woman to be a successful wife and mother as well as earn a living as a full-time professor at a top research university. “Serious science with its long hours and energy absorbing quality is barely compatible with motherhood and being a professor ... it is barely even compatible with a sustained husband/wife relationship.” In her view, “the intellectual processes involved in ‘real’ science are as natural (or unnatural) to women as they are to men. But ‘professional’ science was constructed by and for men (a certain type of man), and a woman who chooses

to conquer this world at its higher echelons usually requires a major overhaul of self and world views" (1975: 16).

Such views impacted me, though not always in ways I was aware of at the time. Years after the fact, the anthropologist William Irons ruefully told me how, shortly after I received my Ph.D., he and Napoleon Chagnon had sought to recruit me to their university. Because the anti-sociobiology campaign was in full swing, Penn State's was virtually the only Anthropology Department in the United States actually *looking* to hire a sociobiologist. In line with current practice, they first approached my advisor, not me. According to Irons, he simply said "Oh, she's married" and proposed another recent Ph.D. with similar training in primate sociobiology, male, and also married, whom they hired.

Nancy Hopkins had in mind scientists who ran big labs, who every few years would have to confront stiff competition in order to get grants to keep them running. Still, my advisor had a point – married women in academics confront special challenges. Since institutions were not going to change in time, I had to make my own adjustments. As a primatologist familiar with spending long periods of time at Abu by myself, I already had an idea about how incompatible fieldwork might be with family life.

After our research permits in India were suspended, instead of trying to start up from scratch in another country I shifted to archival research. I loved fieldwork, and regretted the shift. Yet, I reminded myself, none of my compromises were so great as those made by intellectually ambitious women before me, women like St. Hildegard of Bingen, the abbess who first described estrous cycles in monkeys. She really was a "nun of science." Plus, as I saw it, after the debacle in India, it was Dan's turn. He had made massive sacrifices to share my work in India. Thus in 1981 I followed Dan to Houston, where he could complete his medical residency at Baylor University. I took a part-time position across the street as a visiting associate professor of Anthropology at Rice University.

Spring of 1982 we returned to Cambridge so Dan could complete his Infectious Diseases fellowship at Peter Bent Brigham hospital and his doctoral research in Bernie Fields' lab at Harvard Medical School. By then I had two children. Assessing the three strands to my life then – *family*; *research and writing*; and *teaching* – I figured I could manage two of the three. But given institutional expectations and support systems then available, committing to all three involved cutting more corners than I wanted. I remained an (unsalaried) research associate at the Peabody Museum while working as a volunteer at Sasha and Katrinka's daycare center. In 1984, Dan accepted a position at the University of California-Davis medical school. Benefiting from a special program to target "outstanding women", simply by flying across the country, I went from being a volunteer in a daycare center to being a full professor permitted the option of working part-time.

Work, life, balance, and compromise

Conflicts between women's aspirations and the needs of their children are complex and very nearly irreconcilable. Still, some compromises work out better than others. Whenever some administrator or journalist holds me up as an example of a woman who combined a career in

science with rearing three children, I feel obliged to point out how misleading that term "career" is. I never worked "full-time", or more precisely, was never paid for working full-time. I expended far more on allomaternal assistance than I ever earned from teaching. I could only afford to provide what my children needed, forgoing medical benefits and pension plans, because I benefited from inherited wealth. It's worth asking then how many women far more talented than I am had to forgo careers in science not just for the usual reasons (subtle and not-so-subtle discrimination) but because they were offered situations on terms that few mothers would be willing to accept? Nor do I exemplify the "opt-out revolution," the term used by *New York Times Magazine* writer Lisa Belkin in 2003 to describe highly educated professional women who step off the "fast track" to "stay home" with children. It's true that in 1996 I abruptly retired, but as I tried to explain to the NYT magazine's fact-checker when she called, I was *not* opting out.

By the 1990s, I had begun archival research on human inheritance patterns and "parental investment after death" in collaboration with Debra Judge, another "lapsed primatologist". We combined classic ethnographic research with empirical tests of hypotheses generated by sociobiological theories (Judge & Hrdy 1992; Hrdy & Judge 1993). Ours was the most detailed, longitudinal analysis of American probate records ever undertaken. It was tedious work undertaken in dusty courthouses, but interesting enough once you got into it. More importantly, it enabled two mothers to flexibly schedule research close to home.

Like many women, I entered this period of my life sandwiched between work (research, teaching, and administrative duties) and being a wife and mother as well as having a natal family that needed me back in Texas. My irreconcilable dilemmas involved work versus family as well as questions about *which* family? Commuting back and forth between the university and home, California and Texas, I developed such classic stress symptoms as migraines and crippling neck and back pain.

After a prolonged illness, my mother was dying. Her son, my brother, was killed the year before. Both she and I were convinced, but never able to prove, that he was murdered. In the wake of their deaths, problems in my natal family ballooned out of control. My life was taken hostage by, among other things, a series of court cases and lawsuits. My university allowed me time off. But when the dust settled, I questioned whether after such a hiatus I would be able to re-engage in creative work in a fast-developing and competitive field?

In 1986 I had received a Guggenheim fellowship to write a book on "the natural history of mothering", but my third child, Niko (named for Niko Tinbergen), was born that year. Several papers, but no book, got written. I either needed to write the book, or abandon the idea. In 1996 I submitted a proposal to publishers, and was surprised by the interest, which resulted in an auction. Instead of going with the highest bidder, I selected Pantheon because of their fine science editor, Dan Frank. Even so, I emerged with an advance that over the three years it took to write the book paid more generously than any university salary I had ever received (which at quarter-time was US\$15,000 annually when I retired). For the next three years my time was divided between *Mother Nature* and being a mother to talented, strong-willed, complicated human youngsters. After the book was done, it was awarded the Howells Prize for outstanding contribution to Biological Anthropology and chosen by both

Publisher's Weekly and *Library Journal* as one of the best books of 1999. UC, which had accorded me the title of *emerita* when I retired, awarded me their "Panunzio Award" for post-retirement productivity. Leaving the university was scarcely "opting out."

By this time, Dan was engaged in commercial walnut-growing, and we were living on a farm near Davis, intensely involved in our offspring's lives as well as habitat restoration, consciously seeking to lead "balanced" lives. It was 1997, while working on *Mother Nature*, that I reread Nancy Hopkins' essay and picked up the phone to ask her what she thought decades later. By then, of course, female footholds in science were considerably more secure. Yet as Nancy put it: "Each generation of young women thinks it is an issue of the past and then has to discover for themselves ... (how hard it is) to create an environment where their own way of being is allowed" (pers. comm. July 16, 1997). The elephant in the lab is the tremendous responsibility rearing children entails, even more daunting perhaps for parents familiar with humankind's evolutionary heritage. Left alone, even for minutes, infant primates are well within their rights to exhibit distress. In the hominid line infants seek even more than tactile reassurance, perpetually monitoring for signs of emotional commitment from mothers and allomothers alike. For reasons explained in *Mother Nature* and elsewhere (Hrdy 2005), human infants are "connoisseurs" of such commitment, requiring more than other primates. Nor (as some administrators seem to imagine) does parental investment end with infancy or when children are "school-age" – neither in the Pleistocene, nor today.

Science and motherhood

My first child had been born in 1977. Twice we took Katrinka to remote parts of Rajasthan. I doubt I would have done so had Dan not been an infectious disease specialist (Figure 13.5 and 13.6). Even so, it was harrowing for all concerned. The first year I brought along a young artist/*au pair* eager to experience Rajasthan, but alas, not interested in childcare. Throughout that field season, Katrinka suffered chronic diarrhea and virulent diaper rash. I would come back from watching langurs dawn-to-dusk to find an unhappy toddler and problems needing to be solved, ranging from laundering diapers to preparing healthy meals. Exhausted, I became ill. On the plane home I lay prostrate on a row of seats, suffering from pneumonia, with a temperature of 104 degrees, sucking on one of Katrinka's baby bottles to stay hydrated. Next day, the *au pair* quit.

Another time, I left Katrinka in Cambridge with her father and a housekeeper – also hard on her. My second daughter, Camilla ("Sasha"), was only a week old when she flew with me to Ithaca for the First International Conference on Infanticide – no place for a baby, my co-organizer declared (Figure 13.7). I was told not to bring her into the building where the conference was held. This is why in the days before flying to Ithaca I enlisted another mother with a year-old baby and lots of milk to nurse Sasha daytimes while I suckled her only at night so as not to build up my milk supply so much I could not handle hours at the conference. I was taking a risk. Back at the hotel, Sasha was given expressed milk from a



Figure 13.5. Katrinka was 14 months old when she first traveled with me to Rajasthan. Taking young children to the field can be logistically challenging, and Katrinka herself often found her strange new environment daunting. (D. Hrdy/Anthro-Photo.)

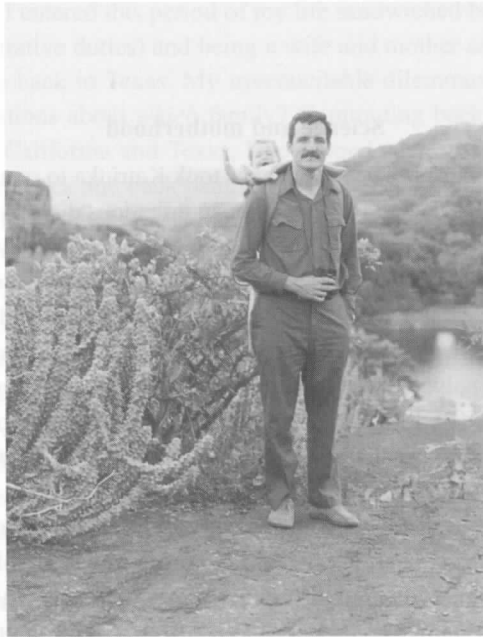


Figure 13.6. Dan joined us when he could take time off from his own research on rotaviruses in Bernie Fields' lab at Harvard Medical School. (S. B. Hrdy/Anthro-Photo.)



Figure 13.7. Sasha was born well past her due date, arriving a week before the 1982 conference on Infanticide in Animals and Man. This panel from a comic strip by my friend Susan Meddaugh, author of *Martha Speaks*, presents a child's-eye view of her mother's obsessions, which may or may not explain Sasha's late arrival. (Courtesy of Susan Meddaugh.)

bottle by a nurse. She might have learned to prefer the more rapid flow of the bottle and thereafter refuse my breasts. Fortunately, Sasha was an easy-going baby.

My third child, Niko, also experienced premature exposure to academia. Joan Silk, Dorothy Cheney, and I decided to organize a session at the American Anthropological Association meetings to demonstrate why anthropology departments needed to hire young primatologists. Such events are planned far in advance. As it happened, we all three turned up nursing new babies. From the outside it may have looked like we easily combined motherhood with professional responsibilities. From the inside – speaking now only for myself – I was too preoccupied to learn or offer much of interest. And what of infants treated this way?

By this time I was busy learning everything I could about infant development and attachment theory, as well as the biological, evolutionary, and historical bases of maternal love, and to combine this with what I knew about maternal ambitions and the ambivalence that I knew from personal experience tensions between maternal love and ambition could produce. Increasingly convinced that, unlike other apes, human mothers had not evolved to

care for their children alone, and that our species had evolved as “cooperative breeders” (Hrdy 1999, 2005), I looked for ways to build up a stable network of as-if extended family, composed of daycare providers and resident allomothers. One of these allomothers, Lupe de la Concha, has lived with us now for more than twenty years. She has her own career but continues to be as involved in the lives of far-flung Hrdy progeny as I am. Also, for the first time I was beginning to have collegial support from women evolutionary biologists, a commodity hard to come by early on.

In graduate school, Martha McClintock had been my only “age mate.” I grieved when she left Harvard to complete her Ph.D. at Penn. I felt increasingly alienated by “post-modern” intellectual trends within anthropology that struck me as anti-scientific. But the closer I got to Harvard’s evolutionists, the scarcer women became. At Davis, fellow professors in biological anthropology were also all male, albeit preternaturally supportive ones. As at Harvard, my women friends tended to be younger, often my own or Peter Rodman’s students, Joan Silk, Meredith Small, Amy Parish, and Debra Judge. It was the 1990s before I connected with a “critical mass” of female colleagues my own age with whom I could explore ideas about how to combine productive careers in science with healthy human lives – a vital, ongoing discussion.

Separated by vast distances, we found innovative ways to meet (e.g. house parties). These friendships made a difference to me personally as well as to my work. Patty Gowaty’s bold theoretical footprints are all over my writings from the late 1990s, Jeanne Altmann’s on *Mother Nature*, and after 1998, nothing I wrote was ever *not* influenced by Mary Jane West-Eberhard’s ideas about development, Kristen Hawkes’ and Polly Wiessner’s views on early human food sharing, or Sue Carter’s coaching on proximate causes. A 2003 Dahlem conference in Berlin that Sue Carter spearheaded was the first scientific meeting I ever attended where discussions about infant emotional needs and optimal daycare were incorporated right into week-long sessions on the neurobiology of parental behavior (Carter *et al.* 2005). The resulting synthesis provides the groundwork for my current book, *Mothers and Others: The Origin of Emotionally Modern Humans*.

If previously I identified with “a lonely castaway throwing message bottles from a desert island,” increasingly I felt like a swimmer “gently uplifted by a pod of supportive dolphins – fellow Darwinians and fellow feminists” (as I mused in 1999: 599). Evolutionary theory, which had for so long overlooked or miscast selection pressures on females and left out altogether consideration of the special cognitive as well as emotional needs of human infants, was being revised and expanded in ways that brought me closer to the old-fashioned anthropological mission I signed on for: “studying human nature in all its diversity.”

Acknowledgements

Thanks to Jeanne Altmann, Dan Hrdy, Camilla Alexandra Hrdy, Bernard von Bothmer, Jim Moore, June-el Piper, Volker Sommer, and M. J. West-Eberhard for advice.

References

- Altmann, J. (1980). *Baboon Mothers and Infants*. Cambridge, MA: Harvard University Press.
- Altmann, S. A. & Altmann, J. (1970). *Baboon Ecology: African Field Research*. Chicago, IL: University of Chicago Press.
- Bartlett, T. Q., Sussman, R. W., & Cheverud, J. M. (1993). Infant killing in Primates: a review of observed cases with specific references to the sexual selection hypothesis. *Am. Anthropol.* **95**: 958–9.
- Bishop, N., Hrdy, S. B., Teas, J. & Moore, J. (1981). Measures of human influence in habitats of south Asian monkeys *Int. J. Primatol.* **2**: 153–67.
- Blaffer, S. C. (1972). *The Black-man of Zinacantan: A Central American Legend*. Texas Pan-American Series. Austin, TX: University of Texas Press.
- Borries, C., Sommer, V. & Srivastava, A. (1991). Dominance, age, and reproductive success in free-ranging Hanuman langurs (*Presbytis entellus*). *Int. J. Primatol.* **12**: 231–57.
- Borries, C., Launhardt, K., Epplen, C., Epplen, J. T. & Winkler, P. (1999). DNA analyses support the hypothesis that infanticide is adaptive in langur monkeys. *Proc. R. Soc. Lond. B* **266**: 901–4.
- Calhoun, J. (1962). Population density and social pathology. *Scient. Am.* **206**: 139–48.
- Carter, C. S., Ahnert, L., Grossmann, K. E., et al. (eds.). (2005). *Attachment and Bonding: A New Synthesis*. Cambridge, MA: MIT Press.
- Dagg, A. I. (1999). Infanticide by male lions hypothesis: a fallacy influencing research on human behavior. *Am. Anthropol.* **100**: 940–50.
- Daly, M. & Wilson, M. (1978). *Sex, Evolution and Behavior*. North Scituate, MA: Duxbury.
- Dolhinow, P. (1977). Normal monkeys? *Am. Scient.* **65**: 266.
- Fuentes, A. (2002). Review of infanticide by males and its implications. *Am. Anthropol.* **104**: 693–6.
- Gowaty, P. A. (ed.). (1997). *Feminism and Evolutionary Biology*. New York: Chapman and Hall.
- Hausfater, G. & Hrdy, S. B. (eds.). (1984). *Infanticide: Comparative and Evolutionary Perspectives*. Hawthorne, NY: Aldine.
- Hopkins, N. (1975). The high price of success in science. *Radcliffe Quarterly*, July issue.
- Hrdy, D. B., Barnicott, N. A. & Alper, C. A. (1975). Protein polymorphism in the Hanuman langur (*Presbytis entellus*). *Folia Primatol.* **24**: 173–87.
- Hrdy, S. B. (1974). Male-male competition and infanticide among the langurs (*Presbytis entellus*) of Abu, Rajasthan. *Folia Primatol.* **22**: 19–58.
- Hrdy, S. B. (1976). The care and exploitation of nonhuman primate infants by conspecifics other than the mother. *Adv. Study Behav.* **6**: 101–58.
- Hrdy, S. B. (1977). 1980 (revised paperback edition). *The Langurs of Abu: Female and Male Strategies of Reproduction*. Cambridge, MA: Harvard University Press.
- Hrdy, S. B. (1979). Infanticide among animals: a review, classification, and examination of the implications for the reproductive strategies of females. *Ethol. Sociobiol.* **1**: 13–40.
- Hrdy, S. B. (1981). 1999 (second edition with new preface). *The Woman that Never Evolved*. Cambridge, MA: Harvard University Press.
- Hrdy, S. B. (1986). reprinted 2006. Empathy, polyandry and the myth of the coy female. In *Conceptual Issues in Evolutionary Biology*, 3rd edn., ed. Elliott Sober, pp. 131–59. Cambridge, MA: MIT Press. (Page numbers refer to 2006 reprint.)
- Hrdy, S. B. (1997). Raising Darwin's consciousness: female sexuality and the prehuman origins of patriarchy. *Human Nature* **8**: 1–49.
- Hrdy, S. B. (1999). *Mother Nature: A History of Mothers, Infants and Natural Selection*. New York: Pantheon.

- Hrdy, S. B. (2002). The past, present and the future of the human family. *The Tanner Lectures on Human Values* 23: 57–110. Salt Lake City, UT: University of Utah Press.
- Hrdy, S. B. (2005). Comes the child before man: cooperative breeding and the evolution of prolonged post-weaning dependence. In *Hunter Gatherer Childhoods*, ed. B. Hewlett & M. Lamb, pp. 65–91. New Brunswick: Aldine/Transaction.
- Hrdy, S. B. (2009). *Mothers and Others: The Origin of Emotionally Modern Humans*. Cambridge, MA: Harvard University Press.
- Hrdy, S. B. & Hrdy, D. B. (1976). Hierarchical relations among female Hanuman langurs. *Science* 193: 913–15.
- Hrdy, S. B. & Judge, D. S. (1993). Darwin and the puzzle of primogeniture: an essay on biases in parental investment after death. *Human Nature* 4: 1–45.
- Hrdy, S. B. & Williams, G. C. (1983). Behavioral biology and the double standard. In *Social Behavior of Female Vertebrates*, ed. S. Wasser, pp. 3–17. New York: Academic Press.
- Hrdy, S. B., van Schaik, C. & Janson, C. (1995). Infanticide: let's not throw out the baby with the bath water. *Evol. Anthropol.* 3: 151–4.
- Jay, P. (1963). The social behavior of the langur monkey. Ph.D. dissertation, University of Chicago.
- Judge, D. S. & Hrdy, S. B. (1992). Allocation of accumulated resources among close kin: inheritance in Sacramento, California, 1890–1984. *Ethol. Sociobiol.* 13: 495–522.
- Levi-Strauss, C. (1966). *Du Miel aux Cendres*. Paris: Plon.
- Mestel, R. (1995). Monkey “murderers” may be falsely accused. *New Scientist* (July 15): 17.
- Mohnot, S. M. (1971). Some aspects of social changes and infant killing in the Hanuman langur, *Presbytis entellus* (Primates Cercopithecidae) in Western India. *Mammalia* 35: 175–98.
- Packer, C. Infanticide is no fallacy. Commentaries. *Am. Anthropol.* 102: 829–57.
- Parmigiani, St. & vom Saal, F. (eds.). (1994). *Infanticide and Parental Care*. Ettore Majorana International Life Sciences Series vol. 13. Chur, Switzerland: Harwood Academic.
- Platt, J. R. (1964). Strong inference. *Science* 146: 347–53.
- Roonwal, M. L. & Mohnot, S. M. (1977). *Primates of South Asia: Ecology, Sociobiology and Behavior*. Cambridge, MA: Harvard University Press.
- Schubert, G. (1982). Infanticide by usurper hanuman langur males: a sociobiological myth. *Soc. Sci. Inform.* 21(2): 199–244.
- Shea, C. (1999). Motive for murder. *Lingua Franca* 9(6): 23–5.
- Sipher, D. (2007). Nancy Hopkins and Dinny Adams. *New York Times*, July 29, 2007.
- Sommers, V. (1994). Infanticide among the langurs of Jodhpur: Testing the sexual selection hypothesis with a long-term record. In *Infanticide and Parental Care*, ed. S. Parmigiani & F. vom Saal, pp. 155–98. Chur, Switzerland: Harwood Academic.
- Sugiyama, Y. (1965). On the social change of hanuman langurs (*Presbytis entellus*) in their natural conditions. *Primates* 6: 381–417.
- Sussman, R. W., Cheverud, J. M. & Bartlett, T. Q. (1995). Infant killing as an evolutionary strategy: reality or myth? *Evol. Anthropol.* 3: 149–51.
- Symons, D. (1979). *The Evolution of Human Sexuality*. Oxford: Oxford University Press.
- Trivers, R. L. (1972). Parental investment and sexual selection. In *Sexual Selection and the Descent of Man 1871–1971*, ed. B. Campbell, pp. 136–79. Chicago, IL: Aldine.
- Van Schaik, C. & Jansen, C. (eds.). (2000). *Infanticide by Males and its Implications*. Cambridge: Cambridge University Press.
- West-Eberhard, M. J. (2003). *Developmental Plasticity and Evolution*. Oxford: Oxford University Press.
- Wilson, E. O. (1975). *Sociobiology: The New Synthesis*. Cambridge, MA: Harvard University Press.