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Chapter 6

MY DESCENT FROM THE MONKEY

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I want to explain why a credentialed specialist in primate social behavior should have wanted to become an ornithologist, just when the Age of Primates was at its height. Could it have been only an obstinate inclination to be always where the action isn't, or could it be that primatology ceased to generate the kind of intellectual action that makes for sustained progress in a scientific domain?

A few of my readers may be primatologists of one sort or another and, as such, may have written funding requests to granting agencies during the early 60's. If so, you probably wrote that dreaded paragraph which explained to the review panel why it should put aside 20 inexpensive studies of local and/or easily maintained species, such as the rat, the frog, the mourning dove, the German shepherd, or the American housewife, in favor of your primate study, which demanded either a Landrover and a ticket for eight to Nairobi or 20 custom-made stainless steel cages and an air-conditioned laboratory lavishly outfitted with equipment for maintaining exotic animals. In such paragraphs one traditionally argued that the monkey should be studied—despite the exorbitant costs—because the monkey is closer to man. In the course of funding my graduate career I wrote many such paragraphs, and each made me more uncomfortable than the one before. The logic was obviously devious. If "closeness to man" was such a certain warranty of theoretical importance, then why not study man himself? I always thought that one day I would be punished for these hypocrisies, but never did I realize how fitting and devastating the punishments would be.

I was, by coincidence of geography and age, one of the first graduate students to specifically train himself for a career studying the social be-

havior of primates. I believed deeply in field study, but because of an irrational fear of leeches and lions, I set about doing a laboratory study in partial satisfaction of the requirements for a Ph.D. in Comparative Psychology. For three years, off and on, I and my monkeys performed together in the beautifully appointed, windowless basement laboratories of Tolman Hall at the University of California at Berkeley. My days were spent from nine to five testing and filming monkeys, while my evenings were devoted to coding and organizing the hundreds of thousands of frames of timelapse film which my labors were generating.

From the horror of those days, I remember almost nothing about simian social organization. I remember the constant battle to keep man, monkey, and environment in their separate places. I remember the paraphernalia of sanitation: the stainless steel buckets, the steam hoses which behaved like sulky snakes when one tried to coil them, and the anti-T.B. foot tray which exuded a pleasant odor of iodine but which left one's foot faintly wet for hours. I remember the virus B. anxiety, helped along by occasional ambiguous reports of deaths at other laboratories, deaths from monkey bite, or monkey scratches, or monkey spit, or perhaps even just monkey breath. Each of these hazards required a piece of garb to protect the investigator, so that the official standard outfit in our laboratory consisted of boots, coveralls, rubber gloves, paper caps, face masks, and goggles. Each worker in the laboratory omitted some part of this armor, based on a personal calculation which included as variables how much he hated the particular item and how recently he had read of a Monkey B. death. The goggles were a favorite omission because they fogged on the inside from one's breath and on the outside from the steam hoses. Those of us who omitted this particular item developed a covert, subconscious squint, which we deployed in the presence of monkeys to guard us against a lethal dose of monkey spit. Thus did we who had pledged our professional lives to understanding monkeys spend the majority of our energies in isolating ourselves from monkeys.

What is significant about this period is that I came to hate monkeys. These were not the lush-furred, loving citizens of DeVore's or Carpenter's troops; these were incorrigible prisoners, and I was both warden and sole guard. All the while I worked with my subjects, I was promising myself never again to look at a confined monkey; since leeches and lions still held their terrible power over me, I had to recognize that I was promising myself never again to look at a monkey. Readers may conclude at this point that my reasons for leaving primate social behavior were too personal to be of general relevance. But personal reasons have a way of gathering logic, and many a scientist has been forced to make observations of catholic importance because of a personal bind. The trivial decision to change from

monkeys to something else was not interesting in itself, but rather the reaction this decision provoked in those with whom I subsequently had dealings.

I took my degree and went looking for a job as a comparative psychologist. On every side my disclaimers of interest in primates were greeted with disbelief. Like every young primate specialist in those happy days of a lush job market, I was courted by universities hoping to start primate laboratories and thus partake in the seemingly limitless flow of funds that was to follow. Over coffee and brandy in the new faculty clubs of many a burgeoning campus, provosts and chairmen inquired solicitously after my needs. How many square feet for my laboratories? How many technicians? How many secretaries would be needed to type the myriad grant requests that I would undoubtedly be filing? And best of all, (*sotto voce*) How did I project my initial publication thrust? Graduate students of the 1970's may think this discussion self serving, but only because you cannot imagine how inflated the stocks of primate social behavior were in the 60's. No sane man—particularly an accredited primatologist—would forsake the monkey for a lesser species. What did I have to offer instead? came the ultimate incredulous query. In desperation, I grasped at the crow. At that time, I had learned only two facts about crows. At the age of 23, I had learned from a graduate student colleague that crows had an interesting vocal system. At the age of 10, I had learned that you can't shoot a crow with an air rifle. For a desperate man, these two pieces of information were enough.

Desperate choices—like personal reasons—have a way of gathering logic. You can no doubt imagine the consternation of my interviewers when I announced that, no, I wouldn't need technicians, no, I wouldn't need help preparing a grant proposal, NO, all I would require, thank you, was a good pair of binoculars (8 × 35 or 7 × 25 would do) and a few years to *think*. I began to make this announcement later and later in the interview, since I began to sense a high correlation between those interviews in which I mentioned crows early and those interviews in which I found myself back at the airport without the customary dinner and brandy at the faculty club.

But time was passing. In that year in which major universities were fighting over M.A.'s, I with my Ph.D. was not finding a job. I decided that early confession of my conversion to crows was essential: at the worst, it might save me unnecessary travel. At best, it might favorably dispose chairmen and provosts toward me, if not for my wisdom, at least for my forthrightness. The first letter in this program went to a department chairman at the major campus of a growing eastern university, who concluded that I was pulling his leg. He called me long distance, with several colleagues lurking on extensions to check me out. They rang up bright and early one morning all full of what I later came to identify as the bustle and good cheer of an eastern university on a rare sunny morning. In their

consternation, they forgot about the time differential between the east and west coasts. Thus the call arrived in the predawn of an all-too-typical gray, foggy northern California morning. I remember very little about the conversation which ensued. My wife tells me that she woke up, gradually and painfully, to see her husband, naked and silhouetted against the chilly dankness, phone in hand, touting the merits of the American crow as a subject for coordinated field and laboratory research.

I finally took a temporary position as a comparative psychologist at a small liberal arts college. Was I sure that I didn't want space for monkeys? I thanked them for their concern, borrowed some binoculars from the biology department, and entertained myself by following crows about the woods. The job market cooled. I shamelessly published a crow paper. Provosts stopped calling me. My mail consisted entirely of entreaties for payment from Brill. I had become (as one of my colleagues put it) a zoologist.

In the peaceful hours which followed, I had ample opportunity to repent the "closeness of man" argument with which I had so often financed my monkey researches. If I had been funded in 1963, '64, '65, and '66 to study monkeys because they were so "close to man," how could I now in good conscience proceed to study so "distant" a member of the animal kingdom as the crow? One solution to this quandry was to argue that the crow is closer to man than the monkey. I used to require a drink in my hand in order to make this argument, but now I can do it cold sober. It entails, as you can imagine, a rather elaborate redefinition of the word "close."

When people use the word "close" in this sense they normally have in mind a phylogenetic tree. Two animals are said to be "close" when they are on offshoots of the same twig, "far" when they are on different major limbs. The phylogenetic tree is basically a classificatory device. Things are "close" or "distant" in the tree because of the degree to which they share similarities. Thus, man and monkeys are on the same twig because of the vast numbers of anatomical similarities which they share, but they are on separate twiglets because of some important behavioral and morphological differences. Morphologically, the evolution of man involves some novelties in the hind limb, pelvis, and brain; behaviorally, man displays a communication system which has no parallels among other primates and a social organization which is unique in its complexity. Man's communication system is useful for conveying detailed information between two individuals with different experiences. His social organizations display the constantly shifting social groupings which would make such information exchanges valuable. Two hunting parties divide up and go in different directions; at night-fall they meet again and exchange notes. Hunting is good to the east, bad to

the west. The next day, both parties exploit the better site. When they return to home that night, they are able to relate in detail their experiences to all who will listen and learn.

In comparison with human language, monkey communication systems seem to be most useful for conveying immanent social intentions. Messages such as "I am going to hit you," "I am going to mount you," "I am ready to be groomed," and "I am going to run away," predominate in monkey communication. Monkey groupings ordinarily make an exchange of information about the outside world unnecessary. Most sorts of monkeys live out their days in stable, closely knit social groupings with a minimum of coming and going. Messages about the environment need only consist of such directives as "look there," or "attention danger," or "danger on the ground." The vocalizations of most monkeys consist in rather unelaborate graded series apparently not far from the mental capacity of the average dog to decipher and emit. In fact, nothing about the complexity or sophistication of macaque vocalization or social organization would exclude from the alpha male role an intelligent collie dog.

Crows, on the other hand, display many characteristics of communication and social organization which, on first impression, seem to mimic their human equivalents. The social organization seems to shift constantly between large groups and small. In one day crows can be seen alone, in family groups, in owl mobs of 50 to 100 individuals, and in roosts of several thousand. The transitions between these various groupings are accompanied by incredibly complex vocalizations, ranging from yips and coos to growls, rattles, and sniggers. These sounds seem to be organized at two levels, analogous to syllables and words in human speech. Some of the vocalizations do not bear any obvious relationship to immediate social events, and thus are possible exchanges of information about circumstances displaced in space or in time. Thus, while nobody would argue that the crow is close in morphology and general behavior, I might argue that the crow is "close" in those features which seem to make man special. The crow may be "close" where it counts.

But the Zeitgeist was against crows. The 1960's were the Age of the Innate Depravity Book. This phrase was applied by Ashley Montague to a class of books which traced man's griefs to his relationship with animals, and particularly primates. Over the last decade, dozens of books and magazine articles on this theme have appeared, as one by one noted—and not so noted—ethologists, anthropologists, sociologists, and just plain intellectual dilettantes cashed in. The common conceptual thread in these books was the idea that man's true animal nature is covered by a veil of civilization. If we are truly to know man, we must not study him but rather his poor relations who wear their true natures on their hairy sleeves. This

argument is the latency gimmick, used to protect a false principle from the facts which would disprove it. The manifest innocence of the accused heretic is seen as evidence for the wiliness of the devil; the manifest sexlessness of a dream is seen as evidence for the power of the repressive forces against sexuality; the manifest loyalty of the State Department official is seen as evidence for the guile of Communism; and the manifest civility of most humans is seen as a veneer over the unacceptable animal nature deep within.

The public affection for the innate depravity idea is truly phenomenal. During this period, I taught an adult education course under the naive presumption that I would have to sell the significance of animal behavior to the average man-in-the-street. On the contrary, my chief problem was to convince the man-in-the-street not to take every monkey anecdote to heart in the raising of his children. My students were New York citizens who rode the subways and daily saw the spectacle of hundreds of people of every age, sex, race, religion, mode of dress, smoking habit, eating habit, and degree of wakefulness compressed peaceably into an intimacy rarely shared between husband and wife in our society, much less by strangers. Yet despite this spectacle, my students were eager to interpret the handful of aggressive incidents which occur daily in the subways as evidence that man's simian nature ill suits him to live in an urban environment. To an ethologist, what is surprising about people in subways is not their hostility; on the contrary, it is the degree of coordination and habituation which permits thousands of people to move daily through an environment so physically hostile as to stampede the herds of any sane animal. But these mature, intelligent New Yorkers rejected their daily personal experiences of this miracle in favor of the distant evidence of some knifing presented by a tabloid. The extent to which members of the public reach out for this sort of lurid information suggests that the innate depravity idea is more deeply rooted in their minds than could possibly have been accomplished in ten years of ethological popularizing. Indeed, it suggests that the ethological popularizers have been reflecting popular beliefs, rather than the other way around.

We in the animal behavior business have felt very ambivalent about these books. The books have resulted in a kind of attention from society in general, and from government in particular, which was personally encouraging and financially gratifying. One year, the Smithsonian wined and dined the ethological profession in a four day conference entitled "Man and Beast." The conference started with cocktails in the National Zoo, complete with a madrigal group in an empty giraffe cage and a string quartet in an empty zebra cage. Cocktails and hors d'oeuvres were served on damask before the hippo cage, while the hippo looked on. Gracious Washington ladies with patronizing pictures of LBJ on their mantles gave dinner parties

in honor of ethologists; earnest roundtable conferences were held, and impassioned pleas for the preservation of the hunt were made by lobbyists from the National Rifle Association. Hands were shaken by Chief Justice Warren, and advice was sought on foreign policy by William Fulbright. A gala held in the National Gallery was attended by international statesmen. It was all very glamorous. I confess that when I saw my colleagues getting all this attention, I wanted very badly to write an innate depravity book. Why shouldn't I trace man's ills to his relationship to the crow? After all, man is vertebrate, too! I rushed home to my typewriter. "The black nature of man," I wrote, "harks back to the black plumage of his cousin, the crow." My electric typewriter jammed in protest, but I wrote on. "Have you ever wondered why social prejudice, man's greatest social evil, is called Jim Crow? Have you ever wondered why, in the mythology of men, both the crow and the devil speak with forked tongues? Have you ever wondered why a man who humbles himself—returns to his origins, so to speak—'eats crow?' Surely [and here my typewriter set about producing a profusion of x's and had to be subdued with a sharp blow] . . . Surely, these expressions are not mere coincidences. Surely they embody the deep folk wisdom, the certain race knowledge, that *man is descended from the crow!*"

But my typewriter and my conscience prevailed in the end. The crow was not to be defended against the monkey by the closeness argument. How was it to be defended? How was I ever to get the monkey off my back?

At this time I attended a symposium on the "Social Significance of Animal Studies." Any reader who has recently been a graduate student in psychology will recognize the title as that of the classic D. O. Hebb and W. R. Thompson entry in the *Encyclopedia of Social Psychology*. The main theme of this article is that the significance of animal studies is not in the generalizations one can make about man, but in the power of such studies to cause human investigators to see human behavior in a new light. A comparative investigation is like a good vacation; when it's all over you can never again quite get your tires into the old ruts. The symposium was convened in one of those plastique baroque ballrooms of a Philadelphia hotel. A standing-room-only crowd of social and clinical psychologists was waiting eagerly for the beast to be unlocked from the cabinet of man's soul. The speakers were magnificent; they urged on the audience a new set of expectations about the relevance of animal studies. One by one they spoke succinctly about their animal researches and went on to offer their results as a standpoint from which to view some aspects of human life. In each case, the animal research caused the investigator to ask some probing questions about how human investigators thought about human behavior. If the order and timing of events are critical in establishing harmonious mother-infant relationships in rats, what about order and timing as determinants of the

mother-infant relationship in man? If birds have some sort of genetic limit on the songs they can learn, what might be the genetic limits upon human language learning? If environmentally deprived monkeys rock rhythmically in their cages, what does this suggest about the etiology of autism, a syndrome which sometimes includes similar rocking behavior?

What made these presentations so useful was not the superficial resemblance they pointed out between animal and human phenomena; what made them of interest was that the animal phenomena they described indicated a contradiction inherent in scientists' ideas about the world. Their presentations showed that the notion of experience deprivation is simply a contradiction in terms: how is one to measure the passage of time inherent in the concept of deprivation except in terms of some sort of experience? To deprive of one experience is necessarily to provide with another experience, and one is hard put to tell if the odd behavior produced is due to the experience that was withheld or to the experience that was provided. The "experience-deprived" animal has not learned "nothing"; on the contrary, it has usually learned something that is incompatible with effective existence in the normal, "experience-rich" world.

By this time, a less precarious defense of the crow against the monkey was beginning to suggest itself: a defense on epistemological grounds. A traditional view of the scientist holds that he is collecting information about the universe and that the more he collects, the more accurate is his understanding of the universe. According to this sort of view, the pursuit of science is like pasting tiny trading stamps of knowledge in giant books of ignorance. The partially filled books are passed on from generation to generation, and every once in a while somebody fills a book and cashes it in, sometimes for a Nobel prize. (Darwin is a particular hero of this school of thought, because he is one of the few scientists who has ever singlehandedly collected enough stamps to fill a book.) The "trading stamp" view of science fails to take into account the fact that most people who collect information have ideas about the meaning of that information, and that for Charles Darwin, in particular, the idea of evolution was family history long before the voyage of the *Beagle*. A more modern view of science holds that we always have ideas about the world no matter how ignorant we may be, and that we seek facts when our ideas come into conflict. According to this view, our ideas about nature become the central figures in science, not nature itself.

The insight that science arises from conflict among concepts is a useful one for explaining characteristic patterns of birth, growth, and decay in the sciences. Initially, a phenomenon is brought sharply into focus by its relationship to a conceptual problem. A first generation of imaginative investigators is attracted to the phenomenon in the hope of casting light on the

related conceptual issue. These investigators generate a lot of argument, a little progress, and a lot of publicity. Then a second generation of scientists is attracted, who are drawn to the problem more by the sound of battle than by any genuine interest in the original issue. By then the conceptual issue has been straightened out, the good people have left, and those who remain devote their time to swirling in even tighter eddies of technological perfection.

If one takes this view of science, then one sees the whole "closeness" argument as spurious. A study does not get to be significant because it is on an animal phylogenetically close to man; it gets to be significant because it relates to an important question. Even studies of man are insignificant if they do not relate to an important question. The early field studies on primate social behavior were not significant because their subjects were close to man; they were significant because the scientists were addressing themselves to interesting ambiguities in their conceptions of man. Previous generations of anthropologists had anchored their conceptions of man as a unique species upon particular features of man's way of life, such as language, tool using, firebuilding, meateating, and upon certain anatomical specializations, such as a pelvis for erect walking and a large brain. The data discovered by the early field workers indicated that nonhuman primates possessed many behaviors previously thought to be unique to humans, and that hominids possessed many characteristics previously thought to be beneath them. These data raised questions concerning the borders of our conceptions of man: hominid, anthropoid, and primate. Do monkeys communicate through vocalizations? (Yes!) Do they normally use a language? (No! On the contrary, most mature nonhuman primate communication systems are rather unelaborate.) Do primates eat meat? (Yes, sometimes! And under circumstances particularly conducive to theories about the evolution of human meat eating and weapon using.) Do primates ever use tools? (Yes, but in ways that are so uninteresting as to cast doubt upon the importance of the original question.) To what extent do primate social organizations resemble man's? (To no great extent; prairie dogs, wolves, and beavers provide parallels that are equally interesting.)

As these questions have been answered, no new ones have been forthcoming. Some may regard this absence of emotion-laden questions as a healthy sign of a disciplined and selfless empiricism. To me, it is a sign that primate social behavior has skipped from infancy to pedantry. A good conceptual problem is a resource, like a virgin forest. Theoretical development, like forestation, must be planned for. Each author who solves a theoretical problem has a responsibility to seed the literature with several more. Primate social behavior is like a forest that has been sloppily and opportunistically logged. Some important discoveries have been harvested, and their

theoretical stumps stand rotting. Strewn about is a tremendous quantity of empirical litter and secondary growth. No new important growth is establishing itself.

Why has so vigorous a field suffered so rapid an atrophy? Possibly because of its uncritical acceptance by the public and by funding agencies. Whereas ethologists of other species have had to justify their researches in terms of important conceptual issues, primatologists have always been able to fall back on the "closeness" argument. So, then, why should such a renegade primatologist want to study crows? Because at this time crows present a greater potential challenge to our ideas about man. What is a language? What sorts of information communicated in what sorts of ways constitute a language? What is a society? What sorts of components formed in what sort of a structure make a society? How necessary is the Big Brain to the possession of language and society? Is the Big Brain essential, or can a language and a society be observed in an organism with a brain the size of half of a small walnut meat?

Of course, the presence of such challenges to basic conceptions of man and nature is not unique to the crow. Indeed, a vast array of lower species may provide equally interesting challenges. A fall from the top of the *scala natura* will always be a fall *into* grace, if the faller lands with his feet firmly set on fertile theoretical ground. This ethologist's lament is thus a call for a revolution by the phylogenetic proletariat. "Rise up my fellow ethologists of the lower orders! No longer need ye doff your caps to the evil-tempered and incontinent monkey. Go forth with a light heart and study the crow, the frog, the paramecium, and the newt. The meek shall inherit the earth!"