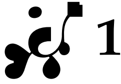 1

The Rabies Run

Marjorie and I sat in the dark on cold sand beside the flooded *molapo*, our chins on our chests in despair. In the middle of the black water we'd tried to ford we could see Mel on the front seat of the drowned Land Rover, reading the *Merck* medical manual by torchlight. In the luggage section behind him, two dogs howled from the crates in which they were imprisoned.

The previous afternoon we had discovered our beloved camp dogs foaming at the mouth. We had been treating them for what we thought was biliary (a mild canine distemper), inserting by hand into their throats big tablets given us by the traveling vet. But the foaming, which was new, terrified us with the possibility of rabies. We crated the dogs, packed our supplies and clothes quickly, and headed for the nearest town. There, the dogs could be tested and we could find a hospital or clinic for ourselves.

Problems: the nearest town of any size was Maun, Botswana, some 250 kilometers away; the astonishing rainy season of 1970–71 turned the western fringes of the Kalahari Desert into a series of swollen *molapos*, rivers running between transverse dunes in a heavy sand landscape that covered at least two hundred of those kilometers; and the only road between our camp at Dobe and the town of Maun was a deep and twisting track, difficult to navigate at the driest of times but treacherous with sandy and muddy potholes from December to March, the only time rain falls at all in the Kalahari. It was then February 1971.

Only a few kilometers east of Dobe, we'd spent fourteen hours digging ourselves out of our first mudhole by lantern light, our first night on the road. Now, only a few more kilometers east of that first mudhole, we were into our second night: water had been sucked into our tailpipe when we tried to cross a section of molapo deeper than we thought it was, and our exhaustion and fear were such that all we could do was sit and worry whether poking pills past our dogs' teeth might have exposed us to rabies.

Other than the dogs' intermittent howling, we were surrounded by the utter silence of the Kalahari. It was then, sitting on the sandy bank with her legs caked in the dried mud of the first mudhole, that Marjorie

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began to have symptoms. She would reach out her arm to pick up her hat or notebook, and her arm would spasm, pulling backwards alarmingly. She was convinced she had contracted rabies and would die.

Mel found in *Merck* that if we indeed had been exposed, we had possibly forty-eight hours to start the rabies vaccine series (fourteen days of painful shots to the stomach). And we were still more than two hundred kilometers, most of them similarly flooded, from Maun. At that news, the tension that had been building between me and Marjorie for other reasons—among them that I was an overeager newcomer, and she was ready to finish fieldwork and go home—overwhelmed us. All three of us stopped talking completely.

I was in hell, I thought. I was twenty-five years old and had been in Africa just 50 of the 550 days scheduled for my PhD fieldwork there. I had no return ticket yet back to the US, I was going to be on my own without anyone to speak English to for most of the rest of the time, and the Ju/'hoan language (erroneously called !Kung at the time by scholars) was a basically unwritten one I would have to learn by immersion. I had had some oral lessons in !Kung back in Massachusetts from fellow grad student Pat Draper and knew it had four click consonants; only later did I learn that, combined with other consonants, the permutations of these four clicks with other consonants yielded a total of eighty-four consonants completely unknown in European languages.

***

I was the last graduate student admitted to Richard Lee and the late Irven DeVore's Harvard Kalahari Research Group (HKRG) project, based in the Harvard Department of Anthropology. I left the US in November 1970 to join fellow students Melvin Konner and his wife Marjorie Shostak at Dobe, in far northwestern Botswana, where the multifaceted HKRG project had been based since 1963. My arrival in Botswana was delayed by almost a month because the Lykes Lines freighter on which I sailed from New Orleans to Cape Town, South Africa, wandered around the equator and the South Atlantic for a few weeks due to the captain running out of booze and coming down with delirium tremens. The first mate took a while figuring out how to get the ship to Cape Town. As supplies began to run out, we actually ate flying fish that landed on the lower decks.

Sometimes I spent the days of delay in language study belowdecks to avoid the other six passengers (all American missionaries bent on converting me to a condescending, Christian view of Africa). Or I communed with a handsome Santa Gertrudis bull, bound for herd-siring

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adventures on the white farms of South Africa, which occupied a sturdy open crate on deck. Mostly I lived in mounting anxiety over the consternation I must be causing my colleagues. There was no way in those days to get a ship-to-shore message to Mel and Marjorie, awaiting me in remote Maun at the expense of some of their precious last weeks of fieldwork.

When I finally pitched up in Maun, after a long train ride across South Africa and a small-plane hop from the Botswana capital, Gaborone, it was early December. Mel and Marjorie were good sports and began their requested chore of teaching the ropes of Botswana fieldwork to the breathlessly excited new arrival. They'd been in Botswana for two years, had both worked extremely hard, and were ready to return to the US to write up their work. Mel was studying infant behavior among the !Kung or Ju/'hoan San ("Bushmen"), and Marjorie, musical skills acquisition among Ju/'hoan children. I was there to do research on the beliefs and expressive culture of the Ju/'hoansi for my PhD, planning to make a collection of their folktales and oral history and to study their lore and practice of religious healing. I set myself the task of learning the language first, with the goal of eventually not having to use interpreters.

***

As we headed west for the first time out of Maun for Dobe, Marjorie remarked that we would "stop at Carvel for soft ice cream on the way out of town." I stared out the window of the Land Rover. What on earth could she be talking about? Although I quickly realized her joke, I was unprepared for the vast, silent range of sand and scrub we began traversing as soon as the huts of Maun were left behind. After Nokaneng we hit a long empty stretch on the way to Dobe. For 150 kilometers along the deep sand road, there was no petrol and no place to buy food. The only water available came from lowering buckets into hand-dug wells that were few and far between and not very near the road, and there were huge distances between very tiny human settlements. I had heard that anything could go wrong along that road, and that it often did.

Crammed into the hot Land Rover with piles of groceries and supplies, accompanied by !Xuma N!aeba, Mel and Marjorie's Ju/'hoan language teacher and aide-de-camp, who was soon to become my assistant in turn, we ground along slowly in low gear toward the setting sun. Unfamiliar species of bushes passed slowly by the dusty windows, becoming familiar by repetition by the end of the day. When it was time to pitch camp, Mel simply stopped the Land Rover in its tracks in the sandy road. Once the incessant motor noise was cut, the absolute si-

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lence itself became a sound. Shovels and tarps and tents and a cooking tripod came out of the truck. A place was cleared for a fire, and we made supper. Meat from the Maun butcher, green mealies (corn on the cob), some cheese in a plastic box, fresh tomatoes, and tea. Marjorie told me to enjoy the fresh food, as it was the last I would see for some time. On top of the Land Rover were strapped huge burlap sacks, called "pockets," of oranges, another of potatoes, and a smaller one of onions. These, along with canned goods, cornmeal, and dozens of eggs carefully buried in a box of sand so they would stay fresh and not break on the bumpy trip, were our supplies for the few months of fieldwork remaining before we would return to Maun. Then Mel and Marjorie would leave for the US. I would inherit the HKRG Land Rover and provision it myself before returning to the bush.

***

Because "the rabies run," as we came to call it, intervened, we returned to Maun sooner than we had planned. Somehow the water receded a bit during our second sleepless night, and we got the Land Rover out of the molapo at dawn. We could then see to avoid the deepest pits and could bush-bash alternate routes around them. Our progress was agonizingly slow.

At last we reached gravel. We drove at breakneck speed down the few kilometers of roadway leading into Maun just as the forty-eight-hour incubation time specified by *Merck* was ending. By this time the dogs had gone quiet in the back. Were they dead already? Could their brain tissue still be analyzed for rabies if they were in fact dead? We were each wracked by such questions, but we kept them to ourselves. I had already revised my definition of hell to include the inability to communicate, in a critical situation, with others. The fear of rabies was one thing, but not being able to talk about it was a hundred times worse.

We drove straight to the little hospital in Maun and around to the back entrance. Mel and Marjorie knew an expat doctor they thought would help us quickly. Providentially, he was standing in the yard when we screeched to a halt. We jumped out of the Land Rover and began—rather hysterically—to tell our story. The doctor rapidly arranged for the dogs to be killed, and for their brains to be sent off to a lab in Lobatse, near Gaborone, for analysis. Then we watched him through a grimy clinic window as he rummaged in a small, gas-powered refrigerator for rabies inoculation vials. At first he could not find any and, thinking of the minutes still ticking away, we felt our hearts sink yet again. "Here they are!" he finally said, hauling them out from the back

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of the crowded, messy little refrigerator. We asked whether we had arrived in time for the inoculations to be effective. He said if we had not yet begun to have symptoms, we would be fine if we started the shots right away. We lined up and big needles were plunged into each of our diaphragms in turn.

Painful as this was, I threw my hat in the air with relief that we had arrived in time. Then I turned to look at Marjorie and she was glaring at me with a look that froze my bones. "I'm already *having* symptoms!" she choked out. "You're celebrating for yourself but you don't care if I die!" These were the first words she had spoken to me in two days. I was horrified that she begrudged me relief about my life, but equally horrified that I had shown insensitivity to her fear. Having privately believed that her symptoms were psychosomatic and due to extreme stress, I had allowed myself an instant of celebration of my own imminent delivery from danger.

I remember that Mel was notably silent at this juncture. I assumed this meant either that he was showing absolute emotional support for his wife, or that he had been saddled with an impossible situation in a three-person camp in which two women were at odds. Of course, in our fraught little crucible of a social situation we had been unable to talk of any of this. We were the only English speakers, effectively, for hundreds of kilometers in any direction: everything we might have discussed, but couldn't, hung over our heads in magnified, heavy, unavoidable form. I recall these events for a specific reason: the paralysis of our communication at that time foreshadowed many other poignant, painful, but productive "learning experiences" still to come in my life and work.

I spent the next few grim days alone at Riley's Hotel on a bank of the Thamalakane River while we waited for the results on the dogs' brains to come back. Mel and Marjorie stayed with their British friends, ornithologist Peter Jones and his wife, Isla. Each day we trekked from opposite ends of Maun through heavy grey sand to the hospital for another painful shot each, deep into our stomach muscles. Each day my allergy-like reaction to the shots seemed to grow worse. By the third day I thought the shots would kill me before the rabies did.

Finally the lab report came back: it was biliary (distemper), not rabies, and we were all safe. In a few days, we headed back to Dobe. Tentatively, Marjorie and I began speaking again, and we managed to get through our remaining weeks together in some dignity.

In later years there was a full, warm rapprochement between us. Marjorie published the first of two books that became anthropological classics, *Nisa: The Life and Words of a !Kung Woman*. The rapprochement

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was—to her credit—initiated by Marjorie after she had been diagnosed with, and knew she would likely die of, breast cancer. Her *Return to Nisa*, published posthumously, is, just like her first book, used in countless introductory anthropology courses.

The rabies run was a huge and traumatic part of my introduction to fieldwork in Africa. It was the first of a long series of "ill wind" situations in my Kalahari life, each of which ended by blowing me some good. I believe it was the start of my emergence from the cloud of romantic impulses that had gotten me to Botswana in the first place. What I now remember most about my first few months in the field was the enormous help Marjorie Shostak and Mel Konner gave me in shedding romantic impressionism and in learning how to live and work well with the Ju/'hoansi.

***

A few early memories help to set the scenes from which my journey to Africa began. My mother worked as a medical technologist; her health had been compromised before I was born by contracting an infectious disease from a specimen during her lab work. As a baby I was afflicted with multiple allergies and cried inconsolably. Both my parents were working at Cold Spring Harbor Laboratories on Long Island at the time. My mother eventually took me back to her home in Port Arthur, Texas, where she and my grandparents took turns staying up nights to walk with me and sing to me. Nothing availed. But when my grandparents' Cajun housekeeper came into the picture, there was a fundamental change. She added a new dimension as an adjunct family member and caretaker, and effectively took charge of me. In the process I was somehow set on the path to becoming a more comprehending human being.

In retrospect I believe what did it was the simple addition of more unconditional love. Eloise Thibodeaux, whom we knew as Lovina, grew up speaking French in a huge Catholic family of "colored" sharecroppers between Lafayette and Mamou, Louisiana. After it became clear that she could not scrape together a living there during the Depression, she walked alone across the state line bridge into Port Arthur and taught herself to speak English—and to read—with the help of the funny papers. My maternal grandfather was a petroleum chemist who ran the Gulf refinery at Port Arthur. Lovina became my mother's parents' cook and housekeeper, and she remained a fixture of our family until her death more than fifty years later.

The childless Lovina took me on—along with my sisters and all our cousins when they visited—as her own. She was the perfect allomo-

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Figure 1.1. Lovina Thibodeaux. © Diana "Boppy" Burnett.

ther. My earliest memories are of long hours sitting on her lap, holding and turning over her two hardworking, prominently veined hands, and marveling at their softness and gentleness. All of us, I think, felt that her fierce love for us transcended the borders of skin color. I will never forget the physical, tangible force of love and the concomitant tough teaching she lavished on me. She told me that when I had a dirty job to do with a rag, I shouldn't hold it at arm's length while averting my eyes and nose but rather grasp the rag with my whole hand and get in there and get it done. She taught me that, in order to "defend my soul," I should always tell the truth. She also shared with me the beautiful visions of the Virgin Mary that animated her own soul. I never became a conventionally religious person, but the visions Lovina shared opened me to the transcendent phenomena (often associated with religion) that became the real focus of my life and anthropological work.

Lovina also introduced me to the power of ritual. When I was twelve or thirteen, I went through a period of painful headaches. She told me to wash my hair, using a bucket outside on the wooded hillside behind the house, at sunrise three mornings in a row. I did, and the headaches vanished, due to this (probably more African than Catholic) advice. I

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was also gradually introduced to Lovina's close-knit family as some of them joined her in Texas and later, when she and I took trips together back to Louisiana. I was thrilled by the affection and easy acceptance I experienced from her family, the closest to indigenous people I had by then met.

Once, Lovina and I walked into a bar and grill owned by a cousin of hers. The cousin, a small, dark, slender man with jet-black, ringletted hair, rushed to the door to meet us. He missed not a beat as he turned from hugging Lovina to greet the blonde, blue-eyed child standing shyly at her side. "Come on in, girl! You look just like a Thibodeaux!" At their homes, I heard Lovina's family telling stories in French and English from Canadian Acadia. I especially remember their African stories. I began to think that, in search of the warmth and fun I had experienced with the Thibodeaux family, I would sometime go to Africa. This belief was reinforced when, as a child of six or seven, I was bouncing on my parents' bed, over which hung a large map of the world. Africa was in the middle of it. In the center of the southern part of Africa, opposite my eyes about as high as I could bounce, was a vast, empty, terracotta-colored expanse intriguingly labeled "Bech. Prot." Whatever that place was, I resolved to go there one day.

My original impulse to go to Africa was, thus, quite simple, childish, and romantic. But soon, under the influence of my father, a cell biologist and geneticist devoted to natural history and to environmental causes, I began to envision specific ways I could get there. I wanted the means to be involved with whatever career I chose. Through junior high, I thought I would go to Africa as a scientist. Paleontology had particular appeal for me. But then I discovered literature. In high school in Austin, Texas, in the early 60s, I had an English teacher named Mr. John Shelton. He opened up literature to me and my classmates so we saw it was a window on the whole of life. That it was, in fact, life itself.

At the same time, my father was introducing me and my sisters Diana ("Boppy") and Janie to the plants and animals, the fossils and geology of his beloved central Texas, where he had spent most of his life observing and studying natural history. My mother, meanwhile, was encouraging us to enjoy music and art and literature and whatever drew our hearts forward. She, too, was a scientist and beyond that she was a promoter of "continuing education for women," a Betty Friedan feminist, an activist in local causes, and a staunch believer in interdisciplinary approaches to complex problems. The Texas senator Barbara Jordan was an occasional visitor to our house with others of our mother's friends devoted to women's political and educational issues.

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For me there was no question but that I would become an academic and get a PhD. Both my father and *his* father had each taught for many decades at the University of Texas at Austin, so that, for me, seemed the model to follow. (My sisters, both younger, forged more original paths!) In high school and college, I found my interests straddling the institutional divide between the arts and the sciences. I agonized over whether to go into biological sciences like my father, a research scientist and professor, or to follow the lead of my mother, who taught me and my sisters to enjoy music, literature, and the visual arts. I ended up following my heart and declared an English major at the University of Michigan, where I enrolled in 1963.

U of M had an excellent, challenging Honors English program starting with Great Books that carried on right through the edgiest dimensions of contemporary literary criticism. I threw myself into literary life in Ann Arbor, enjoying the artsy cultural discussions that continued seamlessly from talk about books in my afternoon English classes into talk about films while standing in the long evening lines at Cinema Guild. Cold as the U of M campus was, it presented me with something like a full-time outdoor café-society intellectual life, and I loved it. But I continued to feel that my responsibility to the natural world—understanding it and conserving it—would somehow be neglected if I neglected science. I didn't know what to do then about a personal propensity that was, seen from later vantage points, a kind of hyper-responsibility. I thought I should somehow transcend disciplinary splits and do it all.

By my junior year, with the help of some excellent literature professors and their courses, I had been introduced not only to the great written works of Western civilization but to philosophers and critics like Ernst Cassirer, Suzanne Langer, Northrop Frye, and Kenneth Burke. I realized through them that my main interest lay not in literature itself but in its social functions. The capacity of literature and other expressive forms to create communities of agreement and understanding seemed profound to me: it seemed, by forging shared meaning, to be constructing the essence of human life itself. It was much later that I learned about the Sapir-Whorf hypothesis concerning language and its role in the construction of perceived reality. I saw that to understand how literature created separate worlds of agreement for human beings in different times, places, and cultures, I must study its earliest forms—in other words, oral literature. Visualizing human time as a very tall column of sand in a giant glass test tube, only the top few inches of which were characterized by written works, I looked into the fields of

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folklore and oral literature as avenues to comprehending the deep human symbolic past.

One spring break I didn't go home to Austin but stayed in Ann Arbor over the holiday. I had just discovered Marshall McLuhan and was excited about the implications for my work of "medium as message." In the library stacks I found untidy piles of *Verbi-Voco-Visual Explorations*, the Canadian media journal started by McLuhan and Edmund Carpenter, which contained publications by both of them. Dusty and freezing in the cold stacks, I read through them all, that holiday, along with books by Jack Goody, Walter Ong, and other "oralists." I was greatly moved by Edmund Carpenter's article "No Upside Down in Eskimo Art." These discoveries respecting the power of media in the framing and creation of meaning brought me the closest yet to my holistic vision of what I was trying to do. What was that? As I then formulated it, I wanted to know *from the inside* what it felt like to be someone from another culture, walking along in a landscape known intimately, practically, and down to its last detail. I was becoming aware that the only way "inside" was through the media and language used by the people in a culture to communicate to each other and potentially to the wider world.

This formulation of my purpose owed something to a fascination I had found as a child in reading folktales (those by Jacob and Wilhelm Grimm, Hans Christian Andersen, etc.). I read them not only for their adventurous plots but for the details they contained of life in other cultures and places. I remember my excitement at reading, probably in the brothers Grimm, that a man going on a journey took along a walking stick and some bread and cheese that he put into his wallet. "His *wallet*?" I asked myself. "Clearly there are some things I don't know about the world!" I later learned that language use has both history and variants, and that the same words were often used for different things in different times and places. In a related discovery, I learned that many folktale plots are widespread across the world, despite differences in geography and ecology, and I began to ponder the reasons this might be so. I read James Frazer's *The Golden Bough*, Mircea Eliade's *Shamanism*, and Arnold van Gennep, and a long, slow fermentation of constructive ideas began. I was fascinated with the idea of the evolutionary value, to human beings, of narrative, and of tracking it back into prehistory.

Shortly after discovering McLuhan, I took my first course in literary criticism. We read the ritual theorists and Aristotle on the ineluctability of tragedy, and the puzzle of how to approach the social functions of literature and art simply cracked open for me then and there. I found that there were knowable relationships between myth, folklore, and lan-

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guage, and also that there was an academic field open-ended enough to constitute a matrix for all my vaguely interlocking forms of inquiry. It was called anthropology, a word I had not heard until I was a junior in college. My first course in anthropology was with the ethnologist Leslie A. White, who had recently published *Evolution of Culture*.

White's publication year, 1959, was also the year of the centennial of the publication of Charles Darwin's *Origin of Species*. Perhaps not coincidentally, this anniversary helped renew the interest of cultural anthropology, along with many other branches of science, in evolutionary perspectives on its lines of thought. Anthropology, I believed, could bring together for me the worlds of science and the humanities I had thought were separate. I realized that doing justice to my main interests would mean studying social and cultural anthropology, but that, due to disciplinary divisions, I might also need to study several other academic areas, including comparative literature, psychology, and maybe others. I didn't know how I could do that, but I intended to try.

My approach up to this point exhibited my typical overkill, but at the time it seemed to make my path forward clear. It was as if a great moral weight of decision was suddenly taken off my shoulders, and in fact since these junior-year realizations I have never looked back. I jumped into a double major in my senior year at Michigan and graduated in both English and anthropology. I looked around at graduate schools for programs that would allow me to pursue my interests beyond the boundaries of their anthropology departments. I found the perfect situation in Harvard's ad hoc PhD program, which allowed students to custom-tailor their relationships with several departments while centrally pursuing a degree from one of them. I signed up to do a degree in anthropology based at Harvard's Peabody Museum, focusing on social anthropology as one of the subfields (along with archaeology, linguistics, and biological anthropology), in which I would be expected to become competent. I also requested courses and advisors in two other departments, working with Henry A. Murray, a psychologist of personality in the Department of Social Relations, and Albert B. Lord, of the Comparative Literature Department. Lord had collected oral epics in Yugoslavia and cocreated the classic theoretical approach to oral composition and performance with Milman Parry, called the Parry-Lord theory. I was at last, I thought, where I needed to be, to do what I wanted to do.

However, as a former English major, I knew I would have a lot of catching up to do in anthropology. Most of my fellow students had focused on anthropology for the bulk of their undergraduate careers. I also felt, like many students who manage to arrive at Ivy League

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schools, that I did not belong at Harvard. My first year I tiptoed timidly around the Peabody Library, the museum, and my study carrel in the attic, terrified that I might run into one of my professors. A newcomer to many anthropological concepts, I battled with the famously rigorous required core courses, fearing one or more of them would cause me to flunk out of grad school. Somehow, though, I managed. I found structural anthropology and symbolic anthropology courses not only more doable but extremely compelling—they were absolutely germane to my evolving approach to myth and oral literature. More than anything else in my life up to that point, I wanted to understand the social implications of shared symbolism. To figure them out, I knew I had to stay at Harvard by hook or by crook. So I muddled through.

One of the happiest experiences of my Harvard years was becoming a teaching fellow for Albert Lord my second year. I heard about Lord's work from a fellow grad student, Chris Boehm, who had already done fieldwork—in Montenegro. Lord's mentor, Milman Parry, had invented an early sound-recording machine using wax cylinders to capture the sounds of oral performances. With the heavy machine strapped to his back, Lord had carted Parry's wax cylinders up the mountains of Montenegro and Albania in the 1930s, '40s, and '50s so they could record Yugoslav epic poetry. Now in the 1960s he was sharing with a generation of students what his Yugoslav work had taught him about the principles of oral epic composition and performance. I began to see that folklore study, far from being some sort of "stepchild" field of anthropology, was itself full of both rigor and insight. In its focus on analysis of recorded texts, I saw, it could constitute my logical bridge between written and oral literature. These influences started to shape my personal approach to the social function of literature. I began to look for a place to do fieldwork where I could try to understand the expressive culture of people still living exclusively "oral" lives.

I found that place when I took an ethnographic film course taught by Robert Gardner, in which Melvin Konner happened to be a fellow student. When Mel heard about my plan to collect oral texts and look at their social function in some nonscribal group, he told me about the Harvard Kalahari Research Group project with hunting-gathering Bushman groups in Botswana. He also told me I should meet Lorna Marshall and read her classic series of papers on the !Kung of the Kalahari. Lorna, seventy years old at that time, had done the pioneering ethnographic work on the !Kung hunter-gatherers (later called the Ju/'hoansi), transforming herself, after the age of sixty, from an English professor into a professional ethnographic researcher and writer. She and her husband, Laurence, had taken their son and daughter,

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John and Elizabeth, on a series of expeditions to the Kalahari starting in 1950.

At that time the Peabody Museum could not find a single anthropologist interested enough to undertake the arduous journey to study one of the world's last groups of practicing hunter-gatherers. (By the midsixties, of course, Harvard had become an epicenter devoted to what was then called the "human baseline" provided by the Kalahari peoples and other contemporary hunting and gathering societies.) The Marshalls, however, saw the need early and just got up and went, funding their complex and difficult trips for the Peabody themselves. All four of them contributed irreplaceable work to the understanding of Kalahari peoples: Lorna in ethnography, Laurence in still photography, Elizabeth in her enduring, classic book *The Harmless People*, and John in ethnographic film. Their family home, at 4 Bryant Street in Cambridge, was a stone's throw from the Peabody; Lorna wrote for decades in an upstairs office in that house. I made an appointment with her, we drank tea and talked, and after several such visits she took my breath away by asking me to work for her as a research assistant.

My exciting new life was encompassed within a few magic blocks in Cambridge. Divinity Avenue had both the Peabody Museum ("Peeb" to us grad students) and William James Hall, housing the Social Relations Department (known as "Soc. Rel."). One block away (arrived at by a clandestine passageway alongside a faculty childcare playground between university buildings) was Lorna and Laurence Marshall's house on Bryant Street, with John Marshall's film archive in the basement and Laurence Marshall's still photography archive on an upper floor. Albert and Mary Louise Lord lived right around the corner. Lord's classes in oral literature and folklore were held a couple of streets away on the main campus in the Classics Building, with Widener Library, where the Parry-Lord Collection was housed, just beyond. In film class, across the quad from Widener, Mel told me about the fieldwork he planned to do on infant and child behavior in a newly independent country I had never heard of in southern Africa. It was weeks before the penny dropped for me: a little over a decade after I was bouncing on my parents' bed, the Bechuanaland Protectorate had become Botswana.

I wanted to arrive in my eventual place of fieldwork with my eyes wide open, ready for adventures. But a drone note of anxiety about anthropological practice and theory was constantly sounding in my brain. How could I ever square a welter of first impressions and the joy of detailed learning with the scientific rigor entailed by currently accepted methodology and theory? What about my ongoing human responsibilities to the people I would study, once I had gotten to know them? In

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Cambridge my encounters with ideas about cultural evolution, hunter-gatherer studies, and the budding field of "action anthropology" went round and round in my head, generating as-yet-unanswerable questions.

I knew there had been an immense impact on the research consortium I joined, the Harvard Kalahari Research Group, by the Man the Hunter Conference that took place in Chicago in 1966. My two professors Richard Lee and Irven DeVore, along with others who took part in Man the Hunter, established the field of hunter-gatherer studies and oriented it in approaches related to both cultural evolutionism and primate studies. So, before and at the start of my fieldwork, I found myself turning into a pretzel trying to fit my ideas about the social function of literature with cultural evolutionary thought in anthropology. This was a nerve-wracking process for a former English major. In the end it was a worthwhile intellectual exercise, landing me somewhere between cultural ecology and an always-questioned "symbolic" fringe area in hunter-gatherer studies. But it involved me very early in wondering whether I could do what Harvard wanted me to do for a PhD and keep true to myself in the process.

I had long been aware that I was a very thin-skinned person. Some said I was hypersensitive, but I knew deep down that empathy was not only a core value to me but one that in any case I seemed to have little choice but to honor. In the pages of my prefieldwork journals, empathy and sympathy vied strongly with intellection for how to know, and how to be with, people in the world. These traits had loomed large, along with intellectual reasons, in my rationale for choosing anthropology. Written anthropological works, especially those by consummate craftspersons of the written word like Lorna Marshall, seemed exciting and filled with human insight. I was thrilled, for instance, with Lorna's careful, articulate prose about the delicate social balance kept by the !Kung hunter-gatherers' attention to unwritten social rules. My fellow student at the time, Sarah Blaffer (Hrdy), drew my attention to the !Kung's "exquisite etiquette," which the pioneering Lorna Marshall had been first to bring into the ken of anthropology. *The Harmless People*, the evocative book on the Marshall expeditions of the 1950s by Elizabeth Marshall Thomas, also inspired me as a fine example of sensitive observation and writing. Elizabeth wrote as empathetically on the Kalahari animals as she did on the people, increasing the richness of her anthropological observations by including the people's social relationships to the animals and plants of their environment.

Yet in my graduate courses I was being taught a model for how to "do" anthropology that seemed almost mechanistic to me: make a

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hypothesis, construct a questionnaire, make numerical observations, crunch the numbers, and either prove or disprove the hypothesis. Where would be the informant individuals in this schema, and where would I be in relationship to them? I couldn't envision myself following people around, watching them, and writing things down about them, knowing that I would be distorting their behavior in the process. It seemed an unequal, and therefore unfair, set of activities. Others might carve out scientific relationships to people as data, I thought, but I promised myself I would always insist on absolute mutuality in all human dealings, especially those with informants, from whom I already felt privileged to be able to learn. Like all absolutes, of course, this ideal was romantic and unrealistic, but I had no idea—yet—just how very much it was so. Nor did I reckon with my eventual informants' insistence, in their turn, on mutuality with *me*—on their own terms.

I was also unrealistic, by academic standards at the time, about how interdisciplinary a thesis I could write and have it accepted. When I submitted my plan for my PhD and fieldwork, I tried to bring together all the relevant paradigms, as well as all the fields of my interest, into what I thought was an innovative, comprehensive "ad hoc" design. I felt that creating an imaginative, innovative, and highly personal plan was exactly what I was at Harvard to do. Only gradually did I see that some of my fellow students—especially the female ones—felt pressure to hitch their plans to a specific professor's wagon in order to ensure a successful graduate career. I was shocked when I realized some of them had arrived at Harvard with ideas as rosy as mine about what they wanted to do, but that they ended up adopting subordinate topics that burnished their professors' academic and research careers instead of their own. I was determined not to capitulate in that way, but as a newcomer to the field of anthropology, I had to rebuild my confidence in my own vision over and over. I felt I had to make double the effort to prove the validity of what I wanted to do.

Aware that my plan was ambitious and perhaps overextended, I pursued it anyway. I was also fully aware that I would have to cover the main anthropological subfields simultaneously for my qualifying and general exams. For a couple of years I felt I couldn't really tell the left hand (which held my anthropology department commitments) what the right hand (which held my oral literature, literary criticism, and psychology involvements) was doing, and vice versa.

This schizy, somewhat grandiose situation was due partly to my not yet having achieved a cohesive vision of what I was doing, and partly to academic politics and disciplinary splits. However, despite the ambiguities of this period I knew I was on the best track for me personally.

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Before I left for the field, I trusted that my linking solution would be found embodied in the people with whom I would eventually do fieldwork—in the way they solved the problems of living with each other and within their environment, and in how they felt and talked about these things. I felt this expressive dimension of life was one that had not yet been addressed as fully as it might in the emerging field of hunter-gatherer studies pioneered by Lee, DeVore, and their colleagues. I was hoping, in other words, that I could bring the right and left hands of my studies together by meeting the specific people studied by the Botswana project and experiencing the importance of expression and communication in their hunting and gathering adaptation.

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In the end, I think I was able to do something very like that. In the chapters of this book I explain how experiences I had in those first months of fieldwork set the stage for my lifetime intention. I have spent the ensuing half century advocating on behalf of the Ju/'hoansi and other San peoples, largely to foster awareness of the intricate symbolic mechanisms of their egalitarian culture. Wanting to make the expressive art and lessons of the Ju/'hoansi more available to the world beyond the Kalahari, I have mostly tried to use, not paraphrases or my own interpretations, but careful transcriptions and translations of their own recorded voices. I wanted to bring readers closer to both the clarity of Ju/'hoan thought and to the mystery, especially in the healing dance, of what the Ju/'hoansi themselves say is beyond knowing.

I believe I was enabled to do this by the months of fieldwork chronicled in this book. I had the chance to live closely with a group of people who, like their ancestors, thought deeply and acted expressively about the best way to live their lives in the environment that surrounded them. People whose survival depended not only on rigorous sharing but on functioning as a creative, intercommunicating collective. People whose insistence on the equitable sharing of resources was endlessly reinforced by communal artistic and spiritual activities. People whose storytelling created a world of unique agreement among them, a world that was adaptive and functional and enfranchised the imagination of every single individual as part of their ongoing, ever-changing tradition. I learned the lessons I did about living oral and ritual tradition by swimming in the ceaseless tide of energetic variants generated by the several hundred Ju/'hoan-speakers—and their storytellers, musicians, singers, and dancers—with whom I came into contact in that first, eighteen-month, slice of time.

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My time in Botswana, immersed in the day-to-day context of Ju/'hoan existence, seeking out and easily finding my own "singers of tales," allowed me to bring together evolutionary perspectives with specific ways that storytelling and other arts are enabling for society. I eventually saw that, as in biological evolution, each story, each idea, is a random existential experiment. Stories that work, that enable social cohesion, are among the most important intellectual achievements of humankind. The stories the Ju/'hoansi told themselves over and over about sharing, social cohesion, and the very possibilities of transformation and healing constitute, by constant renewal, the powerful engine that made it possible for them to get along with each other. They told and danced and sang the stories, incorporating the flashes of insight that occurred to different individuals in the process. Stories lived in their never-ceasing variants, born of the involvement of each participant, of each new generation. Their energy, in a very real sense, was drawn from the storytellers and their listeners, the singers and dancers, each time the creative community gathered to celebrate life and challenge death.

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So I knew intuitively that I would find links between cultural expression and cultural adaptation to environment. But it took me a lot of time to convince Irven DeVore that my thesis work would add value to the Harvard Kalahari Research Group. By the late sixties the HKRG had been in operation for more than five years, and it was well-launched in the direction of classic and adaptationist hunter-gatherer studies, with projects ranging from an input-output analysis of !Kung Bushman subsistence and how it was procured, to child and maternal behavior, to archaeological analysis of the material culture of the forebears of the !Kung in the same region—which they had continuously occupied for many tens of thousands of years. The idea that studying the verbal art and other expressive forms of the !Kung (Ju/'hoansi) could contribute materially to an understanding of their society was a hard sell at the time. Mythology and folklore were still seen, as I have said, as "stepchildren" or cultural froufrou by much of anthropology at the time. Though folklore as a field was already on its way to becoming extremely highly regarded at many world universities, ethnographies we were exposed to at Harvard in the late 1960s often relegated expressive culture, myth, and folklore to a final and epiphenomenal "chapter 10." I was on fire to change all that.

Richard Lee was in Canada for the critical semester. So I knew it was up to Irven DeVore to admit me to the project. I tried in vain to

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catch him at his office hours. I made appointments with him that he failed to keep. I wrote screeds expanding on what my ideas were and why I thought they would be productive even though they were revolutionary in anthropology at the time (though not so revolutionary, I later learned, in literary criticism, feminist writing, and other fields that were so formative in the enterprise of cultural critique!). I eventually resorted to pushing brief descriptions of my fieldwork hopes and dissertation plans under DeVore's office door. I'm sure I made a pest of myself but I was also absolutely certain that this was what I needed to do and that my research perspective could make a worthwhile contribution to hunter-gatherer studies.

I had a strong feeling that my interdisciplinary approach to the adaptive value of narrative could help to place narrative within the cultural evolution paradigm. Too bad I did not know then what Richard Lee's teacher Sherwood Washburn, perhaps the main founder of US primate studies, who also influenced hunter-gatherer studies, had already said in 1951. In his essay "The New Physical Anthropology," Washburn had made a clarion call for using a multidisciplinary approach to human evolution, because evolution itself was multidisciplinary! Luckily, in the end I was written into the big National Institutes of Mental Health grant Lee and DeVore were applying for at the time. I was to be the last of their graduate students to work at the Harvard camp at Dobe, Botswana. I was launched. I congratulated myself on having made my point. Yet years later, DeVore chuckled as he told me why he had finally instructed Lee to admit me to the project: "You were so damn persistent I just decided you'd probably make a good field-worker!"

Membership in the small, special group of Lee and DeVore's graduate students has, even beyond academic concerns, profoundly influenced my life and continues even now to do so. Humanitarian impulses eventually led me and my HKRG colleagues toward activist work on behalf of the community we studied. Together we founded the Kalahari Peoples Fund, at this writing in its fiftieth year as an active and well-regarded US 501(c)(3) nonprofit. I should point out that I did not set out to become involved in "action anthropology" or "development anthropology." When I started my career I cared most about how expressive forms held a society—through its individuals—together. I was becoming more and more convinced that it was symbolic reinforcement via multiple forms of cultural media—everything from rock engravings and paintings to songs, oral poetry, and folklore—that cemented understandings in a society so that diverse individuals could coexist and get on with the business of group survival. I realized dimly that narrative forms were somehow of vast importance in this mix. Though the

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idea that we're "hardwired for narration" is commonplace by now, it was an intriguing new idea for anthropology at the time. It took me some time to realize I might show how narration helps us understand the hunting-gathering adaptation by studying it in a specific context.

At the same time, I was striving to incorporate into my theoretical purview the realizations about knowledge, memory, and creativity that came of my interdisciplinary approach to the importance of expressive forms. As I've said, in grad school I was catapulted into theoretical trends, issues, and debates. Harvard at the time followed British functionalist anthropology in speaking of "social" (rather than "cultural") anthropology. But the Harvard Kalahari Research Group's adaptationist paradigm postdated and challenged British functionalism. Due to my recent switch from English literature to anthropology, all such theoretical stances were new to me, and I struggled to find a way to make my work comprehensible within them. I made an uneasy truce in my mind by adopting the current labeling of all social practices as "adaptations," a concept familiar to me from biology. Meanwhile I went ahead with my grander plan based on the strong visceral conviction that the synthesis I intended actually existed on a different level from competing theories. I thought this level was still to be discovered by me and/or others, and I intended to give that a try. (If theories were competing, I reasoned, wasn't reality still in question?) I was heartened by this notion.

I also remained deeply stirred by the insights coming from hunter-gatherer studies that were developing institutionally right under my nose at Harvard. Though because of the ambitious interdisciplinary path I had charted for myself I didn't manage to actually enroll in any of the large and popular hunter-gatherer courses taught by DeVore and others, I was getting the basics of these courses practically by osmosis from fellow students with whom I hung out. The powerful data and insights presented in these courses foreshadowed the great themes of folklore and the social organization of hunter-gatherers that still animate my life, especially the strengths of their sharing and their social egalitarianism.

What, I realized later, I *didn't* get from my time in grad school was any real sense of what doing fieldwork with hunter-gatherers, or "former foragers"—or in fact with anybody—would actually be like. In particular it is ironic that, sitting in a well-funded, internationally acknowledged center of research on the Ju/'hoan San ("Bushman") people, many of whom were still living by hunting and gathering, a center with access to huge files of photographs and comprehensive bodies of ethnographic film and sound recordings, I managed to get the idea that

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doing fieldwork with the Ju/'hoansi in Botswana would be a quiet, calm experience. I guess I was expecting my time with them to be one of principled "participatory" observation, but observation nevertheless, experiencing the peaceful coexistence, selfless sharing, and mutual caring of people in a well-oiled social machinery that had stood the test of time. I never in a million years expected the confusing din, the unfulfillable demands, and the puzzling contradictions that met me on arrival—and that stayed with me, off and on, for the next eighteen months (not to mention for years thereafter)!

In chapter 2, I present a simulacrum of the welter of chaotic impressions that hit me in my first days in Botswana. Even here, many major themes of that first fieldwork start to emerge: how to cope with the physical and social demands of being there; issues of sharing, giving, and remuneration; and my first healing dance and the importance the healing beliefs began to take on as I planned my work. That I am able to access now both unvarnished first impressions and the process by which the larger themes developed is largely due to one of my professors at Harvard, Dr. Cora Du Bois. I never forgot her injunction to me and fellow students embarking on fieldwork to write down our impressions "before the scale of custom forms upon your eyes." Thanks to her, I can call on nineteen notebooks full of such observations—and of notes about how I scrambled to keep my head above water during my revealing, but often excruciating, field experience. These raw, untidied anecdotes reflect what were for me a necessary precursor to whatever understanding I finally achieved from that experience—not only about the Ju/'hoansi but about myself.

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