

Chapter Two

The Gift of the Monkey Who Danced into Oblivion and the One Dressed in a Cage

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In 1970 I was poised to graduate with a doctoral degree in psychology and had begun the exciting process of searching for my first academic job. My mentors encouraged, or rather directed, me to respond to a request for an individual trained to conduct biomedical and behavioral research with animals with particular expertise in primatology. I was invited for an interview. Early in that process I was taken on a tour of the existing laboratories and animal facilities. As the department chair and I quickly walked through the rooms, we came upon an obese male rhesus monkey living by himself in an empty room in a single cage so small that it barely permitted him to turn around. Later that evening, I recalled another experience that involved a neglected monkey when I was an undergraduate student that tested my ability to tolerate rough treatment toward animals. What follows is a history of my attempt to create and maintain a state of ethical indifference as the recollection of the ordeals of those monkeys, and many others, continued to impose themselves on my life.

THE LURE OF THE RESEARCH IMPERATIVE

It is fair to say that most of the people who work in biomedical and behavioral laboratories that involve the invasive use of animals as research subjects would agree that they were not initially prepared to interact with those animals in some of the ways necessitated by their research. Learning to accept housing subjects in small life-limiting cages; drawing blood samples from eye sockets, tails, or jugular veins; withholding food and water intake when called for by a protocol; purposely attempting to create living models of distressing human health-related disabilities; and killing animals at a time

dictated by researcher necessity and convenience did not come naturally or easily.

This unease is no surprise given that the principle of nonmaleficence, or nonharming, is probably the most widely accepted ethical norm threaded throughout the world's cultures and is variably and increasingly being extended to human interactions with animals. Hundreds of studies of child development have found consistent evidence that interactions with animals in the home and in early school settings meaningfully contribute to teaching children about the basic progression of birth, growth, and the reality of death. Living with animals can teach what it means to nurture and be responsible for a life made totally dependent on the quality of human care. Many popular children's stories and fairy tales also describe intimate and magical interactions with all sorts of animals, though almost always the animals are portrayed from the perspective of the human being who is typically the dominant and controlling focus of the interactions. These experiences are all highly relevant to the process of developing a child's appreciation of the diversity of life and of moral obligations to other beings not like them.

When first encountering many of the currently accepted methods of animal-dependent science that require a transition from caretaker and companion to a purposeful life manipulator, many researchers experience a strong reluctance, at the very least. I have seen supervisor responses to this conflict ranging from the expression of understanding and sensitive respect and encouragement to the military boot camp style of "quit complaining" or the threatening accusation that a stubborn reticence to let go of the repugnance may indicate the presence of a fatal flaw that will require a change in professional direction. In general, a modification of attitude requires accepting, at some level: (1) the inherent priority of human interests and desires over those of nonhumans, (2) a belief that many of the hardships of human life are not an existential given and can be made eventually to succumb to experimental discovery, (3) that time is of the essence, and finally, (4) belief that our teachers are trustworthy models of what constitutes valid scientific practice. This last factor is particularly important as many of a student's initial laboratory experiences begin by being placed into ongoing research activities that may require the acquisition of invasive animal handling techniques before a thorough grounding of his or her justification has taken place. At this point, the excitement of participation, the emphasis on the importance of the pace of discovery, social influence by colleagues, and the almost automatic process of desensitization will all normally help to facilitate the student's continued engagement and reduce initial disquiet and ambivalence. In other words, in the practical setting of the initial stages of learning to conduct animal research, the actual context can work against buttressing a student's natural

and necessary caution and skepticism about the justification of harming in the name of science.

It is well known, but less concretely appreciated, that the leap of faith that students take in assuming the thoughtfulness, clarity, and expertise of our research teachers involves risk. The simplistic view that the activities of a trained scientist are completely objective and totally data driven was already in question at the time when Francis Bacon (1620) published his book *The Novum Organon* (The New Method), which promoted the power of direct experimentation. For example, Bacon cautioned that overly harsh manipulations during the conduct of experimental observation could distort the generalizability of the relationships observed.¹ Bacon arrived at this conclusion by applying the common sense derived from his experience as a jurist in the legal system that showed him that tortured witnesses most often provided invalid and unreliable testimony. Bernard Mandeville, the eighteenth-century physician, poet, and philosopher, added to the discussion about the consequences of harming nature. In his book *The Fable of the Bees*, he points out that doing harm to God's creatures can affect the presence of tenderness in the actor and therefore his or her character.² As evidence, he makes reference to English law, or perhaps the respected tradition, of excusing surgeons and butchers from serving on a jury. Both were men of blood and had hearts of adamant stone.

Sociologists have since that time elaborated dimensions of social influence that can motivate and at the same time interfere with scientific progress. Daniel Callahan refers to the "research imperative" that derives from the natural desire of humans to know and understand the world in which they live; this same imperative can become a force that is voracious and self-reinforcing with some important negative shadows.³ He lays out the following continuum:

- the research imperative as the drive to gain scientific knowledge for its own sake (for example, to understand the human genome);
- the research imperative as a felt moral obligation to relieve pain and suffering (for example, to find a cure for cancer);
- the research imperative as a rationale for pursuing research goals that are of doubtful human value or potentially harmful (for example, some would argue, research to achieve human cloning);
- the research imperative as a public relations tool to justify the making of a good living and the pursuit of profit (for example, the pharmaceutical industry's defense of high drug prices); and
- the research imperative as the pursuit of worthy goals even at the risk of compromising important moral and social values (for example, hazardous research on competent human beings without their informed consent).⁴

In another vein, Ludwik Fleck's path-finding 1935 book *The Genesis and Development of a Scientific Fact* introduced the concepts of "thought styles" and "thought collectives" to describe the ways in which groups of scientists can get prematurely organized into competing systems of belief that influence not only what member scientists think, what they accept and reject as facts, and the methods they use, but even what they are likely to see during experimental observations.⁵ Similarly, Thomas Kuhn described in his crucial treatise *The Structure of Scientific Revolutions* a consistent pattern found in the history of science: after developing an initial seemingly acceptable explanatory paradigm about some phenomenon, the discipline resists the relevance of data that challenge the accepted system. This pattern was so common that he called it "normal science."⁶

As an example, consider the army of research scholars who have looked at a singly housed mouse in a regulatory compliant laboratory cage that measures roughly six by six by five inches and whose floor space is approximately 280,000 measurement units smaller than the area of the species' typical movement pattern in the wild, and yet have called it adequate.⁷ Or consider the case of a highly social and cognitively complex adult chimpanzee weighing over 120 pounds living alone in a cage measuring five by five by seven feet involved in an infectious disease study. The enclosure provides no access to intimate socialization with conspecifics, very limited opportunities for exercise and environmental exploration, and is empty of nesting material that is an essential environmental ingredient for rest and sleep in the wild, and yet it is concluded that welfare needs are minimally satisfied. Such conclusions do not emanate from hostile personality disorders and the desire to cause harm in animals. Rather, it is an unquestioning acceptance of the traditional methods of nonhuman primate laboratory treatment held by many researchers motivated by a valid concern (that is, in Fleck's terms, a "thought collective"). Then consider the range of medical disorders that can affect caged animals like nonhuman primates: stereotyped behavior, acts of bizarre self-injury, lock jaw, stress-induced dysenteries, cage paralysis, gastrointestinal bloat, all of which clearly develop primarily from the state of confinement.⁸ Nonetheless, these conditions were seen for many years not as prima facie evidence that the living arrangements were a disastrous force able to transform the mental stability and physiological condition of a normally resilient species. Instead dealing with these emergent pathologies was either transformed into another unique opportunity to study them in their own right or was just the cost of doing biomedical science with nonhuman primates. To use a phrase that Francis Bacon might sanction, nature was tortured and the relationships of scientific interest might well be distorted and consequently of questionable value. Or perhaps a better analogy is the Roman historian

Tacitus's description of the empire's approach to war: "They create a desert and call it peace."

These issues are not for me just a meditation on important historical, conceptual, and methodological problems. Instead the issues highlight the process whereby a person working as a scientist develops a moral identity when interacting with the Other. Which forms of attentiveness must be cherished and always accompany the practice of a discipline's activities, and which are the ones that may be placed safely and parenthetically behind the exigencies of the presumed requirements of the project at hand for a limited time period and then brought forward again into the ethical analysis? What are the costs to science and the scientist of too easily increasing and then maintaining the critical distance and overriding the norm of nonmaleficence?

THE DEVELOPMENT OF AN ANIMAL RESEARCH ATTITUDE

Like all families, mine contained members whose lives illustrated how indifferent accident, exposure to unknown risk, and chronic disease could come to burden the lives of the vital and the elderly. If there was anything at all unique about my experience it was that it took place very much at close range. My family and maternal grandparents lived in a small space that provided little in the way of privacy to either camouflage a malady or to avert one's eyes from the painful manifestations. What was also in clear view was the relative lameness of clinical medicine in the mid-twentieth century to help in matters of brain disease, depression, and trauma. However, one dimension of our family's values that helped to keep the balance of hope in place was our collective desire to rescue stray dogs and try to change their lives for the better. We cooperated in feeding the winter urban wildlife and learned to express kindness to the rarely encountered working animals like the ragman's old horse who showed up in our city neighborhood each week. I know it is true that the veterinarian bill was paid before my grandfather's cardiologist. After all, the vet could give the gift of final relief from unremitting distress and pain.

While as a high school student I easily recognized that my older sister Caroline had gotten the major cut of IQ points and could mobilize academic focus whenever she wanted, but her interest was in the piano and music education rather than in the sciences and the vague promise of medical research. So after resisting, I decided to pursue education in the clinical professions in hopes of perhaps adding something of value to the treatment modalities I had seen that were so limited. In the 1960s I found myself pulled toward the field of psychology. As a relatively young scientific discipline, it was still struggling for credibility as a science but was at the same time full of itself

in predicting its soon-to-be achieved centrality. Evidence-based methods of behavior control combined with the findings of adventurous studies in neuroscience were promising to take over the clinical world of what then was seen as Freudian mental apparitions. Even Harvard's B. F. Skinner confidently imagined applying psychological principles to the redesign of culture itself.⁹ In response, some of the discipline's acolytes started communes like Twin Oaks to test the possibilities. Twin Oaks was founded in 1967 in rural Virginia and has been actively maintained until the present.

I found that the way into this world of dangerous ideas and the development of effective futuristic interventions was to volunteer whatever time I had available, and some that I did not have, to an aggressively active research-oriented professor. I found such an opportunity and was soon thrust into responsibilities dealing with human subjects participating in studies of memory, and an array of animal species, mostly rodents, studying the effects of brain lesions on learning and motivation. My fellow lab assistants and I, and they were all exclusively male fellows, worked late into the night, dizzy with the excitement of possible discovery and the special status we had inherited while becoming hands-on researchers. We were given keys to locked laboratory doors, access to cabinets containing injectable anesthetics, scalpels, drugs, drills, brain electrodes, bone wax, and antibiotics. We presented late night entry passes to campus police officers, and we had nonclassroom spaces to drink coffee and argue theory, experimental design, and to keep one another on task.

Soon, however, my sleeping reticence to harm animals for any purpose was awakened in a very important episode whose detail is remarkably clear even fifty years later. I was assigned to assist in the killing of an old dark-haired Stumptailed monkey who had been sitting alone and unused in an expanded metal cage in the animal facility. Some of my coworkers with more experience had already made a partial transition from seeing animals as personal and unique individuals to seeing only scientific objects. At least that was how they appeared to me. Or perhaps, like me, they were just acting the part. On this particular occasion it was explained that our supervising professor needed a primate brain for a class demonstration. He had no further plans to use this particular monkey in future studies so sacrificing him for this purpose would also reduce costs and free up cage space. What was required was that we anesthetize the monkey into unconsciousness, open (that is, "crack") his chest, expose the heart, and then fixate the brain by pushing a Formalin solution throughout his body by using his own beating heart as the pump. Finally, we were to remove (that is, "shuck") the brain from the skull, being careful not to damage the tissue, leaving a perfect specimen for the students to see up close and touch. The justification was that this prop would add somehow

to the educative process in excess of that gained by just viewing even excellent and accurate illustrations in neuroanatomy textbooks or plastic models. I wonder now whether some student ever asked the professor how he came to have a monkey brain in his hand and if he would have told them about the animal and the process of acquisition. Might there have been objections? How would they have been handled?

The senior member of the lab was a graduate student named David who was a very serious person but terribly overworked. He was tall and too thin. He was in the process of completing his master's degree research and facing a very short deadline. He seemed annoyed to have to do yet something else that would not foster his degree progress. However, he was the only person in the lab who had experience doing transcatheter perfusions, so there were no other personnel options.

We met at night after our classes and laboratory duties were completed. None of the three of us assigned this task had worked with this monkey before save giving him fruit treats from time to time. He had a very dark patchy coat, a red-speckled face, a captivating and not unfriendly way of looking at you, large canine teeth, and weighed about fifty pounds. In general, he had a calm, even gentle, demeanor and spent most of his time either pacing in small circles, grooming his arms, legs, and the tops of his shoulders, or sleeping. On regular occasions I had seen him invite lab technicians to groom him by placing his chest against the front of his cage with his arms spread wide apart and his chin and face pointing toward the top of the cage. He had a percussive deep bark/grunt that was heard mostly when he saw his morning and afternoon meals about to be delivered. Tonight he knew immediately that his lonely life was about to be altered as Dave entered his room carrying a large catching net with a long aluminum pole handle. After spotting the net the nameless monkey immediately initiated a series of frantic behaviors. He rapidly circled in his cage, his jaws chattering, while his lips were drawn back into a frightened grin. He would then stop and bend over at the waist with his butt raised and prominent round ischial callosities pointed in our direction. He would then continue the circling and barking but in a higher and more disturbing pitch than before. I do not recall if any of us recognized that the bent over posture was the monkey signaling submissiveness, a type of pleading not to hurt him. Dave had one of us open the cage's front entry door so that he could wedge the net through. Once the net was in place, he worked to trap the monkey to one side of the cage. I was wearing a pair of specially made thick leather catching gloves and was directed to press his head and chest into the floor and then slide around and grab his arms from behind, pinning his elbows together. Frankly, I felt like I was in an unfair street fight with a tough but tiny and hopelessly outnumbered little boy. Once trapped and pinned, the

third combatant stuck the monkey in his thigh with a syringe that had been preloaded with the drug Pentobarbital. We remained in this awkward knot of human and monkey arms, forearms, and elbows until he started to relax his resistance. We released our hold slowly when it appeared that the monkey could no longer advance toward us or wrestle his way out of the cage. The physical endurance and amazing strength that the monkey displayed was celebrated by all involved. In a strange way this allowed us to be less embarrassed about our bullying accomplishment. The feeling was not quite “he had it coming” and more like “we had no choice and we did it!”

Carrying out the remainder of the required steps of the perfusion was something like cutting up a large tough whole turkey in preparation for a dinner. The difference was that there was blood everywhere and the internal space we were observing was less familiar. With the heart visible, Dave inserted a needled tube into the heart and started the flow of Formalin. What is still highlighted in my memory was the monkey’s trembling that began as the Formalin began to circulate through his body. When I first saw it I was startled and withdrew from the splayed chest, thinking that the monkey was waking up in the midst of our taking his brain. Dave saw my reaction and assured me that what I was seeing was called a “Formalin dance” and nothing whatsoever related to the monkey’s consciousness. It was just a biochemical reaction. I regained my control and continued to participate. Later that night I began to wonder about the dance. It was a dance taking place just as the monkey’s tissues were heading to a kind of immortality, but an immortality spent in a glass specimen jar sitting on a laboratory shelf probably for longer than what would have been the length of his natural life. Could this be a kind of positive trade-off for the lonely life, the wrestling matches, and the appropriation of his brain? Thinking that way offered me some momentary relief. This creative reasoning reveals, I think, the lengths my mind was going to in an attempt to reduce the repugnance of watching the ease with which “might makes right” fills an ethical vacuum. Susan Sontag was correct when she said that “shock had term limits.”¹⁰

I should add here that Professor Marion Diamond, a highly acclaimed neuroscientist at the University of California, Berkeley, was known to carry a whole human brain in a decoratively adorned women’s hat box to undergraduate lectures about the central nervous system. She would make a show of opening the lid, putting on latex gloves, and then hoisting the brain out of the box to scattered gasps from the audience. I always wondered whether the person or family who donated that brain to her would have approved of this stunt. Professor Diamond died in 2017.

HOW DEEP WAS MY TRANSITION?

Soon after this episode I graduated, and thanks to friends and colleagues, I was admitted to the graduate program in psychology at the University of Wisconsin, Madison, where I worked at the Department of Psychology Primate Laboratory and the Wisconsin Regional Primate Research Center. The director of these laboratories was Harry F. Harlow, who was highly regarded as a scientist for his stunning work on learning, brain function, and social attachment in nonhuman primates. My first major advisor was Gene P. Sackett, who was devoted to unraveling the nature and nurture mechanisms responsible for behavioral development. He was interested in my exploring the effects of early deprived or enriched environments on the learning capacity of adult rhesus monkeys. He made it clear to me that my approach to assessing this basic question was my responsibility.

Consequently, I was quickly exposed to how Sackett produced the differentially reared experimental animals for this research. His method, like others at the lab, began with separating infant monkeys from their mothers shortly after birth and then placing those animals in deprived environments in bare chambers where they lived alone until approximately nine months of age. At that point, testing began and the findings would be compared to the results of monkeys who were raised with their mothers and peers during the same period of time. To look through a viewer and see the actual infants enclosed in barren social isolation chambers with twenty-four-hour light and continuous white noise to block stray animal sounds was quite aversive if I allowed myself to see the world from the perspective of the baby monkey. But shifting the focus to the research questions at hand turned the experience into one of looking into a phase of the data collection that required distance and objective viewing. My laboratory experiences as an undergraduate research assistant had altered my natural sensibilities.

Obviously, I was already well on the way to committing my focus to this type of research paradigm. After all, Professor Harlow's work was referenced in every college introductory psychology textbook and he had been awarded the National Medal of Science.¹¹ It was assumed by many that the Nobel Prize was next. Professor Sackett had what seemed to be an unending list of publications¹² and a considerable flow of grant money at his disposal. His requests for financial support were consistently judged to be cutting edge and important by his peers. His level of energy exceeded his resources and he thought I was worthy of being involved. My confidence in him was solid. This is what it meant to be part of a rigorous research program. Progress required taking risks, and maintaining a distanced, steady heart helped.

On another plane, animals in the lab were the recipients of highly competent veterinary care and many simple displays of kindness and respect. Certain animals were even singled out for protected pet status and became a focus for compassionate expression by some staff and students. Sloppy and careless researchers were shamed through rumor, innuendo, and passive social rejection. The occasional newly arrived student who left suddenly did so without shared explanation. My self-serving assumption was that they found themselves insufficiently prepared. There just couldn't be anything amiss with the program. Here the monkey was totally instrumentalized to the service of research. For the most part, the lab was not about nonhuman primates, it was about how humans likely functioned and how their early environments and opportunities to make and maintain attachments influenced the dimensions of their psychological and biological makeup. As for the ethical foundation of the work, it was quite straightforward. If an experimental issue was judged to be scientifically important and could not be studied in humans due to concerns about informed consent or the degree of possible and real harm, that fact gave ready license to do the work with animals. It was as though monkeys were replacement humans but were protected only by individual acts of researcher restraint and caretaker charity.¹³ Early in the 1970s the possibility of negative public reaction that might interfere with the availability of resources started to be vaguely mentioned but did not influence plans as far as I could tell.

STAYING QUIET, BLIND, AND DUMB

My graduation came sooner than I had planned. My advisors, Professor Harlow and Professor John W. Davenport, were anxious for me to apply for a position at the University of New Mexico in Albuquerque that had just opened up. The new chair of that Psychology Department, Frank A. Logan, was a colleague of theirs and he had come to them to see if there was anyone under their mentorship who might be ready to apply. It was their opinion that I was ready. I was not quite sure, but I followed direction and applied.

When I arrived for my interview, it was made clear to me that the job was mine to lose. The disgraceful "old boy" employment network was at work and I was benefiting from it. Professor Logan liked my research and educational pedigree and he especially wanted to start a state-of-the-art primate facility within the next several years. He asked if I could do that and I answered with an emphatic yes. This plan would coincide with the opening of the department's new building that was about to go under construction and that would take about two years to complete. Assuming correctly that I would take

the New Mexico job if I was offered it, Logan wanted to show me where my temporary office and lab would be until the building was complete. He said I would share his facilities for the time being. We walked to his laboratory space that was housed in a World War II temporary building on the western edge of the campus.

The building was numbered B-1 below the army green eaves by the entrance. He was clearly proud as we walked through the various rooms while he showed me his collection of rats, quail, and capuchin and rhesus monkeys housed in the old rooms of wood construction. The facilities were primitive by any standard. The rooms were dusty and unkempt and the cages were dirty. Mini stalactites of congealed urine, feces, and hair hung down under many of the rat cages. The waste pans under the monkey cages were full and overwhelmed the absorption ability of the sawdust with which they were filled. The rooms did not smell of animal life but of decay. Logan seemed completely oblivious to these conditions. As we walked into a long narrow room, a single large monkey cage stood in the middle of the otherwise empty room. Inside the cage that was built for an adolescent less than half his size was an adult male rhesus monkey. In addition to being large by natural structure, he was also incredibly obese. A layer of pendulous abdominal fat hung off his body like large tan-colored sack. As we approached the cage the monkey began to spin while looking up, his chin pointing at the ceiling. He flashed his large canine teeth at us while making “cracking” sounds in his throat. He was very angry. This odd movement was necessitated by the fact that he virtually wore the cage like a wire mesh suit. It was actually the only agitated move he was able to make given the interior space of the cage. I recognized the open cuts on his upper arms as scars from self-mutilation. I was certain this animal had been living in this situation for quite a while. The scene was horrific. Why would anyone consciously maintain a complex nonhuman primate in a situation like this? My mind actively tried to find a justification. My first thought was that housing a monkey like this must be part of an experiment. Surely someone was studying the effects of limited space or obesity on some psychological variable. When that suggestion was denied by Dr. Logan, I was lost. I could not get any distance. My host was a famous scientist devoted to expanding the scientific credibility of experimental psychology. He had left a tenured professorship at Yale University to bring modern leadership to this developing department and he was not visually disabled or blind. What was going wrong here?

On the plane ride back, I mulled over my reaction to the trip. I liked the environment of New Mexico and the challenge a position there offered. I had had many positive interactions with the faculty I met. But I couldn't get the vision of that fat, scarred monkey stuffed in a cage in B-1 out of my mind. He

was an example of pure neglect. I also thought about my silence. I believed that I dared not to speak up in the defense of this monkey. A sentient part of nature was surely experiencing torture and I remained mute. It was a bitter pill to swallow, but something in me was altered.

After taking the job in New Mexico, one of my first acts as an official member of the faculty was to go back to see if that monkey, also nameless, was still wearing a cage. He was. His cage was still a wire mesh trap and the cuts on his arms from self-mutilation looked worse. I offered him a piece of the uneaten sandwich I had with me and he grabbed for my arm instead. I left, drove to my apartment, and retrieved the drug bottles and syringes I had taken from the Wisconsin primate lab before I left. I returned to B-1 and asked one of Logan's graduate students to help me. We went into the monkey room. I asked Bob to try and draw the monkey's attention with some treats while I circled around behind him. I took the 1cc syringe that I had filled with Ketamine, a disassociative anesthetic, injected him with it, and quickly emptied the contents before he could turn and defend himself. It took only a few minutes before the large, soft monkey began to slowly lean to one side of the cage. In a few more minutes his body remained motionless while his eyes danced. I opened the front door to the cage, removing the hasp and clipping it to the side of the cage. Without gloves, I reached in and took hold of his upper arms and pulled him carefully backward through the door. He barely fit. I carried him with some difficulty to a wooden table and laid him on his back. I searched for an accessible vein. The only vein that I could find that was easily available was the great saphenous vein that ran up the back side of his right calf muscle. This was not ideal but it was going to have to do. I retrieved the second 10cc syringe full of Pentobarbital and inserted it lengthwise into the vein, being careful not to penetrate through the other side. I slowly emptied the contents and withdrew the needle, stopping the backflow of blood with my thumb. After just a few seconds, the monkey's eyelids began to close and the depth of his breathing became more and more shallow. Soon there was no sign of breathing. Having forgotten my stethoscope, I put my ear to his chest to see if I could detect a heartbeat. The fine hair on his chest tickled my cheek and the warmth of his body seemed out of place in this cold scene. I could hear no heartbeat. I pinched hard the soft tissue between his toes and there was no response. I touched his eye and there was no blink reflex. He was dead.

I went back to my office and thought about what I had done. Did I kill this monkey to make living in my laboratory more tolerable for me? With him gone, I no longer had to be reminded of my reluctance to face Logan directly about his failures as a lab director. Or did I do it for the monkey whose life had been so twisted by his treatment that he had no prospects either as a re-

search subject participating in an important experiment or as a being living out its complex intellectual and social capabilities in a nonresearch setting? I found that I honestly could not disentangle these motives. While they all played a part, I knew that there was something right about euthanizing the monkey. Some amount of pain and anguish in the world had been eliminated. But at what cost?

RETURNING HOME

I might have more respect for myself if I could report that, like Saint Paul on the road to Damascus, my view of how to proceed as a scientist became immediately and ethically enlightened by the circumstances of the deaths of these two monkeys. Their gift to me was that the images of the Formalin dance and the wire mesh shroud remained close to me during reflection and even dream states. But egoistic momentum, desire to know, and continuing relationships with my teachers and colleagues all held my general direction more or less in place. I am proud, however, that in organizing my own laboratory I did not assume or reward loyalty to an idea, explanation, or method. I supported students primarily on the basis of their courage, natural skepticism, and breadth of academic interests. Whether they read a lot of nonpsychology books counted more to me than experimental design expertise. Because I was far from famous, the students were less burdened by any established credibility and reputation. Although my own teachers had been open to argument, I found myself more reluctant to engage with them than I should have been given their earned stature. My desire to fit in and to remain a member of the Harry Harlow thought collective was too important.

One of my last conversations with Harry involved my asking for his support to pursue a fellowship in clinical psychology at the University of Washington Department of Psychiatry. I saw this as a possible prelude to going deeper into creating primate models of human abnormal behavior. At least that is how I sold the idea to myself and my department chair. Harry was visiting my lab and we were by ourselves when I asked for his advice. He said, "Of course I will write you a strong letter to Professor Robinson." After a short pause, he looked at me again with just the inkling of a grin and said, "Another man lost to science."

I have come to know the words "a man lost to science" as Harry releasing me from any expectations that I might have thought he had for me and his sanctioning my finding my own independent direction. That direction turned out to be away from animal research and the constant need for thin justifications for the stream of animal harms and death. My interest in research ethics

and general bioethics has since flourished and become the major focus of my work. My path is not anti-science but an expression of the need to see science as not the only value worthy of respect and action. Most importantly, we researchers must work to minimize the critical distance between us and the individual animal being considered for human use. We must place ourselves in a position where we can get some intimate sense of the predicament of the animal subject. And to do this we must have much more than just a cursory understanding of who this animal is. This was the key lever of change in my professional life. The images described here, and many others, did not disappear into a vague past. Instead they continually presented themselves to me over and over, especially in the context of critical outside questions and while considering the evidence of the frequent weak translation of animal data to the human condition. Scientific rigor must not become code for impatient brutality. My advice to the researcher: Move closer please.

NOTES

1. Catherine Drinker Bowen, *Francis Bacon: The Temper of a Man* (New York: Fordham University Press, 1993 [1963]).
2. Bernard Mandeville, *The Fable of the Bees: or Private Vices, Public Benefits*, 2 vols., ed. F. B. Kaye (Indianapolis: Liberty Fund, 1988).
3. Daniel Callahan, "Death and the Research Imperative," *New England Journal of Medicine* 342 (2000): 654–56.
4. Callahan, "Death," 654–56.
5. Babette E. Babich, "From Fleck's 'Denkstil' to Kuhn's Paradigm: Conceptual Schemes and Incommensurability," *International Studies in the Philosophy of Science* 29, no. 1 (2003): 1–39.
6. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 2012), 24.
7. G. Lahvis, "Animal Welfare: Make Animal Models More Meaningful," *Nature* 543 (2017): 623.
8. Hideo Uno, "Age-Related Pathology and Biosenescent Markers in Captive Rhesus Macaques," *Age* 20, no. 1 (1997): 1–13.
9. B. F. Skinner, *Walden Two* (Indianapolis: Hackett Publishing Company, 1948).
10. Susan Sontag, *Regarding the Pain of Others* (New York: Farrar, Straus and Giroux, 2003), 82.
11. John P. Gluck, "Harry F. Harlow: Reflection on the *Ethical Paradox*," *Ethics and Behavior* 7, no. 2 (1997): 149–61.
12. See Gene P. Sackett, "The Effects of Rearing Conditions on the Behavior of Rhesus Monkeys (*Macaca Mulatta*)," *Child Development* 36, no. 4 (1965): 855–68.
13. John P. Gluck, *Voracious Science and Vulnerable Animals: A Primate Scientist's Ethical Journey* (Chicago: University of Chicago Press, 2016).

BIBLIOGRAPHY

- Babich, Babette E. "From Fleck's 'Denkstil' to Kuhn's Paradigm: Conceptual Schemes and Incommensurability." *International Studies in the Philosophy of Science* 29, no. 1 (2003): 1–39.
- Bowen, Catherine Drinker. *Francis Bacon: The Temper of a Man*. New York: Fordham University Press, 1993 [1963].
- Callahan, Daniel. "Death and the Research Imperative." *New England Journal of Medicine* 342 (2000): 654–56.
- Gluck, John P. "Harry F. Harlow: Reflection on the Ethical Paradox." *Ethics and Behavior* 7, no. 2 (1997): 149–61.
- . *Voracious Science and Vulnerable Animals: A Primate Scientist's Ethical Journey*. Chicago: University of Chicago Press, 2016.
- Kuhn, Thomas S. *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press, 2012.
- Lahvis, G. "Animal Welfare: Make Animal Models More Meaningful." *Nature* 543, no. 7647 (2017): 623.
- Mandeville, Bernard. *The Fable of the Bees: Or Private Vices, Public Benefits*, 2 vols. Edited by F. B. Kaye. Indianapolis: Liberty Fund, 1988.
- Sackett, Gene P. "The Effects of Rearing Conditions on the Behavior of Rhesus Monkeys (*Macaca Mulatta*)." *Child Development* 36, no. 4 (1965): 855–68.
- Skinner, B. F. *Walden Two*. Indianapolis: Hackett Publishing Company, 1948.
- Sontag, Susan. *Regarding the Pain of Others*. New York: Farrar, Straus and Giroux, 2003.
- Uno, Hideo. "Age-Related Pathology and Biosenescent Markers in Captive Rhesus Macaques." *Age* 20, no. 1 (1997): 1–13.