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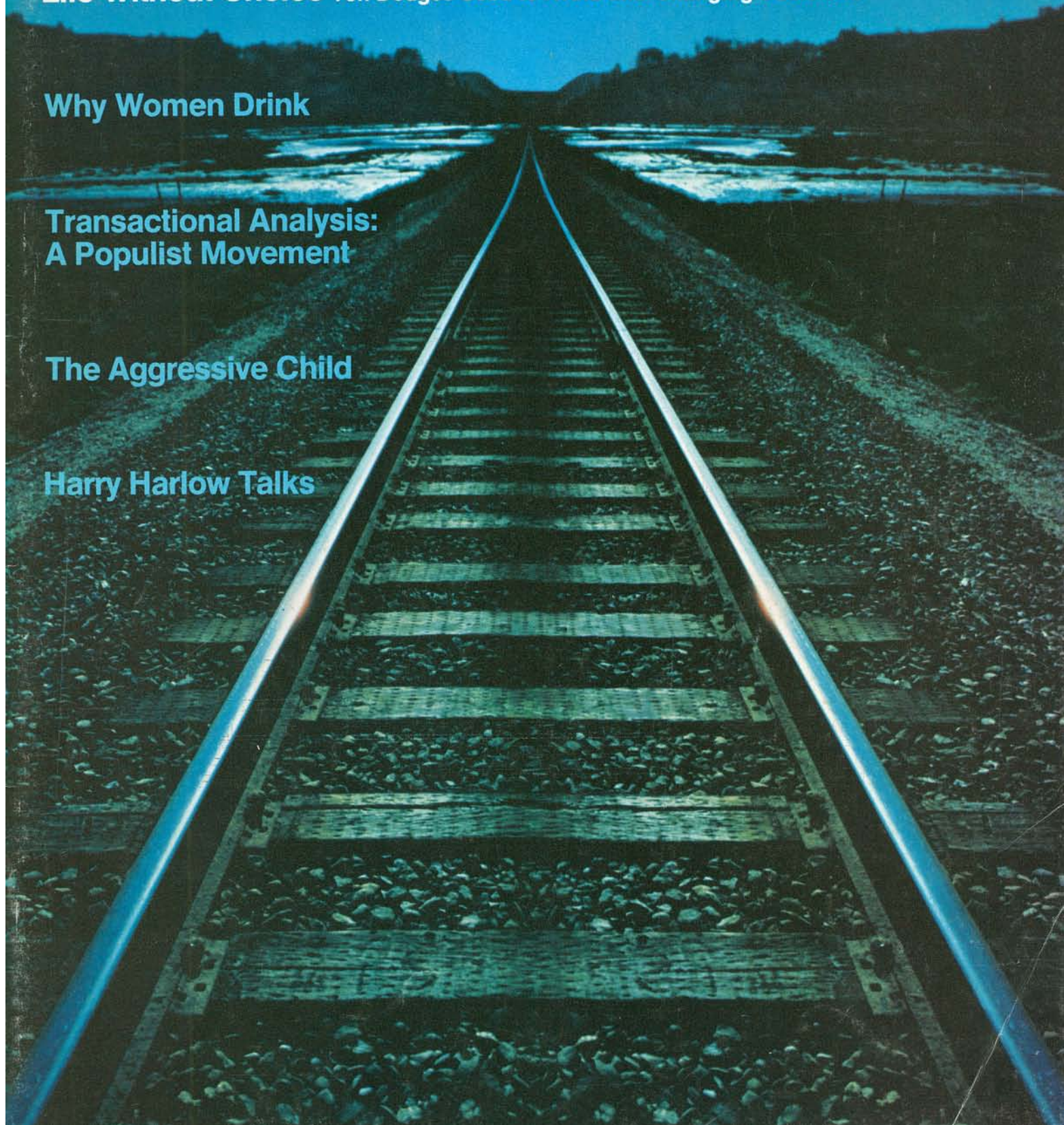
Life Without Choice Ten Dodges Used to Avoid Life-Changing Decisions

Why Women Drink

**Transactional Analysis:
A Populist Movement**

The Aggressive Child

Harry Harlow Talks



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APRIL 1973/VOL. 6, NO. 11

THE MAGAZINE ABOUT PSYCHOLOGY, SOCIETY AND HUMAN BEHAVIOR

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“Harry, you are going to go down in history as the father of the cloth mother.”

A conversation, by way of collision, with Harry F. Harlow

by Carol Tavis



Carol Tavis: Recently the question of how well we can generalize from monkeys to human beings has taken on political dimensions. Feminists, for example,

criticize your work because it implies that human infants need full-time mothers.

Harry F. Harlow: That is obviously true. Look, psychologists are learning mad. They overinterpret all phenomena as being due to learning, but this is incorrect. We have found sex differences in monkeys that can be explained only in terms of maturation. If you don't believe that God created women to be mothers and essentially nothing else, let me prove it to you. Gary A. Griffin took preadolescent males and females, totally devoid of gonadal urges, and observed their responses to young infants. The females loved the infants immediately because they were females, and they weren't fighting about it. The males were either indifferent or mildly abusive. Here in an environment where all other factors had been controlled, the girl monkeys knew that they had been created to be mothers. The boy monkeys thought that

babies were for the birds. Female birds would agree.

Tavis: But your own research shows that there may be nothing instinctive about the so-called maternal instinct; your female isolates were lousy mothers. They had to learn to want infants and care for them.

Harlow: I'll take you apart. Look, we don't deny that apes and monkeys learn. They are bright, and they learn continuously. As soon as a situation changes, or a new ability matures, learning is overlaid on innate qualities, and it becomes difficult to tell them apart. But the innate components are there. God created two species, one named *man* and the other named *woman*. I can even tell you the difference between them. Man is the only animal capable of speaking and woman is the only animal incapable of not speaking.

Tavis: Women's liberation will get you for that one.

Harlow: They already have at the University of Chicago. I showed some slides—they didn't deprecate the female, they merely told the truth. For example, the only behavior that matures earlier in man than in woman is aggression; this is a clear-cut finding and it is the way it should be. Or take play. The sexes play

Harry Harlow: A Passion for Primates



He is a small man in a large, neatly cluttered office. Pictures of monkeys are everywhere; official slides of myriad experiments vie with photographs of favorites. Wooden monkey toys perch on sober volumes of monkey lore. The walls are covered with Harlowiana: plaques, certificates, awards, and a photo of Harlow and President Johnson shaking hands, all testify to the man's long and meritorious career.

Harry Frederick Harlow got his Ph.D. in 1930, at the age of 25, from Stanford. His first appointment was at the University of Wisconsin, where he has been ever since. "I almost accepted an offer from Texas once," he says, "but I chickened out at the last minute. Wisconsin has been too good to me." Harlow's work began ingloriously at the local zoo, since the University had no laboratory facilities for the young experimental psychologist. Soon, however, primate research found a two-story building "on the wrong side of the Milwaukee Railroad tracks" and studies began in earnest. Harlow supervised and developed programs on primate learning, the effects of cortical lesions on learned behaviors, and the study of primate motives, for which he is most famous.

On one excursion out of Wisconsin, from 1950 to 1952, Harlow served as the Army's Chief Psychologist, and helped to create HumRRO [Human Resources Research Office], which conducted research for the Army at George Washington University. "I felt very incompetent for the job," he says now, "but it was the only thing I could do. I missed being drafted in World War II, and at that time if you didn't serve your country you felt guilty. So when they offered me a research position to study battle noises I took it. I thought it was absolute nonsense. Hell, they didn't care about battle noises."

Harlow quickly moved to other research topics. After an Army course in radiation physics, he did several experiments on radiation tolerance in monkeys. Harlow even joined in the brainwashing controversy that had emerged during the Korean War. He wrote a paper that explained the phenomenon with learning and conditioning principles, and concluded that brainwashing wasn't as mysterious as many believed.

After two years in Washington, Harlow asked Wisconsin to extend his leave of absence. They told him to "come home or resign." So he went home to a professorship, and thereafter became Director of the University's Primate Laboratory.

Harlow worked with his second wife Margaret (Peggy) from their marriage in 1948 until her death in 1971. Peggy had received her Ph.D. in psychology from the University of Iowa in 1944, and worked as a project associate at the Primate Research Center and as a lecturer in educational psychology. The Harlows collaborated on many studies and papers, and shared other professional responsibilities as well. While Harry was editor of the prestigious *Journal of Comparative and Physiological Psychology* [JCPP] from 1951 to 1963, Peggy was assistant editor. (Last year, Harlow remarried his first wife, Clara. He has two children from each marriage.)

In the mid-1950s the Harlows turned to the study of affection, a subject that many other scientists thought belonged to fuzzy-minded clinicians or to poets. The Harlows began with the first love relationship, that between infant and mother. To control for the *importance* of a mother in the infant's development and to control for the *kind* of mother, they isolated newborn monkeys and reared them with various parent-surrogates.

The Harlows created a variety of substitute mothers: some made of cuddly terry cloth, others of frigid wire; some that could rock, others that remained immovable. They found that the critical factor in the infant's love for its "mother" is contact comfort—not breast-feeding, as many theorists had argued. The baby monkeys consistently preferred the terry cloth mother to the wire mother, even when the latter provided milk. "Certainly," Harlow concluded, "Man cannot live by milk alone." Most of these babies, however, grew into neurotic, asocial adults. "Deprived primates," summarizes Harlow, "would rather fight than flirt."

The Harlows went on to study other affectional systems in monkeys: maternal, peer, heterosexual, and paternal. They discovered that while the mother-infant bond is important during the early months, the infant-infant bond is more essential—it can even counteract the effects of maternal deprivation. Harlow raised infants without mothers but with playmates, and the monkeys adjusted normally. But when he raised infants with only mothers and no playmates, the monkeys grew up to be socially abnormal. While Harlow acknowledges that primates have a tremendous ability to learn, his work has sought the biological bases of behavior as well. His research shows that affection, fear, and aggression have major innate components. The ability to affiliate is present at birth, while the other two emotions are absent, thus permitting the infant to become attached to others. Eventually, fears mature; the infant stops trusting everything in its environment. Still later, aggression appears. This sequence develops in an orderly fashion—"regardless," writes Harlow, "of the nature and timing of the infant's experiences while these emotions are maturing."

Harry Harlow is noted for his sarcasm and wit, though people seem to disagree on whether he is funny or infuriating. At least his papers are never boring or pompous, even when he gave his presidential address to the American Psychological Association in 1958. That paper, "The Nature of Love," is as full of warmth and whimsy as it is of serious scholarship. His 1962 farewell editorial in *JCPP* spoofs psychologese, obtuse reports, trivial research, and the publish-or-perish syndrome in one blow.

When I set out for Madison, Wisconsin, and the Primate Labs on Charter Street, I had only an inkling of what to expect. "He won't take you seriously," a friend had warned me, "he never treats women seriously." "True," said another, "but then he doesn't take *anyone* seriously." "I'm not sure about that," said a third. "I think he's very serious about his monkeys."

So Harlow and I sat down for two sessions of conversation and coffee. He turned out to be nowhere near as intimidating as I had anticipated, but every bit as sarcastic. At first, I wasn't sure whether his antiwomen comments were a true reflection of his beliefs or an attempt to be provocative. So I followed the old give-a-man-enough-rope principle, and chose not to get too ruffled while he ranted. I apparently passed test one, and was pronounced a "fine figure of a woman."

Harlow must have asked me at least as many questions as I asked him: how was my love life, where had I studied, how could any man of mine let me fly around the country this way, was I going to have children, etc. Then he marshalled me through the laboratories, pointing out the nuclear-family apparatus and other testing cages. Rod Kamitsuka, the photographer with me, commented along the way that the big-eyed little creatures were really most endearing. At one point he took a shot of a placid monkey that had electrodes implanted in its brain. Instantly, the monkey became wildly excited and fearful, scampering around its tiny cage. For the rest of our tour, Rod felt as though he had committed a crime.

Then Harlow showed us some of the isolated infants, cowering in lonely terror in their cages, and explained that my sympathy response was a natural result of my femininity. "I'd be worried about you," said the expert on primate sex differences, "if you *didn't* think they were cute."

—Carol Tavris

"Being a smart woman, she knew it was better to marry a man and lose a job than hold a job and not marry a man."



differently. Males play rough, and females play soft and sweet and gentle. They sit quietly on the side lines saying mean, catty, nasty things about other women. This is true even if monkeys grow up without members of their own sex to observe. Maturation brings out the capabilities that God intended.

Tavris: I was a tomboy as a child, and I don't like to say nasty things about other women. Does that mean I'm not a female, or that I'm not mature?

Harlow: You prove my point. Physical strength is the one trait in which man is superior to woman, and speaking is the one trait in which woman is superior to man. Now consider what happens when a couple argues. The man tries to *talk* to the woman. The stupid fool, he can never win. Are you married?

Tavris: Do you have to be married to argue with a man?

Harlow: Don't mind me. I have been married twice but both of my wives have been too bright to be sucked in by women's lib. My wife Peggy probably had more of a gripe against female discrimination because she lost her job [assistant professor in psychology] when she married me. But being a smart woman she knew it was better to marry a man and lose a job than hold a job and not marry a man.

Tavris: Why shouldn't women have the opportunity for both, as men do?

Harlow: Peggy eventually attained both. At the end of her life she was clearly recognized for her own contributions and she became a full professor of educational psychology.

Tavris: Did Peggy ever feel resentful, or did she ever try to compete with you?

Harlow: There was no competition at all. She knew that I was better at creating research and that she was better at presenting it. My only complaint was that she cut out a line from a paper I gave on sex: "I hope you do not think this is a sa-

lacious paper. It is only a little sermon on the mount. I gave it on Sunday." Otherwise, we got along fine. She was a wonderful editor and a wonderful female.

Tavris: If you got along so well together, why shouldn't other couples in the same field be permitted to work together? Why should nepotism rules exist?

Harlow: Well, the closer you are, the more difficulties you are likely to have—and the more adverse feelings will arise among colleagues. I don't think it is ideal for husbands and wives to be in the same department, and Peggy and I were not. She was not listed as a member of the psychology department until the last departmental budget presented after her death. They thought that made the percentage of women look better.

Tavris: In your article some years ago for *PT*, you said that the existence of segregated male and female clubs is based ultimately on anatomical differences. Do you believe that close, nonsexual friendships are impossible between men and women?

Harlow: Well, the interests of the sexes are basically different, because of different innate capacities, which learning exaggerates rather than minimizes. So even if husband and wife are happily married she will still go to afternoon bridge parties and he will go out and bowl. Which is just fine. Even so, if you want to fondle women other than your wife, wait until you are over 65.

Tavris: What do you think of current efforts to modify the nuclear family, so that the woman could spend less time mothering and the man could spend more time fathering?

Harlow: I certainly would favor such flexibility if it can be done effectively, so that a reasonable part of the woman's time is free to do other things besides changing diapers and washing pots. But I reject the assumption that in order to liberate the woman you must penalize

"I was delighted when Premack showed that chimpanzees could learn, with patient training, the basic rudiments of comprehensive language."



the male. I resent this. This is not women's lib, this is women's glib.

Tavris: Why does it penalize the man to free him from pressures to achieve, to permit him more time with his children?

Harlow: There are always going to be different styles. I have said to many females, if you are stupid enough to marry an academic man, don't think that it is going to be easy. At best you will be a handicap. Actually I have no right to make that statement, because a great deal of what I have achieved is through the assistance of two very bright wives.

Tavris: I'd like to return to the question of generalization. You have said in many of your articles that you are not interested in any monkey research that does not have bearing on human beings.

Harlow: The only reason for using monkeys rather than rats is that the data will generalize better to man, and I have a basic fondness for people. I feel this is my obligation. Now I don't ask anyone else to study monkeys. Others can study the cockroach or the louse if they feel more at home with those forms.

Tavris: But the problem is defining the limits of such generalization. For example, the rhesus male is not much of a father. But according to Gary Mitchell's research, there is enormous variation across primate species, from some in which the father is totally indifferent to the infant, to others in which the father takes on most of the responsibility for infant care. I agree that monkeys have much in common with human beings, but which monkey species shall we choose?

Harlow: Every animal is worth studying if you can get him, but you'd better have a good reason. We use rhesus monkeys because they were the first monkeys over which one could have disease control. And they were the first monkeys that one could breed at will—our will, not theirs. Finally, the rhesus monkey is

a standardized Old-World monkey. New-World monkeys are far different creatures, and show more variability. And apes pose other problems. The chimpanzee is too big, too expensive, and too dangerous. Those animals you see on television are only three years old.

Tavris: You once wrote that one of the limitations of monkey studies was that monkeys have no symbolic language. Have David Premack's and R. Allen and Beatrice Gardner's research changed your mind?

Harlow: The Washoe study was brilliant. There have been many attempts to teach a chimpanzee expressive language and they all failed, because the researchers didn't realize that the chimpanzee just did not have the unlearned responses for human verbal language. The mouth and throat structure of simians cannot possibly evoke language sounds. The Gardners' great accomplishment was to use the unlearned responses that chimpanzees do have, of gesture. Then they had no problem in getting the animal to develop language.

Tavris: Do you think that Washoe's "speech" will meet Piaget's definition of symbolic language?

Harlow: I'm willing to bet it won't, saying nothing against the Gardners. Actually, there is no sharp cut-off between symbolic and nonsymbolic language.

For countless decades no one could get a chimpanzee to solve as complicated a learning test as the ones monkeys regularly solved. It was inconceivable that the monkey was brighter than the chimpanzee. I was delighted when Premack showed that chimpanzees could learn, with patient training, the basic rudiments of comprehensive language. A nice study found that an elderly aphasic stroke victim was able to remaster some basic language skills with the Premack technique. This shows a generalization from the chimpanzee to the human

being. That is intriguing. But you are never going to get a monkey to demonstrate our capability of learned expressive language, whether you use the Gardner or the Premack technique. Probably the chimpanzees like it that way too.

Tavris: But Washoe, it seems, is able to ask questions in novel situations and uses sign language creatively.

Harlow: That's nice. Chimpanzees have been signing to each other for countless millennia. If they learn a different sign language, they will use it.

Tavris: All right, let's turn to the other factor that you said limits monkey studies—rhesus males have no real father roles. When Mitchell paired an adult male with an infant, he found that their affectional bond increased over time. The adult learned to be a good father, we might even say, when the mother was not available.

Harlow: Peggy and I could never find anything in the paternal love system of the rhesus, until she created the nuclear-family apparatus. This consists of four large cages, each containing an adult couple and their offspring. Small passageways permit the infants to run into any cage they want to join their peers, but the adults stay put. It is a brilliant creation of a brilliant woman.

This apparatus turned out to be an ideal way to measure father love, because it provides a more normal life-style for the monkeys than we had when Gary Mitchell was here. The fathers are affectionate, but not like the mothers. The male's primary role is to protect the group, and not let anyone abuse the infants. He will allow the infant to pull his ears and hair without retaliating, but he is not as playful as the mother and he doesn't show the real maternal responses of cuddling, holding, and educating the babies.

The mothers have two basic func-

"There is no good study on communal living that does not show that it produces children with mildly—or even extremely—aberrant personalities."



tions: to love the infant, and eventually to urge it to go out and play with its peers. There are many reasons why the male should not try to be a female. He is anatomically inferior. I have often said that the best way to be a mother is to be born a woman.

Tavris: The family apparatus keeps couples in a sort of enforced monogamy, which is not the way rhesus monkeys live in their natural habitat. Don't cages tend to produce behaviors that do not occur in the wild?

Harlow: That apparatus probably provides a better environment than the wild. For example, we have been trying to find out what social isolation does to learning. Over all these years of testing, we could get no impairment in intellectual ability at all, even among completely isolated monkeys. But most recently we discovered that a socially enriched environment—the nuclear family apparatus, which provides ample association with mothers and fathers and playmates and toys—*improves* learning ability on complex tests. And the monkeys are completely healthy and sexy at maturity, the best indication that they have had a normal environment.

Besides, what is so damn good about the wild anyway? Look, you were raised in a laboratory environment. You wouldn't survive in the wild. The feral environment is pretty bad.

Tavris: Yes, but some behaviors occur in the wild that we would not see in a laboratory.

What about Jane Goodall's observation of toolmaking among chimpanzees?

Harlow: Jane Goodall observed toolmaking in the wild 40 years after it had been demonstrated in the laboratory. Goodall's observation was remarkable, but only because no one would have guessed that an animal raised in a deprived feral environment—and I *mean* deprived—would show toolmaking behavior.

Tavris: Does this mean that you are opposed to increased funding for field research?

Harlow: Oh, no. Sherwood Washburn, who is a physical anthropologist and the first to write meaningful papers based on field research, said that you can't put field and experimental work against each other; each makes its own contribution. Of course I would agree even though I am less interested in field contributions than in laboratory endeavor.

Tavris: Does your success with the family apparatus suggest that a similar communal arrangement would be beneficial to human beings?

Harlow: In the apparatus the male and female live in blissful monogamy. Each infant has its own parents, and its relations with them are much warmer, much more important than its relations with the other adults.

There is no good study on communal living that does not show that it produces children with mildly—or even extremely—aberrant personalities. The Russians tried it and quit. You need both mother love *and* peer love. The Jewish kibbutz did not completely dispense with nuclear-family relationships, remember.

Tavris: Then you think marriage and the family will carry on?

Harlow: I don't think anyone has found a better system. Now the Polyynesians developed this technique of promiscuity until marriage and rigid monogamy thereafter. We already have incorporated the first stage of their method in our culture.

Tavris: Let's move back a step. What sparked your interest in psychology?

Harlow: Well, I went to a very inadequate high school. The first course to stimulate me intellectually was a freshman course in zoology that I took at Reed College. But they made me dissect

a dead frog and I *despised* dissecting frogs. So I decided to find a science that was like zoology but that didn't specialize in dead frogs. That was physiological psychology, thanks to Walter Miles.

I partially worked my way through college by running rats for Miles (he gave me a footnote in his paper saying that I was an able young man). In fact, although I am thought of as a monkey psychologist, I'm sure that I have spent more man-hours studying rats than any two living psychologists. This proved useful when I was editor of *JCPP* [*Journal of Comparative and Physiological Psychology*]. If somebody tried to push a rat paper down my throat I'd tell him off.

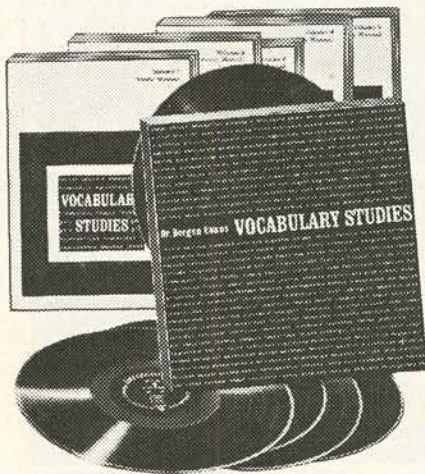
Tavris: How did you happen to choose graduate school at Stanford?

Harlow: My brother was going to law school there, so my parents thought it was a likely place. They telegraphed Stanford 10 days before the semester started and asked for my admission. Stanford said no, flatly, but allowed me to take a competitive examination. I went there in fear and trembling—until I took one look around the room. Three-fourths of the people there were potential football players for Pop Warner's team—enormous hulks. I said to myself, "If I can't beat them I should quit now." I was admitted.

Tavris: Whom did you work with at Stanford?

Harlow: I started by running rats for Calvin Perry Stone, who ran the comparative-psychology show. Stone and I got along fine because we played tennis together. He was a good doubles player, but a little erratic for singles. Stone was an incredibly unimaginative, methodological man. He said, "We will push the domain of science forward inch by inch by inch"—and that is exactly what he did. Stone was one of the least-known APA presidents, a wonderful person. He was nice to me—but he was unpleasant to

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“We think you are a bright young man but we are convinced that you will never be able to speak in public.”



some others. That was just his way. Once I was almost bleeding to death from a lab accident and met him in the hall. He talked to me endlessly; I wondered how long it would be before he would notice the blood all over my hand and my gown. Finally he looked down and said, “Oh, bitten by a rat, eh?” You see, he was methodical, he wasn’t jarred by the fact that a person was bleeding to death.

Tavris: But he was wonderful to you?

Harlow: Look, he was completely sympathetic. He probably went out and bawled the hell out of that rat.

Tavris: Did you know Lewis Terman at Stanford?

Harlow: Terman was entirely different. He was out to find the creative and he took great pride in that. Terman knew something about everything, and he ran beautiful seminars. When I’d been in graduate school a year and a half he called me in and said, “We think you are a bright young man but we are convinced that you will never be able to speak in public.” This was not a criticism, but an accurate appraisal. So he said, “I think we will try to get you a job in a junior college.” A week later he told me he had been unsuccessful. “You have no education credits,” he explained, “so you are condemned to take a Ph.D.”

Tavris: Why were you so timid?

Harlow: First because I had a speech defect. I couldn’t pronounce th’s and r’s. In college I had to take a French course that just traumatized me. The first day this bitch got up and said “now in French we pronounce ‘r’ in three different ways.” And I thought, “Oh my God, for 17 years I failed to pronounce it one way in English.” I was destroyed.

Tavris: What cured you?

Harlow: Teaching elementary psychology. It’s the best possible speech and timidity therapy you can have. At first, of course, the students would make fun

of my pronunciation because I couldn’t differentiate *th* and *f* and I could not pronounce *r*. I still can’t but I don’t care any more.

Tavris: What other influence did Terman have on you, other than to condemn you to a Ph.D.?

Harlow: For one thing, he named me. He gave me the name Harlow—my real name is Israel.

Tavris: I know that many Jewish psychologists changed their names about that time. What prompted you to do so?

Harlow: Terman called me in as the year was ending, a gloomy year called 1930. And he said, “I am sorry to tell you this, but it doesn’t look like we can place you next year. The problem is that we have lost two good opportunities for you because your name is Israel.” You can’t believe the depth of Jewish discrimination at that time. I don’t want to imply that I was persecuted, because I wasn’t; but with the name Israel and being a timid boy, I certainly had seen discrimination. So Terman recommended that I change my name. Ironically, I wasn’t even Jewish.

Tavris: I beg your pardon?

Harlow: No, Gentile for generations. An aunt traced the error back to 1753, and found an ancestor who had been buried in a Jewish cemetery. I often wondered where the family got any brains.

Tavris: So Gentiles didn’t accept you because you had a Jewish name. Did the Jews accept you?

Harlow: Oh no, they could see my cultural background show right through. By the time I finished Stanford I had learned to hate both Jews *and* Gentiles. That helped overcome my timidity too, let me say.

Tavris: How then did it happen that Terman named you?

Harlow: After his dire warning that I wouldn’t get work, I got a telegram from Wisconsin, offering me a job as assistant

professor for the fabulous sum of \$2,750 a year. Terman said he was very pleased, but still felt my name would hurt me. "Well," I said, "I have two semifamily names and you can pick one: Crowell and Harlow." "I like Harlow better," he said, "I'll take that one." So I became a Harlow. I guess I'm not alone. Once a man called me up and said he was looking up the Harlow ancestry. I said I was sorry, but I had changed my name. "Oh heavens, not again," he replied. "Everyone named Harlow that is worth a damn has changed his name."

Tavris: If you spent your time at Stanford running rats, how did you get into monkey research at Wisconsin?

Harlow: At Stanford I had never seen a monkey and had never wanted to see a monkey. When I got to Wisconsin I asked my major professor—Dr. V.A.C. Henmon, the nicest man in the world—where the animal laboratory was. He told me that they had just torn it down, but that there was a beautiful new primate building in the Vilas Park Zoo. I was insulted. Testing animals in a zoo? But I went over there, and with the help of some good students, we were enormously successful. After that I never wanted to run another rat.

Tavris: For a number of years your primary interest was in physiological and learning experiments. How did you move into motivational and social-psychological questions?

Harlow: I had never considered any research having to do with social psychology. I got into the surrogate study for which I am known through an artifact of my learning research. We were studying the maturation of learning, which you can't do unless you have animals of all ages available, even tiny babies. We gave the babies soft cheese-cloth diapers to hold, so that their feeding reflexes would not become confused. We couldn't believe how attached those



Ana Maria is always a little hungry

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35 YEARS OF HELP WITH A HUMAN TOUCH

"I think that I am a soft-hearted person but I never developed a fondness for monkeys. Monkeys do not develop affection for people."



infants were to the diapers. In retrospect, it is incredible that we did not recognize the social implication of those experiments. We were completely stupid, fixated on learning. The light dawned for me on a plane trip from Washington to Madison.

Tavris: What happened?

Harlow: Somewhere over Detroit I realized that I was sitting next to a cloth-surrogate mother—an inanimate female with a wire body covered by terrycloth. So I came back and tried to find a graduate student to work with me on maternal deprivation, and by God I couldn't find anyone who thought it was a researchable topic. Finally Bob Zimmermann saw the light and joined me—he made the apparatus of the surrogate mothers. Later the people who had the chance and muffed it were bitter.

Tavris: Many critics of your classic research have argued that taking infants away from their mothers was cruel and that the results do not justify the cruelty.

Harlow: I think that I am a soft-hearted person but I never developed a fondness for monkeys. Monkeys do not develop affection for people. And I find it impossible to love an animal that doesn't love back. Some of the female researchers have become fond of the animals, but I think this is a mistake. You must be objective and try to avoid personal feelings. Of course some researchers have favorites; some of the monkeys are brighter or easier to work with than others.

Tavris: In a paper you wrote for *Nation's Business* on creativity, you cite the Soviet Union as a state that represses its researchers, and America as a country that permits free research. Do you still hold this view?

Harlow: Well now, I don't know many Russians and I have never been to Russia. I think they censored research

more in the social sciences—and they made Trofim Denisovich Lysenko's genetics a social science in this case—than in physics, chemistry and engineering. My guess is that they are just as competent as we are today.

As for our own creativity—that's impossible to define. What is creative one year is not creative the next. But most good schools encourage it. You get a research grant and you had better be creative or you won't last. Take Abraham Maslow, who was my first Ph.D. You see how far you can go if you are well trained in primate research! Abe did research on dominance that has remained the best piece of research for 30 years. Now that is creativity, when you can work with nothing and make a great scientific breakthrough.

Tavris: Has the quality of graduate research improved since you started out teaching?

Harlow: Enormously. If you doubt it all you have to do is to read the journals of 30 years ago and compare them to the journals today. Of course, it is possible—I don't really mean this as a criticism—that in an effort to obtain statistical significance they harden the studies sometimes. Still, statistics are no handicap as long as you are sophisticated about them—or as long as you have sophisticated graduate students.

Tavris: Speaking of journals, let's discuss your experiences as editor of *JCPP*. As a good writer yourself, what is your attitude toward journalese?

Harlow: The goal of a scientific paper is never to put in a single wasted word. If you are writing for a different audience, your goal is to put in as many wasted words as you can, for effect. I have nothing against journalese. Now my wife Peggy was the first managing editor for the APA journals and she could write both ways.

Tavris: Your last editorial before you

left *JCPP* was a very funny parody of journal style. You criticized triviality and obscurity of findings.

Harlow: I was a brilliant editor. I would read the paper and decide whether to accept it. Then I gave it to my wife Peggy and she did all the work. She would change or rewrite parts, if necessary. She made my being an editor tolerable. But I am not the only person to work with a wife. I pointed out to Bill Estes, who followed me as editor of *JCPP*, that his wife is as bright as he is and could follow in my family pattern.

Tavris: And did the wives get proper credit for their contributions?

Harlow: To be honest, not what they deserved. But that was a tradition established in APA, and APA got better editors to work with than they could have gotten otherwise. Besides, why would a very bright woman want credit?

Tavris: Why would a very bright man want credit?

Harlow: The answer to both questions is yes.

Tavris: I see that you're going to be evasive, so I'll try a different issue. What current research are you doing?

Harlow: A couple of years ago I took a wild gamble and devoted a large part of the laboratory to psychopathological research. We began with depression because we already had some data from the love studies. There are two separable aspects of the depression program: one is the attempt to produce a behavioral syndrome that we can manipulate; the second, which depends on the first, is the attempt to study the biochemical variables. We've got the behavioral syndrome—I just looked in the mirror one morning and then I knew we had it.

Tavris: Your research should help counter the popular theory that depression is aggression turned inward.

Harlow: It is incredible how widely held that concept is, and there is vir-

"You see here the difference between love and lust. Sex without antecedent affection in the primate is a dim and gloomy thing."



tually no hard data to support it. Why isn't paranoid schizophrenia "aggression turned inward?"

Tavris: How do you create depressed monkeys?

Harlow: We put them in a small closed chamber, shaped like an inverted triangle—a narrow base and a wider top. You might say it is a modified form of sadism.

Tavris: You said the word yourself.

Harlow: It really isn't a sadistic device. The animals show normal appetite, no weight loss, no abnormal susceptibility to disease. We did not want to impose physical stress upon mental stress.

Tavris: Isn't inducing mental stress sadistic?

Harlow: Look, you will never learn the factors that produce depression or other pathological syndromes in the wild. You will never find the biochemical variables underlying such syndromes in the wild. Sure, you can get some crude information for evening chatting, and you would have plenty of evenings; but you will never get definitive data by observation. Take play. You could study play in the field for millennia and no one would have found its meaning. But our laboratory work gave the basic answer and is incontrovertible.

Tavris: What did you find?

Harlow: For one thing, that play is probably the best therapy. We know this is true for monkeys and it would probably be true for human beings if psychoanalysis understood it. But it never dawned on Freud that there were love systems other than the maternal. It has been said of Freud that after he escaped from Nazi Germany and went to live with his daughter in England, he recognized the power of peer love when he watched his grandchildren at play. Apparently he was too busy to watch his children. But he was too old to change his theory. Recently my wife Clara sug-

gested that the critical variable in causing the depression in these chambers was that the animals are denied motion play. They just cannot move about. It finally crossed my cement-filled brains that this might be a big factor. So we are going to study the development of motion play. We do know that play is terribly important. Young monkeys will play with anything of any age and any sex.

Tavris: How does your play therapy work?

Harlow: Steve Suomi did one major study with monkeys that had been totally socially isolated from birth to six months. We were positive that such a protracted period would eliminate the animal's social responses. In fact, if you take a monkey out of isolation and leave him in a normal situation he gets worse. Fears mature and then aggression matures, and they run their evil course no matter what you have done. Suomi put these isolates with three-month-old normals—younger, so they would not frighten the isolates, but old enough to have a complex repertoire of play behavior. And the isolates were completely rehabilitated.

Tavris: Are they sexually normal, too?

Harlow: We don't know that yet, but probably they will be.

Tavris: Were the "therapists" and isolates of the same or opposite sex?

Harlow: As it happened, all four isolates were males and all four therapists were females. Must this mean that women make excellent psychiatrists?

Tavris: Is six months of isolation the maximum a monkey can tolerate and still hope for rehabilitation?

Harlow: Recently Melinda Novak, a very bright female graduate student—there are some if you search—did the same study with one-year isolates. She apparently has obtained total rehabilitation of two animals and successful rehabilitation of two others. This is a

feat—if you don't think I believe in learning, I sure do.

Tavris: Do you believe that behavior therapy—of the sort you use on your monkeys and that is in increasing use among human beings—will make psychiatry obsolete?

Harlow: My wife Clara does.

Tavris: Do you?

Harlow: Being a good friend of Bill McKinney, the psychiatrist I work with, and being married to Clara, I will keep my mouth shut.

Tavris: On the subject of therapists, tell me about Smiley—one of the male monkeys you used to rehabilitate the isolates—and the "rape rack."

Harlow: Well, we were trying to breed our monkeys. Our goal was to get isolate females, who had never known love, and see what kind of mothers they made. Since you have to become pregnant to be a mother, we had a problem. In desperation we created a device in which the female is strapped down. Now the interesting thing was that half of the breeding males regarded the rape rack as the chance of a lifetime and the others wouldn't buy the deal.

Tavris: Not unlike human males.

Harlow: You see here the difference between love and lust. Sex without antecedent affection in the primate is a dim and gloomy thing.

Tavris: Was Smiley successful?

Harlow: I'll tell you about Smiley and his success. Smiley didn't *have* any success. At the end of the summer he was the most frustrated male you ever saw. Then he was brought back to his loving girlfriends and he wouldn't buy them. "The hell with women," he said. But they rehabilitated him.

Tavris: I am much relieved. Why is he called Smiley?

Harlow: It is because when he opens his mouth he shows his enormous canine teeth. The name is cynical.

"We have just started the research, and we are enormously excited by it. Ten years after I am buried someone will become famous. I hope it is the right man."



Tavris: Oh, I thought it had something to do with his sexual prowess.

Harlow: For many years, he was the best breeding stock on the island. Smiley doesn't suffer from gonadal atrophy, I assure you.

Tavris: Speaking of sex, have you witnessed homosexuality among your monkeys? Suzanne Chevalier-Skolnikoff observed a remarkably high incidence of homosexual behavior in stumptail monkeys.

Harlow: I don't have to read her paper to know that it is stupid.

Tavris: Why is it stupid?

Harlow: Because in the '20s there were unlimited reports of homosexuality in monkeys. What does that mean? That males mount males and females mount females? Washburn and Irven DeVore pointed out long ago that these are social signals that have little or nothing to do with sex. There is no information indicating relatively equal social status.

Tavris: She acknowledges this argument but found that male homosexuality was often an expression of generalized affectional ties.

Harlow: Look, the love systems of the monkey, much as I like to talk about them, are relatively transient. How in hell can an animal whose heterosexual love system is transient engage in full-fledged homosexuality? It can't unless homosexuality is better and I have found no evidence that it is.

Tavris: Why does one form preclude the other? Why is bisexuality not the "normal" state? Many primate species engage in sexual acts with both sexes, from infancy through adulthood.

Harlow: Bisexuality is the only natural sex behavior in this sense. Early sex is completely diffused. It can be shaped through learning in any direction whatsoever, but that doesn't mean homosexuality is normal in the sense of biological goal. There are damned few children born as a result of homosexual relationships.

Tavris: What other forms of psychopathology are you studying besides depression?

Harlow: We are trying to create what we call a "second syndrome." We want to produce a schizoidlike syndrome in monkeys in which they will look and behave differently from the depressives. We have just started the research, and we are enormously excited by it. Ten years after I am buried someone will become famous. I hope it is the right man.

Tavris: How are you creating your schizophrenics?

Harlow: We are basing the research on the double-bind theory, that schizophrenia is produced by a mother who gives unpredictable and contradictory messages to the child: sometimes positive, sometimes negative. A key part of the situation is that there is no escape for the child. This theory, of course, was first put forth by Gregory Bateson.

Tavris: How do you create the double-bind situation in the laboratory?

Harlow: The infant is put in a cage by himself, with a surrogate mother. There is no escape for him. The mother is a nice, warm cloth-covered creature who erratically turns frigid and ice-cold. It would be difficult to use real mothers, though they would be ideal. Perhaps in the future.

Tavris: Do you favor the double-bind theory over genetic arguments for the causes of schizophrenia?

Harlow: The evidence coming in from genetics does account for a certain percentage of the variance in schizophrenia but by no means all of it. I think we must conceptualize schizophrenia on many different levels; early experience is certainly critical in many instances.

Tavris: Once you wrote: "If we face our problems honestly and without regard to or fear of difficulty, the theoretical psychology of the future will catch up with, and eventually even sur-

pass, common sense." Has it happened?

Harlow: In the first place I have an enormous regard for common sense. Any time we discover some great thing and it contradicts common sense, we better go back to the laboratory and check it. So I have respect for common sense although I know it is often wrong. Psychologists have always been hampered because human subjects are hard to manipulate and animal subjects are stupid; so when laboratory research consistently surpasses common sense it has gone a long way.

Now part of the problem is that most experimental psychologists—I would guess about 90 percent—are indifferent to any real human problems and their research is not set up to deal with them. They aren't hostile to or actively disinterested in human problems—they don't have enough courage to be that extreme. They are just indifferent. Almost all of our research has been aimed at the human model. You don't pick up a model for love by studying ants and you don't pick up a model for psychiatric rehabilitation by studying hedgehogs. No, all my life I have thought in terms of human models and I have never made any bones about that.

If you are not interested in human applicability then there is no sense in studying monkeys.

Tavris: At an age when many of your colleagues are giving up students, concluding research projects, and resting on their well-earned laurels, you are still involved in work and teaching. Why?

Harlow: My motive for starting the program in psychopathology came from several friends who, within a month, told me, "You know, Harry, you are going to go down in the history of psychology as the father of the cloth mother." That was a tough realization. So now, as the curtain closes, I am making one last desperate effort to erase this career and substitute another. 