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Reflections of an Imperfect Anthropologist

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Abstract

This article summarizes my perspective on vital lessons that I have learned over my 45 years as a practicing anthropologist. To avoid repeating previously published biographical details of my life, I have only briefly described the facts and stages of my career here. Instead I have focused on a personal account of what I have learned from the events and experiences of my career and have attempted to distill them into underlying premises that have guided my academic vocation. I call them "nine truths I live by."

False modesty is the peccadillo of the scientific memoirist.

—E.O. Wilson (2013)

INTRODUCTION

Biographical descriptions of the major aspects of my career as an anthropologically trained primatologist have been published previously (Asquith 2018; Fedigan 2000a, 2004, 2014; Haraway 1990; Jack 2017; Jack & Kalbitzer 2017; Szathmáry 2018); therefore, I do not linger long on the factual aspects of my career in this article. Instead, I extract and describe several key lessons that I have learned from sometimes challenging experiences over the course of my scientific career. I could call them "nine truths I live by" or perhaps "nine things about a life in academia that I know for sure." My act of hubris is to anticipate that by giving readers, especially younger ones, a peak behind the curtain of what may appear to be a perfectly successful career, they will see that shoring up the convincing scenery at the front of the stage are a lot of busy stagehands and jerry-rigged, sometimes malfunctioning, stage sets at the rear. And thus, I trust that these readers will find solace and inspiration in the knowledge that their own experiences of an imperfect academic career, with its missteps and unanticipated occurrences, are common to us all.

To provide some context, I start with just a few basic biographical and career facts. I was born an only child to a European war bride mother and a military father, and we moved from place to place and country to country in my growing-up years. I received my PhD from the University of Texas in 1974 under the supervision of Claud Bramblett and soon thereafter was hired at the University of Alberta, Canada, where I remained until 2002, when I was recruited to the University of Calgary as a Canada Research Chair, where I presently hold a Faculty Professorship.

As an undergraduate student at the American College of Paris, I found myself drawn to anthropology, especially ethnographic studies of the various ways in which people around the world live in social groups and how women manage to successfully live out their lives in the social company of men. When I transferred to the University of Texas, I took a range of anthropology courses and was hired as a research assistant on a graduate student project that asked women about their views on men, sex, and morality. I soon discovered that I was not comfortable asking women about their views on intimate matters. Concurrently, I took a course in primatology and found during a class project at the nearby zoo that I was well suited to observational (ethological) research. I also realized that the field of primatology could simultaneously fulfill my love of animals and my interest in social systems and the relations between the sexes. As a graduate student, I first worked with captive primates (Bramblett's colony of guenons, which had been passed along to him by Thelma Rowell). However, knowing that I wanted to study free-ranging primates, I then participated in the international project that brought the Arashiyama B (later called "West") group of Japanese macaques from Kyoto, Japan, to a large ranch in south Texas. These monkeys had been studied in Japan for 18 years before being transferred to the United States, and they were my introduction to the many benefits of long-term, collaborative research. I soon came to focus on the question of why some female monkeys were more reproductively successful than others. However, living as they did in a large 44-hectare fenced enclosure with provisioned food, the Arashiyama monkeys were still only semi free-ranging, and thus I continued my search for a suitable field site where the monkeys would safely live in a more natural setting. In 1983, I began a study of the monkeys in Santa Rosa National Park, Costa Rica (now called the Área de Conservación Guanacaste), and that project continues through the present day with the help of local assistants and university students and collaboration with several former advisees who are now fully fledged in their own careers, in particular Kathy Jack and Amanda Melin.

When I received the invitation to join the list of distinguished anthropologists who have previously published a Perspective article in the *Annual Review of Anthropology* by providing a personal

overview of my involvement in the discipline, I was honored. I decided that since the facts of my career can be easily googled, it might be more informative as well as more engaging for me to provide a frank description of some underlying truths I have come to hold as self-evident during my life as an anthropologist. Here they are.

THE SCIENTIFIC PROCESS IS SO MUCH MESSIER THAN PUBLICATIONS WOULD LEAD ONE TO BELIEVE

Given that my fundamental research topic of interest has long been "success" in its various guises [social success, reproductive success (Fedigan 1994)], it took me a long time to accept that failure is a large part of the scientific endeavor. Whether it be the failure of our hypotheses to be supported, our experiments to work, our manuscripts to be accepted for publication, or our attempts to obtain funding and tenure-track positions, my experience and those of my mentees have been that disappointments, errors, and setbacks are at least as common as successes.

Furthermore, the polished, extensively revised research that is available and consumed in the form of publications is the end product of a long endeavor that began as an idea in the researcher's mind and progressed through many arduous stages. This process, from light bulb moment in the scientist's brain to published product, is what Shapin (1992) referred to as "science-in-the-making" versus the sanitized, hopefully perfected version that readers of publications see, and what Latour called "science-already-made" (Latour 1987, 2000). In the field of science studies, which includes several schools of thought and methodologies (Thompson Cussins 2000), many researchers observe the actual processes of science in a variety of labs and disciplines and document the ways in which scientific knowledge is produced. What these researchers have found is that the actual practice of science is usually complex, demanding, culturally laden, and imperfect. Many scientists do not like the idea of being the subjects of study themselves, and they sometimes view science studies as inherently critical of science [leading to the disputes between practitioners and observers or between scientists and humanists that became known as "the science wars" in the 1990s (Strum & Fedigan 2000)]. However, science studies scholars often view themselves as anthropologists (or sociologists) who specialize in the "ethnographies" of scientists and their labs and observers of scientific behavior. Their research on the actual practices and processes of science leads us to understand that it is an inherently human activity (rather than a transcendental feat) with its share of mistakes, setbacks, dead ends, and economic, philosophical, and social biases. The self-awareness we can gain from studies of (and introspection about) our process makes for better scientists. In my experience, young scholars are especially prone to perceiving themselves as failures when their projects do not proceed according to some perfectional ideal, and it behooves us to help them realize that their experiences of failure, setbacks, messiness, and rejection are in fact inherent to the practice of science.

It is admittedly difficult to accept that our research necessarily includes flawed ideas, dead ends, wasted time, and the stinging criticism of our peers. But even scientific geniuses such as Albert Einstein had some ideas that did not pan out (Isaacson 2007). As an example from my much more modest career, some readers may know that I have now successfully conducted primate research over three decades at a protected and productive field site. However, what most will not know is that it took five substantial attempts and four failures before I found a site where I could conduct the type of long-term research that I had in mind. After working with captive and provisioned primates, I sought a field site where the animals lived in a natural setting with little anthropogenic disturbance and where the animals and researchers would be safe. I first obtained grant funds to study Barbary macaques in North Africa but failed to obtain a visa to work there. Next, I studied spider monkeys briefly in Central America at a time when the government was unstable, and one

of our local team members disappeared. The monkeys were safe there but the people were not, and so I had to give up that project. For my third attempt, I spent a year on a Caribbean island where the government was stable, but the vervet monkeys were not protected. Belatedly, I came to realize that my attempts to habituate those monkeys to observation only rendered them more vulnerable to being trapped and harassed. Thus, I had to terminate yet another project. I also attempted to study nonprovisioned macaques on a southern island of Japan but became seriously ill and had to fly home. Finally, I was able to establish a long-term field project in Costa Rica, where the animals are protected, the government is stable, the students are safe, and the local people are mostly ardent conservationists themselves.

Similarly, I have had manuscripts that have been rejected out of hand or put through what felt like unending rounds of revisions until my own papers seemed no longer recognizable. And since the beginning of my career, publication of manuscripts has become increasingly difficult. The first manuscript I ever submitted (as an undergraduate student) was accepted promptly with no revision requests, an accomplishment I never repeated in my 48-year publishing history. My graduate students and colleagues and I have faced increasingly uphill battles in our attempts to publish our work and in their attempts to establish their own field sites and tenure-track positions. My experience (and that of my collaborators) is that it is especially difficult to publish negative or nonsignificant results or findings that run counter to popular theories and hypotheses in one's field. A similar experience in the field of medicine led to the establishment of a journal called *The Journal of Negative Results in Biomedicine*, but unfortunately it folded in 2017.

Even outright failures inevitably teach us a lot. In their edited volume *Scientific Failure* (Horowitz & Janis 1994), Janis outlines three common types of failures that are the basis of much scientific progress: (*a*) when an accepted theory is found to be incorrect or incomplete because of newly discovered observations; (*b*) when a theory was never generally accepted and is finally discarded after being tested; and (*c*) when researchers fail to take account of all the relevant data and are later called to task by subsequent researchers. Janis also argues that progress in engineering proceeds in similar ways to that of science, with advancements often developing from previous failures of design. In an article titled "Will the Museum of Failure Succeed?," Petroski (2018) laments that science and technology museums showcase successful masterpieces without showing patrons the inevitable mistakes and missteps that occurred along the path to brilliant developments.

Ice and colleagues edited a volume (Ice et al. 2015) on the types of mishaps and unexpected events that typically occur while conducting international field research. They point out that although anthropologists often experience such challenges while conducting their research (e.g., equipment failures, illness, injuries, permit delays, cultural misunderstandings), we seldom train and prepare our students for them. Along with providing substantial advice, their book includes 46 anecdotes of "disasters" submitted by colleagues of the editors who experienced setbacks while doing fieldwork. These anecdotes are both instructional and entertaining. A recently developed website called *#FieldWorkFail* invites researchers to submit their blunders. My favorite entry on that website is the biologist who accidentally superglued her hand to the back of the crocodile on which she was fixing a radio transmitter. Whether our failures are minor or major, amusing or frightening, we do learn from them, if only not to repeat whatever brought them about.

PASSION AND PERSISTENCE ARE AS IMPORTANT AS TALENT

"Nevertheless, she persisted" became a popular expression (and T-shirt and ball cap slogan) in the feminist and activist movements after Senate Majority Leader Mitch McConnell made this

remark in 2017 as justification for the silencing of Senator Elizabeth Warren when she refused to stop reading to the US Senate a letter from Coretta Scott King. Variants of this phrase form the title of a series of children's books by Chelsea Clinton (e.g., Clinton 2017). With some gender-neutralizing modifications, "Nevertheless, I persisted" would be an excellent motto to adopt as practicing scientists.

I have mentored many students over the course of my career. Some of them were highly talented in that they were quick to understand new material, promptly conceived of worthy research problems, and possessed excellent observational and/or quantitative skills. However, some of these very talented people dropped out of their programs. Occasionally this was because major life crises intervened; often, however, these individuals were not really in the right discipline to begin with and thus lacked the necessary amount of passion to pursue their topics, or they became too discouraged by setbacks and failures. I came to conclude that being clever does not suffice. As H. Jackson Brown, Jr., put it, "Talent without discipline is like an octopus on roller skates. There is plenty of movement, but you never know if it's going to be forward, backwards, or sideways." Others of my mentees, who are certainly smart but did not immediately dazzle me with their mental acumen, have gone on to very successful research careers because they love what they are doing, they work hard, and they carry on after setbacks.

I am not certain how much the trait of persistence (also known as grit) is inherent versus developed, but it has been a key factor in my research career. My interest in female primate life histories and reproductive success has by its very nature necessitated long-term research. In the several monkey species that I have studied, females live up to 25 or more years; if you want to understand the trajectory of their lives, you must be persistent and you must enjoy the work of tracking them. Early studies of primates in the wild usually lasted only a year at most, often just long enough to collect data for a PhD project. Today, despite the difficulties of maintaining consistent funding and continuous data collection, many primate field projects have lasted more than a decade. Some social behaviors such as changes in dominance hierarchies and dispersal between groups occur only over long periods of time, and without long-term studies it would be quite impossible to understand the influence of kinship, ontogeny, mating histories, and rare ecological events (droughts/hurricanes) on behavior. Classic examples of noteworthy behaviors seen only after years of study are Goodall's observations of tool use and Boesch's observations of lethal aggression in chimpanzees (Kappeler & Watts 2012). In the case of the capuchins that I study in Costa Rica, it took Kathy Jack and me many years of data collection to confirm that males move between social groups not just once but multiple times in their lives, often in the company of their male siblings, and that these male dispersal patterns have significant repercussions for females (Fedigan & Jack 2012, Jack & Fedigan 2004).

Mother Teresa is reported to have said, "We cannot all do great things on this earth, but we can all do small things with great love." My adaptation of this aphorism is that we may not all be able to do great (or famous) science, but we can all do our research with great passion and persistence. And love.

THE ABILITY TO COMMUNICATE YOUR WORK EFFECTIVELY IS AS IMPORTANT AS THE CONTENT

I taught a graduate seminar in professional skills for 25 years, and I was originally prompted to do so for three reasons. The first was because I observed a consistent feature in many academic failures: lack of adequate communication skills and poor assessment of the audience by the speaker/writer. In the first half of my career, I had seen many a conference talk dismissed or critiqued because the speaker had poor delivery skills, even though there was valuable content in

the presentation. I had sat on grant and scholarship review panels during which proposals were turned down because they were not strategically presented, even though the research concept itself seemed worthy. As an editor and reviewer, I had seen manuscripts rejected because the authors did not write clearly, correctly assess the readership, or follow some simple procedures. And I had seen applicants fail to be selected for a position because they did not know how to handle a job interview. The second reason I began to teach the pragmatic, nitty-gritty skills of academic performance is that for seven years I served as an Assistant Chair in charge of a large graduate program, during which time I found that some supervisors trained their mentees in professional skills and other supervisors expected their students to pick up these skills on their own, or absorb them from the atmosphere, or "learn the hard way" through bad experiences. It made for a very uneven playing field. The third and most honest reason is that I was tired of sitting through badly presented talks and reading poorly written proposals.

A national survey of some 3,000 social science PhD recipients followed five or more years out from their degrees (Nerad et al. 2007) found that many of them wished that they had received more training in grant writing, speaking and teaching skills, publishing strategies/processes, and managing of budgets. These professional competencies (mostly communication skills) were the aspects of their training they felt were most lacking and yet most needed in their post-PhD positions and stages of career. Some anthropology departments now offer professional skills courses; but even if the departments do not, most universities include resource units such as writing support services, student success centers, teaching and learning institutes, and centers for scholarly communication, where students can take courses or be otherwise mentored in these aspects of scholarly presentation. At other institutions, grassroots organizations founded by graduate students themselves develop workshops in science communication (e.g., Shorr 2017). You can be the most brilliant scholar in your area, but you likely will not obtain a grant or a job or be published and recognized if you present your work and yourself poorly. Young scholars clearly need to acquire expertise in their chosen fields, but mentors and students also need to invest time in developing good professional skills.

QUESTIONS CHOOSE US AS OFTEN AS WE CHOOSE THEM

I have devoted a large part of my research career to understanding the ways in which female monkeys coexist in year-round societies with males. Yet, for many years, it seemed that every time I was interviewed by a popular science journalist or cornered at a party, or explained my work to my friends and family, I was asked, Why are there so many women primatologists? For the longest time, I dismissed this question as an unwanted digression and replied that I did not know the answer (while inwardly thinking it was just a mistaken assumption that was due to the wide media coverage of Jane Goodall's work with chimpanzees). Then one day, the irony hit me that I was actually being repeatedly asked about the coexistence of females with males, even though in this case it was human females and males coexisting in a scientific "society." This was in fact a good research question. According to Wilson (2013), finding the right question to ask is even intellectually superior to finding the right answer. The Buddhists have a saying that when the student is ready, the teacher will appear, which to me means that when the scientist has an open mind, the work will unfold. Or as Louis Pasteur said, "Fortune favors the prepared mind." That certainly was true for me in this example.

After I opened my mind to the value of addressing this issue, I began by collecting and analyzing data to determine if there are in fact "so many" women primatologists [and yes, there are from the perspective of analogous rather than ancestral disciplines (Fedigan 1994)]. After that "Aha!" realization, an entirely new and productive field of inquiry opened for me, as I began a series of

papers exploring why primatology might be an especially welcome field for women scientists as compared with other disciplines (e.g., Fedigan 1997a,b; 2000a,b).

Speaking more generally, an open mind is clearly an important characteristic in a scholar. We undoubtedly need to come up with ways to test theories that are dear to us, but we also need to accept that we may fail to find support for our favorite hypotheses. And the data always win, that is, the results trump our preconceptions. We must always listen to the data. Contrary to expectations, dominant females may not produce more surviving infants than do subordinates, and adult male monkeys may both kill and care for infants. What takes place in nature (both human and nonhuman) is not always pleasant or ethically desirable, but as scholars we must accept what the data have to say.

WORK FOR THE CAUSE, NOT THE APPLAUSE

Science as a career has a reputation of being a cutthroat enterprise, and certainly some fields of science are highly competitive, with researchers being highly driven to obtain the largest grants and awards and racing to publish their results before others do. But my experience is that cooperation produces as many breakthroughs and advancements as does competition. And I would argue that collaboration is increasingly the general direction of scientific funding and publishing in North America. Even for those scholars who prefer to work alone rather than in teams, it is both productive and fulfilling to become part of a network of people who share ideas, experiences, and lessons learned from both failures and successes.

Early in my career, I was mentored in the study of a large group of macaques by a Japanese researcher who taught me to recognize individuals while he described for me their personalities, social networks, and life histories. In fact, he turned over 18 years' worth of genealogical data to me and to another young researcher, following in the Japanese primatology research tradition of working collaboratively and sharing information. That was a formative experience in my history as a scientist, and it set the stage for me to always look for ways to cooperate with other scientists, a practice I have made one of the hallmarks of my career and tried to instill in my mentees.

Rather than racing to be the first, or jealously guarding our data from the eyes of others, we can begin to address much more interesting questions when we work together or share our data. In 2007, I was invited by Karen Strier to join a group of seven other researchers who are directors of long-term (>30-year) primate field projects, and we developed an integrated database for our life history data. The result has been a series of influential papers addressing comparative life history issues that none of us could have addressed alone, plus a literal scientific treasure trove of thousands of life history records in the Primate Life History Database (Strier et al. 2010). Because integrated databases allow us to address bigger, more cross-specific issues, the primatology colleagues in my department and I have also developed more recently a collaborative database at my home institution, which I trust will also generate comparative analyses above and beyond what any one of us could have done on our own.

Harry Truman put it this way: "It is amazing what one can accomplish when no one cares who gets the credit." This statement exemplifies another of the lessons I have learned over the course of my career: If you do the work because you believe in it, and you care about it, then satisfaction, accomplishments, and accolades will follow unsolicited. Conversely, to do the work because you want to become famous sets you on the road to an unhappy career because the desire for fame is an empty goal that is never truly fulfilled. You become like the "hungry ghosts" in Buddhist myths: greedy beings who endlessly try to drink and eat but are never sated. My experience is that if you seek and find a topic or a cause about which you are passionate and make that your focus, you will also find fulfillment.

ANTHROPOLOGY COVERS A HOST OF TOPICS AND WE WOULD DO WELL TO GET OVER IT

I once sat in a six-hour departmental meeting during which faculty from across the main subdisciplines tried and failed to come up with a definition of anthropology upon which everyone could agree. It was exhausting and demoralizing. Apparently, an executive committee of the American Anthropological Association had similar difficulties agreeing upon an encompassing definition of anthropology in 2010 and set off a viral debate in *The New York Times* when they suggested omitting the word "science" from their newly proposed definition (Horowitz et al. 2019, Kuper & Marks 2011). I will not repeat that futile search for uniformity/consensus here and instead assert that there is enormous variety in the kinds of questions addressed by anthropologists, a breadth that is impressive and perplexing even to ourselves. Traditionally, a common definition of anthropology is that it is the study of human behavior, biology and evolution, or the science that studies humankind in all its aspects. There are many (somewhat disputed) ways to cut up this pie, but in the era in which I was trained as a graduate student and in my first 20 years as a professor, it was common in North America to divide anthropology into four major subfields: sociocultural, linguistic, archaeology, and biological anthropology ("four-field anthropology"). There is clearly overlap between the research carried out by members of these four subfields and that conducted by neighboring social and biological disciplines. To my mind, however, anthropology is basically a comparative science that differs from sociology, linguistics, and biology in its cross-cultural perspectives, from psychology in its greater attention to social systems than to individual cognition, from zoology in its cross-species (including humans) comparative perspectives, and from history in its focus on material remains of both prehistoric and historic societies.

However, anthropologists themselves are often shocked by the multitude of research topics encompassed in our discipline. For the many years that I was the coordinator of a large graduate program, a constant refrain of committee members, as we read and evaluated the wide array of applications that came to us, was "But how is this anthropology?" This question was asked not just of primatology, but of applications in all the subdisciplines. As noted by Kuper & Marks (2011), anthropology has become increasingly a fragmented discipline with relationships among the subfields becoming more and more distant, and sometimes fractious [the so-called science wars played a role in the disintegration of some major anthropology departments in the United States in the latter part of the twentieth century and may continue today to a lesser extent (Horowitz et al. 2019)]. However, some leaders in anthropology, such as Sydel Silverman, have worked hard toward communication among, and integration of, the subdisciplines. In her decade as the president of the Wenner-Gren Foundation, Silverman organized and hosted many international conferences whose objectives were in part to foster cross-disciplinarity (Silverman 2002). If it is too lofty a goal to construct a truly holistic, collaborative study of humankind in all its facets, then the least we can do is to accept and support the many branches of the ever-growing tree that is our discipline.

THE STUDY OF ANIMALS (ESPECIALLY PRIMATES) HELPS US TO UNDERSTAND WHAT IT MEANS TO BE HUMAN

When I was 16, my parents took me to live in Europe for three years near my German/Italian family. That experience gave me enormous perspective on what it means to be a North American. When I was 23, I spent two years living from dawn to dusk every day with a group of monkeys, during which time I developed new points of view on what it means to be human. You can hardly claim to be unique if you know nothing about anyone else. Without a comparative perspective, there is little depth to your perception. Sherwood Washburn was enormously influential in the development of primatology in North America, and he argued that the study of primates in the wild

would give us unique and valuable insights into the evolution of human social behavior (Washburn 1973).

Nonetheless, as described by Pavelka (2002), there has been extensive and ongoing resistance among sociocultural anthropologists and other social scientists to the extension of a cross-cultural to a cross-species (interspecific) framework. That question of "How is this anthropology?" is most often directed at primatologists, and members of this subdiscipline often feel marginalized and isolated in anthropology departments (Strier 2011). Why might this be the case? One reason is that because anthropological research ranges so widely from the highly scientific and quantitative to the highly humanistic and qualitative, there is inevitable tension among the subdisciplines (Kuper & Marks 2011). A second reason concerns mistaken assumptions about primatologists—for example, that they (we) are all biological reductionists, that we draw simple, straight-line analogies between animal and human behavior, or that we support social Darwinism.

At the same time as social scientists were resisting the interspecific perspective, many anthropological primatologists themselves were moving away from Washburn's rationale for including primate studies as an arm of anthropology because these scientists of the next generation were adopting the methods and theories of behavioral ecology. Instead of saying that they studied primates in order to provide context for the evolution of human behavior, more and more they said that they studied primates for themselves and in order to test hypotheses drawn from wider evolutionary and ecological models of animal behavior (Strier 2011).

Despite the continued unsettled positioning of our science within departments of anthropology, Strier (2003) has argued that contemporary primatology is developing overlapping interests with anthropology in several ways: for example, studies of local behavioral "traditions" in chimpanzees and capuchins; documentation of the wide range of intraspecific and interspecific variation expressed by humans and other primates; examinations of the multifaceted interactions between human and nonhuman primates (ethnoprimatology); and, not least, shared conservation concerns.

I study primate life histories and behavioral ecology because of a lifelong interest in animals, social behavior, sex differences, and the puzzle of what makes some individuals more successful than others. However, there is no doubt that an interspecific comparative framework provides me and my colleagues with insights into human patterns that would not otherwise be available. For example, Mary Pavelka and I have examined the phenomenon of human female menopause (universal termination of ovarian cycles halfway through the species life span) as one example in the larger context of reproductive senescence patterns in female primates (Fedigan & Pavelka 2015). This endeavor has demonstrated that the lengthy postreproductive life stage of human females is indeed unique among primates, and our interspecific perspective forms part of a larger body of anthropological research as to why the phenomenon of menopause in human females might have evolved and continued to exist (Alberts et al. 2013, Fedigan & Pavelka 2015, Hawkes 2004, Pavelka et al. 2018, Pavelka & Fedigan 2012).

WE ARE GOING TO CHANGE OUR MINDS

As anthropologists we learn that our study subjects never let us rest complacent in our understanding of them. We are constantly revising our views of the people and alloprimates that we study. Many examples of such changing views and resultant controversies can be found in the various subdisciplines of anthropology. Derek Freeman (1986) famously (some would say infamously) challenged Margaret Mead's descriptions of adolescent female sexuality in Samoa and was then criticized for his own views. Jeanne Altmann (1980) produced a very different view of sex roles in baboons than had Washburn and DeVore in their earlier descriptions of a male-centric society. Sometimes we critique and re-evaluate our own earlier work.

In the case of the capuchin monkeys that I follow in Costa Rica, I have spent 30 years trying (and mostly failing) to determine why female dominance rank does not predict the highly variable female reproductive success that we have documented. Over that time, I have evaluated multiple factors, one after the other, that might definitively predict reproductive success and have largely failed to find one key factor. It helps a female capuchin to have lots of cooperative female relatives living in the group with her and for her to give birth in years with ample food. But only recently have I gathered enough information from the course of my long-term study to decide that the best predictor of differential female reproductive success lies with male behavior and the sexual conflict that is often targeted at high-ranking females (Fedigan & Jack 2013). For the longest time, I admittedly hoped that infanticide as a male reproductive strategy would not play a role in the capuchin society that I study. However, when my advisees, assistants, and I began to observe and report infanticide (leading one of my colleagues to say that I had "gone over to the Dark Side"), I had to accept what the animals were doing, what the data were showing me. The adult males in the species that I study routinely kill infants that they have not sired after these males immigrate into groups. So, along with studying female life histories, I now examine exactly what the costs are of male infanticide to female reproductive success—yet another question that chose me.

CONSIDER THIS: WHAT WILL YOUR LEGACY BE?

As a researcher or academic, you will inevitably find that there are too many things to do and too little time in which to do them. People choose various ways to cope with this stress: They may adopt/develop time-management skills, learn to say no to unwanted requests, or become incredibly organized, or, conversely, they may simply fail to keep up and constantly drop the ball. My suggested strategy is to decide on your endgame. What do you most want to leave behind by which you will be remembered?

Some of my colleagues have carried out amazing studies that pushed forward the theoretical, methodological, and/or knowledge frontiers of their research topics. Others have published prolifically and won many grants and awards. Some have determined that the larger calling of their career is to work toward the conservation of their study subjects. A few have built research teams of peers that function collaboratively to answer more advanced questions than each could address on their own. Others have decided that their special gift is to bring science to the public, or they have taken leadership roles in, and provided service to, our departments and professional organizations.

As a graduate student, I had the benefit of a wonderfully supportive supervisor and supervisory committee, and I have always wanted to emulate this experience. My fundamental goal has been to mentor students not just in their coursework but throughout their careers. It has been my objective that every one of my advisees not only develops expertise in conducting research, obtaining grants/scholarships, and producing theses/dissertations of which they can be proud, but also that they publish from their theses and obtain employment relevant to their research training. For me, the success of my advisees is as rewarding as my personal awards. It is my best accomplishment.

Determining what you want your legacy to be early in your career will provide a guide as to how best to spend your limited time and energy, and a clear articulation in your mind of your ultimate objective will provide your career with focus and you with fulfillment.

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The author is not aware of any affiliations, memberships, funding, or financial holdings that might be perceived as affecting the objectivity of this review.

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