Science of the Brain as a Gateway to Understanding Play

An Interview with Jaak Panksepp

Jaak Panksepp, known best for his work on animal emotions and coining the term *affective neuroscience*, investigates the primary processes of brain and mind that enable and drive emotion. As an undergraduate, he briefly considered a career in electrical engineering but turned instead to psychology, which led to a 1969 University of Massachusetts PhD dissertation examining how electrical stimulation of brain regions affects aggressive behavior. Since then, Panksepp has written more than three hundred articles in scientific books and journals, along with the pathbreaking 1998 study *Affective Neuroscience*, in which he detailed the neurology, neuroanatomy, neurochemistry, and functions of the emotional brain. For Panksepp and his students, studying play in animals opens a window into what they and other thinkers have come to call the *BrainMind* or *MindBrain*. He discovered that rats chirp ("laugh") during their rough-and-tumble bouts and that play deprivation is a potent motivator akin to thirst or hunger. Optimal brain development depends on healthy play experiences in early life, Panksepp contends, and he observes that over the long evolutionary haul, play has promoted social bonds and nourished social learning. Currently, Panksepp is Baily Endowed Chair of Animal Well-Being Science in the College of Veterinary Medicine at Washington State University; Distinguished Research Professor Emeritus of Psychobiology, Bowling Green State University; and Head, Affective Neuroscience Research, Falk Center for Molecular Therapeutics, Northwestern University. In this wide-ranging interview, Panksepp notes how play shaped his own experience, discusses his life's work and the context within which he conducted it, and presses for greater recognition of the value of play in psychological research.

American Journal of Play: Professor Panksepp, before we get into the groundbreaking discoveries you have made in the study of play, would you please tell us how you played as a child?

Jaak Panksepp: I will, but it is not a short story. Because accurate autobiographical memories do not seem to emerge until we are about age three or four—when rough-and-tumble play becomes prominent—I will not even speculate about how my mother or other early caretakers played with me and whether I experienced all those wonderful face-to-face interactions and

playful "motherese" communications that are so well recognized these days. All of that is lost in the fog of prepropositional, implicit memories, and no one of my clan is left to provide any veridical accounting of my early life. In 1944, when I was less than a year old, my family escaped from Estonia in front of the advancing Red Army and then slogged around war-infested Germany until the Allied victory. My most explicit early play memories are from our four years as displaced persons, from the end of the war in 1945 to August 1949—just a few months after my sixth birthday—when we got visas to the United States, after moving across Northern Germany from one displacement camp to another.

In those seemingly—but no doubt deceptively—carefree days, when parental worries were only implicitly the worries of a child, I always had an age-appropriate playmate in my brother. He was a year and a half older, heavier and stronger, and usually the dominant one. There were also many other footloose kids in the camps, so I never had any lack of other playmates. We could commonly play in whatever way we pleased until our rambunctious activities displeased the prevailing elders and they wielded some harsh discipline. Those were the days when dads were not culturally discouraged from applying their belts to tender bottoms in order to regulate the natural excesses to which playful childhood energies can lead. Of course, in the free flow of life in camps, parents often had little idea about what their kids were up to.

AJP: And what were their kids up to?

Panksepp: Besides the everyday rough-and-tumble activities that my brother and I enjoyed, several examples of playful excesses come to mind. Once, about a quarter of a mile from camp—I think that was when we were in Merbeck, Germany—we were playing hide-and-seek and king-of-the-mountain in a dump for ruined war hardware, including the skeletal remains of German tanks and trucks. I took a big tumble off the top of a tank and fell on some rubble that skinned my head pretty badly. After I regained my wits, my brother helped me get home, but I was bleeding and wailing all the way.

At one of our next camps, in Oldenburg, where the Estonian sector and the Latvian sector were divided by a soccer field, youngsters would gather on each side of the field holding clumps of grass—still weighted by dirt-clenching roots—as grenadelike ammunition and then start a *Lord of the Flies* sort of battle. Hurling these missiles at each other was simply great

fun—not much different from a snowball fight but much more within a symbolic context of the times. There would be abundant shifting alliances, perhaps to prepare us for the coming real social challenges of adult life.

Of course, childhood shenanigans would easily go too far and offend social mores. There is a vivid example I am hesitant to mention, but I will. The outside latrines in one of our camps consisted of barracks with limed pits and seats arranged against a middle wall or partition running longitudinally. The female and male sides were arranged back-to-back against the partition, and human waste dropped to a common pit below, which offered a clear view to the other side. Well, a bunch of older lads, of which I may have been the smallest party, decided upon some under-the-wall shenanigans. I probably need not go any farther. This beyond-the-pale game lasted about one cycle before a group of us was promptly identified as perpetrators, and I—an innocent bystander according to my perhaps reconstructed autobiographical memory—received the discipline so commonly applied in those days to the bottoms of naughty boys.

AJP: What did you learn from these experiences besides, perhaps, the advisability of steering clear of the authorities?

Panksepp: My childhood play took me to extremes, and all of them, I now understand, were a fun way to test the social realities into which one is born. Surely this is a most important evolutionary function of play—finding out what is fun and fair or not fair on the field of life.

AJP: Did your experiences change much when you came to the United States?

Panksepp: After my family arrived here and I began my formal school days in the cordial environments of small-town America, I had good friends with whom I navigated many possibilities for having fun, usually games that we ourselves devised. We always had the freedom to organize our own baseball, football, and snowball games, and I think those halcyon days of free play—where little else seemed of equal importance—reflected the role that play should *play* in the lives of all children.

AJP: We noticed you used the past tense in reference to play in general. What in your view has changed most about play since your school days?

Panksepp: Play is now increasingly rule bound and organized by adults and seems increasingly lost in our evermore regulated and litigious society where too many kids have little freedom to negotiate the social terrain on their own terms. That is, of course, understandable from adult perspectives. For instance, I incurred a painful back injury during a back-lot football game

in fourth grade, and it has seemed to make me forever susceptible to lower back problems. I never got medical treatment for that injury because I fibbed to my parents about what had happened and minimized how bad it really felt so their parental worries about playground and sports injuries wouldn't convince them to restrict my activities.

AJP: What was your schooling like? Was play part of the curriculum?

Panksepp: After our family spent a month in New York City, where we landed just at the beginning of the academic year in 1949, we moved to rural Bethel, Delaware—a town of about two hundred people, now on the National Register of Historic Places, in the southwest corner of the state—where I entered first grade. My brother and I had the good fortune to start our schooling there in a one-room schoolhouse with a single teacher—Miss Hitchens—whom I well remember. She took care of six grades, with just about enough children to fill one row for each grade level. It was perfect! We had a superb chance to listen to and absorb the English language without all that much need to use it immediately. Miss Hitchens was a kindly disciplinarian who knew how to run an orderly curriculum, and she took special interest in us. She gave us several boxes of elementary storybooks to facilitate our interest in reading, and we loved them, and her. She also gave us free play before class. The schoolhouse was about half a mile from home, and we walked there and back every day without parental supervision. At noon we had a whole hour for free play, just enough to slake our desire for bodily activity. Then in late afternoons, we were left to our own devices on a large farm, just on the outskirts of town, where our parents worked. There was no shortage of fun. After a year there, we moved again. In those days, immigrants needed secure job placements before they could get Green Cards to live in the United States. Once our parents had fulfilled their contractual obligation, we moved to Lakewood, New Jersey, to live in a community with many more Estonians. I spent the rest of my childhood, through high school, in schools in relatively small-town America, almost always close enough to walk to school, as we had in Bethel. My brother and I were consistently good students, and we adequately fulfilled the expectations of our immigrant parents, who were by no means as enthused about childhood play as we were. For example, only rarely did Christmas or birthday presents consist of toys; I had to first borrow and then buy my own first bicycle. However, as I noted earlier, recess was always a part of the free-play time we could enjoy before school and during

lunch hours. Plus, at least one class each day was devoted to gym, where we had various organized sports activities from basketball to wrestling.

***AJP*:** These do indeed seem like halcyon days.

Panksepp: I never dwelled much on our play in those days until I started a research program in play in the late 1970s. However, I am now convinced that the failure of so many children, especially in only-child families living in big cities, to have sufficient free play, may be gradually changing the fiber of our society. The world is a more dangerous place now than when I grew up, and so perhaps we have to begin to consider building more play sanctuaries for our kids.

***AJP*:** That is a good segue into your research. You first thought you might become an engineer. What attracted you to that field, and how did you become interested in psychology?

Panksepp: I initially dreamed of majoring in architecture, with aspirations of going to Carnegie Institute of Technology—CIT—even though the single-minded focus one needed to pursue that curriculum, in lieu of a broader liberal arts education, did seem a bit worrisome to me. I received a fine scholarship offer from CIT, but not as good as from other schools to which I had applied as back-ups—usually in electrical engineering. I really didn't know what I wanted, but I realized that because of family finances, I would have to pretty much put myself through school. So after I got better offers from other universities, including close to a free ride that I accepted at the University of Pittsburgh, I rapidly found out how little electrical engineering really captivated my interest. From my sophomore year onward, I slogged from one academic major to another—first chemistry and then creative writing. I loved writing, but John Irving, also at Pitt at the time, was so outstanding that I realized I couldn't excel in it. And so I moved on, finally, to psychology.

This last transition was largely precipitated by an emerging interest in philosophy of mind and many late-night bull sessions with like-minded friends. I developed a growing aspiration to go to graduate school in clinical psychology, but the critical event that led me to shift was a summer job during the last years of college. I was a night orderly in the psychiatric unit of a Pittsburgh hospital. The unit was organized as one long linear space with three wards. The front or easy ward consisted of living space for reasonably well-regulated, often short-term, patients. That was followed by a locked ward for the more difficult-to-manage, chronic cases. Then came

a padded ward for the very hard-to-manage, floridly psychotic, and often violent individuals. I worked in all the wards, and the evening shift allowed me much free time not only to get to know many patients, but also to read about their life histories and to see how they responded to the many psychiatric medicines that were coming into use in the early sixties.

AJP: Once you hit upon psychology, where did you train, and where did that early study lead you?

Panksepp: Toward the end of my undergraduate days, I increasingly wanted to understand how the human mind, especially emotions, could become so imbalanced as to wreak seemingly endless havoc upon one's ability to live a happy life in the outside world. I was accepted to pursue clinical training in the Department of Psychology of the University of Massachusetts (UMass) in Amherst. My first stipend was a Veteran's Administration (VA) clinical traineeship, in which I participated during my first year of graduate work. This required me to motorcycle to Northampton Veterans Hospital, just west of Smith College, several days a week. The training program had considerable flexibility, and that became decisive in my shift to neuroscience.

AJP: And this is where you began to learn about human emotions?

Panksepp: Yes—at the VA hospital, not the university. In my classes, I rapidly learned how little clinical psychologists knew about human emotions. At that time, many clinical psychologists were entranced by the most recent hot new idea: behavioral modification—a therapy that focused on utilizing reward contingencies to modify undesirable behavior patterns with little concern for emotional feelings. I found the abysmal lack of discussion about human emotions very disappointing.

At the VA hospital, I ended up joining the Electroencephalography Clinic in the Neurology Unit under the leadership of Arnold Trehub. He was providing this service to the VA, but his heart was in the research proceeding at that same time in his well-funded and well-equipped laboratory devoted to analyzing visual dynamics in the brains of rats. After many discussions of training possibilities, Arnie asked me if I had some scientific questions I wanted to pursue. I said I was fascinated by the recent discovery of self-stimulation reward in the brain, and he promptly encouraged me to give it a whirl. The work was engaging from the outset and rapidly successful. I was entranced, and I quickly picked up the skills I needed to work on ancient neural systems that seemed to mediate reward in the brain. Then I decided that a shift toward training in physiological psychology—now called

behavioral neuroscience—would be an ideal way to learn about the nature of basic human emotions and motivation—namely by studying comparable processes in animal brains because detailed work on these ancient brain systems was impossible in human beings.

AJP: What happened next?

Panksepp: Fortuitously, Jay Trowill, a psychobiologist trained in Neal Miller's lab at Yale, had just been hired by UMass. He also had just received a substantial National Institutes of Health (NIH) grant to pursue an even more recent hot new topic: autonomic nervous system conditioning in curare-paralyzed, artificially respirated rats. In such animal models, where somatic participation in conditioning was eliminated through the use of curare, one could only reward the animals by direct brain stimulation. Jay was happy to find a fresh student who already had the requisite skills, and I was fortunate to find a mentor who was delighted to have an engaged student who had his own ideas and could do work without much supervision. But after being convinced that autonomic conditioning was a weak phenomenon—indeed, the area of inquiry died soon thereafter, apparently with intimations of scientific hanky-panky by a postdoctoral fellow in Neal's laboratory—Jay set me free to pursue other scientific questions that interested both of us. I enthusiastically focused on elucidating brain-stimulation-induced reward and punishment, setting in motion a novel view that this brain network constituted a unified incentive motivational system of the brain, which we wrote about, with Ronald Gandelman, in *Psychological Review* in 1969. This germ of an idea was refined into the SEEKING-EXPECTANCY system concept that I advocate to this day, against the still-prevailing concept of "the" brain-reward system. Activation of this system is rewarding, but so are stimulations to other brain systems.

Eventually, for my PhD dissertation, I mapped the rodent brain for aggressive behaviors and learned much about the distributions of many other affective processes in the subcortical regions of the brain. There seemed to be an ancient emotional brain in all mammals that I suspected must be the foundation for the affective aspects of human nature, and evidence continues to support this notion.

These were rather novel areas of investigation, but I had also developed interests in more traditional areas such as energy- and water-balance regulation (much safer areas in those days), and I went to do postdoctoral work on the biochemistry of energy-balance regulation with David Booth,

a nutritional biochemist, at the University of Sussex in England. David had become interested in the conditioning of food preferences, but he was quite happy to have me develop my own research interests, again to my delight. Regrettably, such freedom to pursue one's own ideas has become ever rarer in the ever more rigid, grant-driven research programs of modern neuroscience. On returning to the States, I was fortunate to have a transition year developing expertise in the study of sleep physiology in Peter Morgane's well-funded lab at the Worcester Foundation for Experimental Biology in Massachusetts. I gave a paper on my work on brain glucose metabolism and feeding at the first Society for Neuroscience Meeting in Washington, D.C., in 1971. That was when the meeting consisted of just a few hundred presentations compared to more than fifteen thousand these days.

AJP: As a young scholar, you enjoyed extraordinary freedom to jump from interest to interest and field to field, didn't you?

Panksepp: Yes. Throughout my graduate and postdoctoral work, I was given the blessing of playing around with intriguing—and perhaps profound—neuroscientific questions of how the mammalian mind is organized. As a young faculty member interested in many topics at Bowling Green State University, I decided to focus my initial efforts in a very fundable research area—brain energy-balance regulation. But when I got tenure in the mid-1970s, I promptly shifted my work to very untraditional areas of inquiry, which led directly to the novel approaches and visions of *affective neuroscience*. I used that term first in the late 1980s for basic neuroscience studies of emotional systems, but now it has much wider implications.

AJP: So you began as a clinically oriented physiological psychologist and worked first on self-stimulation, then aggression, then energy balance, then sleep physiology, and finally on the brain organization of social emotions. Not long after, you were studying the nature of play in rats. What led you there?

Panksepp: Fate led me to research on play—a topic that many still deem as relatively frivolous and unimportant and perhaps unworkable. Hence, it is largely an unfundable research area. My path there was a natural one, but explaining it requires me first to describe the work on social attachment that preceded and gradually led to the work on play. When the opiate receptors in the brain were discovered in the early 1970s, my mind was prepared to envision that a major substrate for social bonding had finally been revealed in the brain. Remember that Harry Harlow's classic University of Wisconsin research program—inspired partly by the ideas of British

psychoanalyst John Bowlby—studying the enormous emotional deficits of motherless rhesus monkeys had yet to connect up with neuroscientific causes for the depressive-emotional and sometimes autisticlike deficits arising from inadequate social bonds. Indeed, there existed no substantive understanding of the neuroscientific underpinnings of social bonds or the other prosocial behaviors, such as play, that presumably facilitate the development of emotionally healthy minds. John Paul Scott, who helped recruit me to Bowling Green, had been pursuing these ideas with the conviction that understanding the separation-distress responses might be a key to understanding the nature of secure social bonds. Indeed, John Paul, being on the verge of retirement, had his eye on me as a potential individual to continue his seminal work on social processes in dogs—the way Stephen Suomi, a social psychologist, has marvelously transformed Harlow's original research inspirations into neuroscientific and genetic understanding at his spectacular lab at the National Institutes of Health.

Soon after the discovery of brain opiate receptors, I shared my idea about the potentially addictive nature of social bonds with John Paul, and in his inimitable, even-tempered way, he gave his blessing for me not only to pursue the idea in his lab but to become graduate supervisor for his remaining graduate student, Kenneth Davis. Both opportunities proved fortuitous and very productive. Along with my own graduate students—Paul Bishop, Barbara Herman, Rick Meeker, and Thomas Vilberg—we started to evaluate the role of opioids in the regulation of separation-distress vocalizations in dogs, guinea pigs, and young domestic chicks. Ken accepted the challenge of evaluating the opioid modulation of socially facilitated tail wagging and face licking in dogs. In all models, incredibly low doses of opioids modulated all of these basic social processes, and we proceeded to evaluate the participation of many other neurochemical systems in these and numerous other indices of social bonding and social emotions.

AJP: So now you were involved with brain chemistry too. Given existing assumptions in behavioral research, did your findings rock the boat?

Panksepp: Our ideas were very radical and not easily accepted by colleagues, most of whom rejected emotional concepts. Many also claimed we were simply overdosing animals with opiates; critics apparently did not recognize that we were using the very lowest doses of these agents (even down to 0.2 mg/kg) ever to yield robust emotional changes in animal models. Our initial paper on opioid modulation of separation distress in three species was

accepted by two reviewers for *Science* magazine but rejected by the editor, whom I called up promptly. He indicated that our theoretical suggestion at the end of the paper—namely, that the modulation of the psychic pain arising from social loss and social disenfranchisement may be one reason for the rising prevalence of opiate addiction in our society—was "too hot to handle." It set us back enormously because publication of new socially relevant findings in *Science* in those days would have attracted recognition and perhaps funding.

AJP: What happened next?

Panksepp: We kept working. In her dissertation, Barbara Herman mapped out circuits in guinea pigs, along with definitive evidence that internal brain opioids released by localized stimulation of separation-distress regulating circuits specifically modulated separation-distress circuitry within the brain because naloxone—an opiate-receptor antagonist—blocked the ESB-induced crying threshold changes. We published these findings in 1981. In their dissertations, Paul Bishop and Tom Vilberg mapped out the separation-distress circuits and their modulation by opioid peptides in domestic chicks, though regrettably neither dissertation was published. The evolutionary story seemed to be that this ancient emotional system had very similar brain and neurochemical controls across very diverse species.

Here was an ancient social-emotional system for a form of psychic pain that arises from social isolation—grief, loneliness, perhaps even panic attacks. In a 1982 article, I called it the PANIC system, and I became receptive to the idea that there were other primary-process social-emotional circuits in the brain, such as ones for sexual LUST and maternal CARE. In my writing, I used capitalizations as markers of *primary-process* emotional networks and also to alert my readers to potential part-whole confusions that need to be minimized in order to understand the brain. Having finally obtained substantive scientific evidence for a psychic-pain-inducing separation-distress PANIC system that regulated the formation of social bonds, I wondered if there were some other basic social-emotional systems yet to be studied neuroscientifically that were important for the mediation of social joy. I promptly decided to try my hand at seeing whether social play could be systematically studied in laboratory rats. For the first few years, I did all the work myself, as I was usually in the lab as much as my grad students. Gradually, students became more interested in what I was claiming as a breakthrough in understanding social processes. And we now have abundant evidence for a primary-process PLAY system in the brain.

AJP: So, in contrast to behaviorists who began using animal models but started with simple designs, established simple goals, and measured responses and learning after stimuli, you looked for more complex motivations and behaviors. What made you think that rats could have such emotional range?

Panksepp: There is no substitute for understanding what the brain does than actually looking at animals and studying their natural behaviors systematically in the laboratory, the way ethologists did with animals in the wild. I was not terribly interested in acquired memories, but rather I wanted to know about the kinds of memories that evolution had built into organisms as the basic, unconditional tools for living. Emotional networks with their various affective states are evolutionary memories that allow animals to automatically anticipate certain survival concerns. But experienced emotionality in animals remains a controversial issue to this day, largely because the behaviorists asserted that one should never talk about the internal mental processes of animals.

My forerunners, the strict behaviorists, were mainly interested in the *prediction* and *control* of learned behaviors, and their techniques led to highly reliable findings that could be harvested with microswitch closures—lever presses, for example—in highly constrained prisonlike environments, such as Skinner boxes, with no need to actually look at the natural behavior of animals. Although learning is of clear importance for understanding animal and human behavior, natural behavior patterns that animals exhibit in the real world are equally important. I was more enchanted by what the ethologists were doing: trying to understand the natural behavior patterns animals normally exhibit, especially in their real-life encounters with each other. It was clear that even in Skinner boxes animals would continue to show many of these instinctual behaviors; they were what Keller Breland and Marian Breland had called "misbehaviors of organisms" in a similarly named *American Psychologist* article back in 1961. In general, the most commonly observed types of behaviors were those that had, at one time, been called *instinctual,* and many clearly seemed to be of an emotional nature.

AJP: Do we share not only reactions and responses with animals? How about feelings as well?

Panksepp: It seems that all higher animals—by which I mean nothing more than critters with complex brains, like mammals and birds, all of which exhibit quite similar emotional and motivational urges—share very similar primary-process infrastructures in their brains. Affective-emotional behavioral tendencies seem to have been built into their behavioral repertoire as

ancestral memories that generate various instinctual, emotional, and motivational urges that are accompanied by feelings—which the behaviorists merely called *rewards* and *punishment* because those could be defined by external objects and events. The behaviorists chose to ignore the possibility that in the brain emotional circuits induced feeling states that guided behavior.

The brain locations and functions of these neural networks tell us much about their evolutionary relatedeness, and hence their age, in brain evolution. Clearly these subcortical networks are self-similar—they have homologous infrastructures—across the brains of many species. Thus, we were pleased, but not too surprised, that the localized brain-stimulation mapping of separation-distress vocalizations in guinea pigs and young chickens were concentrated in the same brain regions. Wow! That had to mean that separation calls were very ancient, perhaps the earliest forms of socioemotional communication that could tell us much about the evolution of mind.

AJP: So you set out to test and prove this experimentally?

Panksepp: Yes, indeed we did. It became central dogma for us that if we could activate distinct and coherent emotional behavior patterns in animals using localized ESB—electrical stimulation of the brain—especially in the same brain regions across different species, we had evidence for the existence and location of emotional operating systems that were constructed by evolution rather than by individual learning. And whenever one found brain sites like that, one could empirically demonstrate that they could serve as rewards and punishments in learning tasks. This means that they generated experienced states of the BrainMind. I cannot emphasize this simple fact too much, especially since so few scholars, even cognitive neuroscientists, appreciate the point: If you activate a brain system with electrical garbage and you consistently generate coherent emotional behavior patterns accompanied by affects, then there is no other logical option but to conclude that behavioral and affective tendencies, in raw form, were constructed into the infrastructure of the brain by evolutionary selection as opposed to individual learning. This criterion has been central to our claim that there is an ancient emotional and motivational evolutionary infrastructure to all mammalian creatures—self-similar from mice to men, so to speak.

AJP: Problems like the connection between mind and body had stymied philosophers since the time of Descartes. Yet you sidestepped the old problems and used the term BrainMind. Tell us about that.

Panksepp: Yes, thanks; that follows nicely from what I have just said. As soon as one has identified instinctual, emotional networks that control similar emotional behaviors, one can ask a single momentous question: Can activation of these same systems also serve as rewards or punishments in the control of learned behaviors? If they can, then the most sensible option to consider is that the stimulation is not only just activating behavioral patterns, but also contains the neural complexity to elaborate various kinds of feelings as part and parcel of the same neuronal networks. This profoundly changes both the philosophical assumptions and the psychological approach to the way mind and brain interact. Remember, the behaviorists had no clear scientific way to talk about the mental—the experienced—aspects of organismic behavior sequences. Thus, they were forced to expunge all experiential concepts from their lexicon—perhaps a wise move for their times but not for ours. If you have no understanding of the brain, and, indeed, argue that an understanding of brain functions is not needed for a complete science of behavior, as the behaviorists did, then one has no possible way of making any useful scientific linkages between any mental concepts and the functions of the brain. Indeed, it can lead to the delusion that mental processes do not really exist, which aborts any sophisticated or even coherent conversation about the neuroevolutionary nature of the mind.

AJP: Are you saying, in other words, the behaviorists begged the basic questions of how our physical equipment generates our mental and emotional selves?

Panksepp: Indeed. The key question for all consciousness studies these days is how do brain activities create experiences? I expect that the first answers to such questions will come from the study of primary-process affects, perhaps the emotional ones, such as PANIC and PLAY, because we already know much about the underlying instinctual networks that concurrently activate instinctual behavior sequences as well as rewards and punishments.

This will be a momentous scientific discussion once neuroscientists get engaged. Right now, most who indulge in such considerations seem to be convinced that the solution to primary-process aspects of consciousness will be solved by understanding the experiences that arise from cognitive-type information coming in through our sensory portals, for instance the experience of color, such as redness when eyes are exposed to certain frequencies of electromagnetic radiation. But no one has come close to solving that puzzle, except for the identification of various visual pigments in

the retina and the corresponding color processing areas in the neocortex. That tells us what types of receptors are needed to trigger a brain process we don't quite understand. I suspect a more complete in-brain answer may come from understanding the most ancient subcortical manifestations of affects, especially emotional affects, where instinctual behavioral displays can be used as proxies to track the underlying neural processes. Indeed, it seems reasonable to believe that an understanding of the most evolutionarily ancient form of mentation could lead our inquiries. A coherent case can be made for the supposition that an understanding of all higher neuromental processes will be critically linked to the most ancient forms of experiences. In my estimation, those will be affective—various feelings that are *good* and *bad* in distinct ways—and hence can serve as rewards and punishments in the regulation of behavior.

AJP: You are studying age-old questions—why and how we feel the way we do. Why aren't more neuroscientists engaged?

Panksepp: Why so few investigators are following this track is puzzling to me. In this whole poorly developed arena of knowledge, a robust finding, across species, is that the brain systems that generate emotional behaviors during ESB also engender some type of associated emotional experiences. It seems to be a law of nature that whenever and wherever in the brain one evokes a coherent emotional response using localized ESB, one can also use that stimulation as a reward or punishment in various learning procedures including evaluative choices such as those monitored by conditioned place preferences and conditioned place aversions. In humans, such brain stimulations routinely generate central states that feel good and bad in various ways. In other words, people commonly desire or despise electrical stimulation to brain areas that provoke robust positive and negative emotional behaviors in animals.

Granted this concordance, we now have a way to study the neural infrastructure of emotional feelings at ever finer levels using the increasingly sophisticated armamentarium of neuroscience techniques in conjunction with diverse behavioral learning tasks. Regrettably, however, few researchers are pursuing such studies. Most are satisfied with and still insist upon behavior-only analyses, and discussions of animal feelings remain taboo in animal neuroscience. This is the tragic result of way too many students having been trained with behaviorist biases, as was I, which has prevented the needed conversation from being engaged.

AJP: Where, in your view, did the behaviorists go wrong?

Panksepp: Historically, behaviorists were wise to focus their attention on generating rigorous methodologies for studying learned behaviors, but they were unwise to discard issues that their approaches did not—and could not—illuminate. All their work was guided by a Law of Effect—actions followed by rewards or punishments respectively increase or decrease in incidence. They never acknowledged that the original verbiage of this great insight was affective and more appropriately might have been called the "Law of Affect"—as Edward Thorndike originally stated it: actions followed by satisfactions are increased, and those followed by distress are diminished.

Without brain research, the rewards and punishments—those many satisfying and distressing feelings of the brain—could not be studied. After the advent of modern neuroscience, about forty years ago, they could have been, but students were even discouraged from bringing up such issues. When I did that in a neuroanatomy class, one of my professors said, "I've seen guys like you before, and they are not around anymore." In any event, it is only from the more ancient regions of the brain that we can evoke coherent emotions with ESB and obtain strong indices of rewards or positive affects and punishments or negative affects. Primary-process emotions are very ancient as indicated by (1) the very ancient caudal and medial locations of the key networks and (2) their continued functionality in animals whose neocortical networks have been destroyed soon after birth. Unfortunately, animal feelings were marginalized by so many influential and powerful scholars who simply could not tolerate such thinking that instead of essential conversation, there was mostly enforced silence. The whole field continues to be influenced by too many elders whose scientific arrogance was deeper than their wisdom.

AJP: Do you feel like a pioneer or even a revolutionary?

Panksepp: I did not initially, but I came to. When I was a graduate student, few of my professors had the language to talk about emotions. There were few academics to engage in the types of conversations I was having with myself and eventually with my students. However, the many psychologists who eventually started to talk about emotions in the 1990s were little interested in the brain, and they did not wish to acknowledge that their approaches were not sufficiently robust for us to make progress on profound questions such as: "How are affects created within the human BrainMind?"

I finally found a like-minded community of scholars among psychoanalysts—especially the new brand of neuropsychoanalysts, psychotherapists, and many psychiatrists. They were hungry for real scientific knowledge about the emotional mysteries they had to contend with on a daily basis. I have remained amazed and perplexed that some of my closest colleagues, those with expertise in the requisite experimental approaches, have resisted joining the conversation. Even those presumably working on emotional process have chosen not to engage intellectually on the nature of experience; many think that those processes are unconscious (unexperienced) in the animals they study. I discuss this in, among other places, my article, "Affective Consciousness: Core Emotional Feelings in Animals and Humans," in volume fourteen of *Consciousness and Cognition*.

Earlier in my career, I never thought of myself as a revolutionary, but I gradually realized that I was pioneering a new approach to understanding the foundations of the MindBrain that all mammals share as evolved tools—or gifts—for living. I did an extensive interview about that—"How to Undress the Affective Mind"—for a 2008 issue of the *Journal of Consciousness Studies*.

AJP: Tell us about some of the resistance you have met.

Panksepp: I have encountered much resistance to my way of thinking and talking about the neural foundations of mental life. When I raised one such issue at an NIH workshop on emotions back in 1998, a close and admired colleague asked, "Jaak, why do you continue to impale yourself on the horns of this dilemma?" I smiled and responded, "I guess I enjoy being impaled on the horns of reality." Perhaps, in part, my early childhood experiences—the many hardships as well as the liberties of my youth—allowed me to be more flexible than others. Another factor was the indirect way I got into the field, first being interested in relevant clinical issues, but then realizing that certain aspects of our minds—for instance, raw emotional feelings—could never be understood without having animal models where the neural details could be worked out. I do not know anyone else who has taken that indirect path to behavioral neuroscience.

I did not fully realize how deeply ingrained the resistance was to such approaches until I had grant application after application turned down because I was frank about the role of emotional experience in the control of animal and human behaviors, not a popular idea in behavioral neuroscience. Before shifting my research energies largely to emotion studies, I was as well funded as the many behaviorists that were flooding into the neurosciences

in the 1970s—their funding and job opportunities had suddenly diminished with the onslaught of the cognitive revolution in psychology at that time, and the alternative big money was in neuroscience. However, once I made it clear that I was interested in the primitive emotional feelings in humans that arise from ancient mammalian brain functions, federal support for my research dried up completely. It also often became much harder to publish our empirical work.

Meanwhile, the work of the behavioral neuroscientists continued to flourish, and they remained powerful, continuing to hold the reins of power in federal agencies supporting research. Had I hidden my questions under traditional behavioristic jargon, I would have been much more successful in attracting funding. Indeed, after recognizing how intolerant funding agencies were of emotional concepts in the 1980s, neuroscientist Joseph F. LeDoux decided to eliminate any reference to emotions in his grant applications and contextualized the work of his rapidly expanding and well-funded lab completely in terms of learning and memory. Other fear-learning investigators used the same tactic.

I regret that so many reviewers gave us so much grief about our use of emotional terms and words like *crying*, *psychological pain*, *playfulness,* and *laughter* in our publications. We would have made more rapid experimental progress had I chosen to be deceitful in my verbiage. However, I chose the path I believed ontologically correct. Now that my active research days are almost over, I have no regrets about having made that choice. Blood, sweat, and tears, along with a few fine graduate students, kept our project afloat. And in retrospect, someone simply had to represent and stand up for this credible alternative—that emotional experience did matter in the control of animal brain functions and behavior.

AJP: How did animals—as a way to view our ancient minds—come into the picture?

Panksepp: I was an evolutionist from the beginning and felt confident that the origins and neural substrates of the human mind could only be well studied in our fellow animals. I guess that reflects a pioneering spirit too. In any case, Lucy Biven, an English psychotherapist, and I are currently completing a book titled *The Archeology of the Mind: Neuroevolutionary Origins of Human Emotion*.

It remains a great challenge to convince most psychologists studying humans that they can understand the ancient psychobiological principles that underlie human nature more effectively by studying related animals

than by studying human beings. Human experimental psychologists, including those keenly interested in emotions, typically invest most of their efforts in documenting the cognitive and higher neural complexities associated with human emotions. Because of their own recent history of cognitivism, they seem to have little interest in what we share affectively with the other mammals. Many are still unable to fathom the depth and breadth of mind in brain evolution and remain devoted to the view that emotional feelings are higher mental processes that could not exist without the many tertiary higher brain functions. But these functions—thoughts and other cognitive and cultural processes—interact with the primary-process emotional networks I have been studying. Indeed, esteemed colleagues like Joe LeDoux and Edmund Rolls see them as necessarily connected. They envision emotional experiences as higher cortical read-outs of more primitive unconscious bodily and brain commotions.

Still, it is gratifying that an enormous number of scholars, especially in the clinical, psychotherapeutic disciplines, get it. There is a hunger out there for scientifically credible conceptions of our emotional nature, and with them, I feel I am among kindred thinkers. Also, my students have consistently understood the importance of such undertakings, and I trust that many will continue to cultivate the furrows of understanding that have already yielded a remarkable harvest of knowledge about our deeper nature.

AJP: How does your work studying play change the study of the emotions?

Panksepp: The most popular view about emotions in psychology has been that they reflect read-outs of peripheral bodily changes that occur in emotional arousals. The largely mistaken James–Lange theory [a theory of emotions developed independently by two nineteenth-century figures—American philosopher and psychologist William James and Danish physician-psychologist Carl Georg Lange] dominated emotion studies in twentieth-century psychology—work that clearly focused much more on affectively negative emotions than on positive ones. I think all this is historically understandable, including the tendency of most in the field to conflate cognitions and emotions as being part and parcel of higher MindBrain functions. The recent transition to brain studies in human psychology has only come about through the development of spectacular human brain imaging, but much of that work is mistakenly reinforcing the view that higher cognitive brain functions generate emotional feelings.

The fact that new tools such as fMRI—functional magnetic resonance imaging—are more sensitive to highly firing neocortical networks that mediate cognitive processes than to the many slowly firing subcortical networks that control primary-process emotions is leading to an excessive, and I would say mistaken, neglect of the lower brain processes we share with the other animals. I think these researchers would be wiser to follow the lead of Darwin, who was clear about the probability that all mammals share very similar emotional and other mental processes.

In any event, the BrainMind has to be envisioned as an evolutionarily layered organ system, with all higher developments still anchored to the lower primary processes of the brain. This is simply the way the brain is organized. The original foundations of mind remain critically important for the ability of higher processes to function.

The play processes of the brain are delightfully representative of such complexities. Play studies have brought positive psychology one of the most important inbuilt emotional complexities that can help clarify many higher-order issues, from our love of sports to the rough-and-tumble nature of power politics. Thus, in addition to other subtle positive emotions, such as SEEKING, which are closely related to LUST and CARE, we are finding that PLAY urges are intimately related to all of these others.

AJP: So, play helps connect these original foundations and the later higher processes?

Panksepp: Indeed. Our urge to play may be the most complex and recent primary-process emotion in BrainMind evolution, but one that is still linked to our other positive emotions in ways not yet well understood. I think play research conducted by investigators that acknowledge emotional feelings in animals, largely unfunded in our zeitgeist, is a beautiful counterpoint to the incredibly well-funded work on fear learning by investigators who do not yet explicitly acknowledge that their animals experience fear. PLAY, perhaps more than any other positive emotion, opens up radical new possibilities for our consideration of flaws in our brain research and our cultural practices. It tells us much about the kinds of creatures we really are, highlighting aspects of our nature that we have studied scientifically. Many would prefer to envision our playfulness as reflecting higher mind function rather than lower, more ancient ones. The sooner we shift our perspectives, the sooner we are likely to build cultural institutions that support our joyful *lower nature*, so important for mental

health, as vigorously as our *higher* cognitive *nurture* that is all too often administered in rather unpalatable ways to our children.

AJP: What made you suspect that rats could play? How did you set out to find out? And were you surprised when you found that they could?

Panksepp: Having decided that I wanted to evaluate the positive side of social processes, after we had intensively studied one of the most negative affective aspects—namely, separation distress—I selected play as the most likely approach to yield compelling data on the positive side of the emotional coin. In other words, I just said to myself that surely play is a natural social function of all young animals. Without reading much of the existing literature, I promptly went to the laboratory to see if I could study it systematically. I discovered later, of course, that Karl Groos had published a wonderful book, *The Play of Animals,* back in 1898. Anyway, in my ignorance, I simply rehoused some of my young rats alone and then put them back together in pairs after various periods of lonesomeness to see what would emerge. They played with such eagerness that I was blown away. I was most surprised by how easy this was to study—how a little play deprivation brought this behavior under experimental control so clearly and dramatically that highly systematic research could be conducted quite easily.

I am fond of saying that play behavior is as responsive to social deprivations as measures of hunger and thirst—that is, food and fluid intake—are to energy and water deprivation. To this day, I remain enchanted by how eagerly young rats pursue the fun side of life. I soon wanted to know everything I could about this ancient MindBrain process, and it wasn't hard to get students interested in studying this neglected joyous side of life.

AJP: What does rat play look like? Can you define it?

Panksepp: It is rather amazing how self-similar play bouts are in pairs of rats. As a result, it was easy to identify a few key indicator variables that could be focused on using on-line measurement procedures—behaviors such as chasing or overall motor activity, pouncing, and major events such as pins during their wrestling. This allowed the research to move along rapidly and efficiently. Each animal was scored, on line, independently, and we soon found that after a few sessions—females took twice as much time as males—one animal of a play pair became dominant, ending up on top an average of about 70 percent of the time. Much more complete descriptions of play were soon generated by Sergio Pellis and Vivien Pellis at the University of Lethbridge in Canada. You should see their excellent new book, *The Play-*

ful Brain. Such research seems adequately funded in their country; there are wise folks up there.

People keep asking me to define play, and I tell them that the ultimate definition of every primary-emotional process has to be based on an understanding of the underlying brain mechanisms. However, for operational definitions, we use the behaviors I noted a moment ago along with indices of the desire to play—for example, willingness to work for access to play. Of course, we also provide a loose definition by sharing the flavor of what play looks like. We can't improve much over one of our early descriptions: When two rat pups "are placed together in a non-threatening environment, they rapidly begin to exhibit vigorous play fighting: animals chase and pounce on each other, sometimes unilaterally, sometimes mutually with rapid role reversals. They repeatedly poke and nip each other, often at the nape of the neck but also on the ventral surface when one animal is pinned." That is from a piece that Steve Siviy, Larry Normansell, and I did for *Neuroscience and Biobehavioral Reviews* in 1984.

AJP: Is rat play in any way a model of play for other species?

Panksepp: Each species exhibits species-typical play sequences, but still there is a self-similarity in the overall impression of joyous lightness of being. Although all the complex dynamics of play cannot be captured easily in numerical measures, there are a sufficient number of repeated sequences, easily operationalized, that can be used effectively as proxies for overall estimates of play activities. I personally do not think that a frame-by-frame analysis of play provides all that much more useful information, but if one has the time, money, and students to do the tedious work, it is surely useful to be as complete as possible.

AJP: Would a frame-by-frame analysis help to show the difference between playing and fighting? Since rats seem to play rough, is it possible to draw a clear line between play and aggression?

Panksepp: This seemingly subtle issue is pretty evident to the tutored eye, even though certain adults are so remote from playful realities that they can't tell the difference. When we first started to study play fighting, we thought some would call it aggression, so we asked various observers to give us their opinions without subjecting them to any of our own biases. Among the dozen or so university professors we screened, the large majority categorized the behavior as aggression. Even Robert Plutchick, the famous investigator of basic human emotions, did so when he visited our lab. Among students,

there was much more variability. About half called it play. The graduate students and better undergraduates—such as those who got higher grades in my courses—tended to call it aggression, and the ones who seemed more relaxed about their studies called it play. When I polled a group of young children between the ages of four and seven, though, they all called it play. Having observed many bouts of both serious aggression and play fighting, I have never had any ambiguity.

AJP: Could you test the difference in other ways?

Panksepp: Early on, I tested the aggression aspect and found that giving animals high doses of testosterone diminished their playfulness, largely because the animals got, all too readily, into serious fights. Likewise, placement of large ventromedial hypothalamic lesions, which promote savage aggression in adult rats, diminished play among juvenile rats. They showed chronic irritability when touched.

Of course, separating what is fact from opinion is hard, but eventually we devised a more rigorous index based on the concurrent analysis of ultrasonically measured emotional vocalizations accompanying play activities. As a play session proceeds, happy chirping and laughter vocalizations diminish systematically in both rats and humans, and complaints increase. Indeed, among rats, every time a complaint of a certain level—22 kHz—occurs, playful activities cease for a while. It is important to note that these kinds of vocalizations are very abundant during adult aggression and very few happy sounds are heard. This measure beautifully discriminates between joyous play and angry fighting episodes. Jeffery Burgdorf and I graphed and discussed this in a research article in *Peptides* in 2006 as well as "The Neuobiology of Positive Emotions," a 2006 review in *Neuoscience and Biobehavioral Reviews*.

AJP: How did you discover that rats could laugh? Can we even *hear* a rat laugh?

Panksepp: We had been studying play-related vocalizations for about half a dozen years when it crossed my mind, upon waking up one morning, that perhaps this vocalization was related to human laughter. Maybe the idea reflected the spontaneous but implicit juggling of information and possibilities in my dreams. It certainly was not something that emerged from any form of conscious deliberation. In any event, that same morning, sometime in 1996, I went to the lab as usual, ready to help Jeff, who was my undergraduate assistant at the time, with some ongoing experiment.

When I got there, he was waiting for me, and I simply said, "Jeff, let's go tickle some rats." He looked at me with a brief moment of perplexity and then simply said, "OK." He got my drift without much need for explanation, but I explained anyway.

Jeff was as intrigued by this possibility as I was, and the first animal we tested chirped like crazy when I tickled it. So did the second, and the third, and so on. Of course, we could never have discovered this if we had simply been listening in with our own ears. A rat's 50 kHz ultrasound vocalization or chirping sound is way above and beyond our normal hearing range, and we used bat detector equipment to bring the 50 kHz sound down to about 5 kHz, which we could hear very well. It was one of the most robust and reliable nonobvious behavioral phenomena I had ever observed. We dropped everything and started a long research program that continues to this day on this fascinating phenomenon.

I might add that we didn't publish this finding right away. We wanted to test the idea from various vantages, to see if we could negate our own wild idea. We could not. Experiment after experiment suggested that this was an affectively positive vocalization that had functional similarities to childhood laughter which is most abundant in the midst of childhood rough-and-tumble play.

AJP: Did you run afoul of the old guard here too?

Panksepp: Indeed we did. When we tried to publish our initial round of findings in *Nature,* we were thwarted by a mean-spirited reviewer whose bottom line was, "Even if this phenomenon were true, you would not be able to convince your colleagues." In frustration, Jeff and I submitted our paper, exactly as originally offered to *Nature*, for the 1999 proceedings of a conference I had attended on emotions, even though that compilation was out of the mainstream of scientific literature. In the published version, however, I framed our contribution with some frank reflections on the state of our scientific zeitgeist that had so little room for discoveries that strongly indicate how much we share emotionally with other creatures.

AJP: You said your idea to tickle rats was spontaneous, but you must have had some inkling of what would happen. Can you say what it was?

Panksepp: We were accustomed to doing some handplay with our rats. Perhaps the earliest experiment was in the early or mid-1980s, when Larry Normansell helped me to determine if the satiety curve for rat play—that is, gradually descending amounts of play across a set period—could be

simulated by human handplay with them for the first fifteen minutes of a half-hour session. It worked very well. Rats that had fifteen minutes of human handplay behaved just like animals that had played with another rat, indicating that this was a real satiety curve.

Eventually, Jeff Burgdorf mapped out the laughter-type chirping system in his PhD dissertation that he did with me, and it corresponds quite closely to the trajectory of the mesolimbic SEEKING system. Several times now, we have summarized the overwhelming evidence that this phenomenon does reflect some kind of a positive affective response that has more than a passing evolutionary resemblance to primordial human laughter. Further, the phenomenon has now been well replicated by Tanel Mällo and others in Estonia and by Rainer Schwarting and others in Germany with enough differences of opinion to add intellectual spice to the ongoing discussions.

AJP: Can you tell us how laughing and playing rats helped you to think about why play may have evolved?

Panksepp: This is a premier question, but one that cannot be easily answered through scientific analysis. Science is not well positioned to answer *why* questions, especially ones with evolutionary dimensions lost in the mist of time. There can be only plausibility arguments that may guide science if different arguments make different predictions. We have not yet reached that level of knowledge. However, we can certainly entertain options.

I suspect there will be no better general answer to the question of why play evolved than the supposition that without play it would have been difficult to build in all the needed social dynamics that complex animals such as mammals need to thrive within the complex worlds into which they are born. Often social dimensions of survival vary depending on local environmental conditions, and hence the nuances of the most adaptive social dynamics in specific environments need to be learned. Thus, when I ponder such difficult issues, I suspect play is one of the major ways that the complex social brain emerges from the experiences of living within various ecological and cultural constraints. In short, much of the social brain is created by experiences, and the urge to play is a primary process that helps achieve the programming of higher brain regions, such as the neocortex, which resembles a tabula rasa of massive random-access memory banks within the higher regions of our brains.

AJP: Are you saying it is possible to know where in the brain the urge to play arises?

Panksepp: If, as I stated, the urge to play is a primary process that helps achieve the programming of higher brain regions, then we should anticipate that it arises from ancient regions of the brain, which it clearly does, because removing a rat's neocortex early in life impairs play hardly at all. In contrast, small lesions in thalamic somatosensory projection areas strongly reduce play. However, even though the neocortex is not needed for play, playfulness has abundant effects on gene expression patterns in the neocortex, as we have recently found with as-yet-unpublished microarray—that is, gene-chip—analysis of DNA transcription studies of animals that have been allowed to play compared to those that have not. Jeff Burgdorf led that work at the Falk Center for Molecular Therapeutics at Northwestern University.

AJP: Can looking at the brain tell us what advantages playfulness confers?

Panksepp: At a whole-animal functional level, such effects are likely manifested in more useful social strategies and flexible behavioral responses to unexpected future events. However, experimental work at this level remains in its infancy. Our best hypothesis right now is that the primary-process emotional urge to play, when allowed abundant expression, helps construct and refine many of the higher regions of the social brain. Perhaps it is especially influential in refining our frontal cortical, executive networks that allow us to more effectively appreciate social nuances and develop better social strategies. In other words, play allows us to stop, look, listen, and feel the more subtle social pulse around us.

AJP: Do you see similarities between the play of rats and other animals and the play of children?

Panksepp: Anyone who has lived around young animals would never fall into the intellectual trap of assuming that play is an apparent emotional capacity of just humans. It has been pretty obvious to naturalists that all kinds of critters play; some sensible radicals such as Gordon Burghardt have even envisioned the ancestral footprints of playfulness in various cold-blooded vertebrates.

Because of the importance of play for scientifically understanding the play of our children, Eric Scott and I proceeded to conduct the first formal ethological study of natural play in the youths of our own species. We published "Rough-and-Tumble Play in Human Children" in *Aggressive Behavior* in 2003 (after it had been rejected by three premier child-development journals as "being of no theoretical interest"). The similarities to the play of

other predatory and omnivorous mammalian species are striking. The evidence now clearly indicates that the play mechanisms are very ancient in the mammalian brain, and even though each species has diversified in the specific manifestations of play—herbivores run about and prance more; predators chase and wrestle—we are convinced that the primary-process driving forces for play will be found to be quite similar in all mammalian species.

AJP: We have already talked about the behaviorists' resistance to your earlier study of emotions. However, given the research and findings you have just described, do you think that influential behaviorists like the late B. F. Skinner or John Watson would ever have acknowledged that rats could have feelings?

Panksepp: It's hard to speak for anyone else, but in his own mind, Skinner might have just been one of the methodological behaviorists who simply could not acknowledge the power of emotions in guiding behavior and still maintain any scientific coherence to his behaviorist vision. Thus, we must simply take him at his word when he made such bold claims as, "The 'emotions' are excellent examples of the fictional causes to which we commonly attribute behavior," which he wrote in *Science and Human Behavior* in 1953 without any relevant data to back him up. I tried in 1989, the year before Skinner died, to initiate a conversation with him about his naughty single-mindedness about complex BrainMind issues, but he really did not wish to engage with topics—brain functions, for example—that lay outside his sphere of expertise. I expect that if we had a good psychohistory of Skinner, we would find that his public scientific face and his personal life would not have matched up too well.

So, no, Skinner would never have accepted that rats have feelings. I gave him a chance, but he blew me off with his standard response, which I included in an article called "Can 'Mind' and Behavior Be Understood without Understanding the Brain?" in *New Ideas in Psychology* in 1990. Skinner said, "A behavioral account has two unavoidable gaps—between stimulus and response, and between reinforcement and a resulting change in behavior. Those gaps can be filled only with the instruments and techniques of neurology. A science of behavior need not wait until neurology has done so. A complete account is no doubt highly desirable but the neurology is not what the behavior really is; the two sciences deal with separate subject matters. A third discipline may very well wish to deal with how the two can be brought together, but that is not my field." Well, it is *my* field,

and that is what my 1998 book, *Affective Neuroscience: The Foundations of Human and Animal Emotions*, is all about.

John Watson, the original father of behaviorism, is a slightly more interesting case in this respect. Although he wrote the behaviorist manifesto and changed the scientific landscape of academic psychology in ways that remain to this day, his human sensibilities and distasteful fear-conditioning studies with infants convinced him that all humans had at least three basic emotions: fear, anger, and love. Whether he would have been courageous enough to consider that such functions exist in rat brains seems unlikely. Whether these fathers of radical behaviorism would face up to the evidence we now have about emotional systems that exist in all mammalian brains is debatable, but I see nothing in their writings to provide much optimism. They were very successful revolutionaries at a time in history where a rigorous science of behavior did not exist. They created one, and in so doing, they destroyed bridges to future research options that my radical work symbolizes. Because they burned so many bridges to reasonable possibilities, they simply made my life's work more difficult, and less funded, than it should have been. A pity, but their type of intransigence also made me a revolutionary who has no hesitation in speaking his mind among those who abhor listening to such arguments even when they are based on pretty hefty evidence.

AJP: Turning back to your findings, what does rat play teach us about human play? You investigated the way rats responded when play was denied to them for a time. Can play deprivation in rats inform us about play-deprived humans? For example, do you find that children who are deprived of play respond in comparable ways?

Panksepp: No one has yet explicitly conducted a play-deprivation study in our species, even though I do suspect we are currently in an unplanned cultural experiment of that kind. Too many youngsters of our species never get sufficient amounts of natural, self-generated play. If so, that may be one cause of our current epidemic of hyperkinetic kids with inadequate control over their own impulses.

Overall, I think that our work on the primary-process aspects of play will one day illuminate the brain mechanisms of human play. However, as we proceed to *secondary processes* (learning), there are bound to be more species differences, and even more as we proceed to *tertiary-process* levels (fantasy play and humor) where animal models may never suffice.

Thus, to simplify, the answer to your question is really an empirical matter. Inasmuch as there is currently no systematic understanding of play mechanisms in the human brain, we must use the vast amount of data collected on rat play to see whether it can effectively guide our thinking about the dynamics of human play. I personally suspect this is an invaluable source of relevant information.

AJP: Do the lessons of play deprivation move us closer to understanding the functions of play? Do your conclusions hold for people too?

Panksepp: Well, I hope so. Let's just consider one example: We think that play-deprived children are more likely to show symptoms of ADHD—Attention Deficit Hyperactivity Disorder. Such kids are quieted by psychostimulants such as methylphenidate—Ritalin. The playfulness of rats is also dramatically reduced by giving them such drugs.

Likewise, ADHD children are typically thought to be especially deficient in frontal lobe executive functions—in simple terms, in their ability to stop, look, listen, and feel with consideration for others. When we damage the frontal lobes of rats, they exhibit extreme ADHD-type symptoms, which we can diminish by giving them abundant daily opportunities for play.

We have sought support for such a study in human children for a decade, without success. We find this potentially a tragic shortsightedness in our social and scientific fabric. Abundant happy social play, we believe, will allow every child "to thrive by five." We expect that lots of early play will tend to produce happier and more and productive citizens.

AJP: What are we mammals feeling when we are playing? And how do these feelings instruct us?

Panksepp: Play produces a powerful positive affect that, as I discussed earlier, can be monitored in rats by happy, 50 kHz, chirpy vocalizations. Just like all positive feelings, the joy of play instructs us that we are doing something good for our minds and bodies. Likewise, negative feelings tell us we are in the midst of something that is not beneficial for us. For instance, chronically angry kids are usually telling us there is not enough of the "good stuff" in their lives.

The play urge should also instruct scientists that a study of playfulness is an open gateway to understanding some of the most powerful positive emotional feelings in our lives. Once we understand the chemistries that control these feelings in rat brains, I think we will better understand the

positive affective chemistries of our own brains. This knowledge could help reveal brand new possibilities for generating mind medicines that could be very useful in psychiatry, such as positive-affect-promoting antidepressants.

AJP: You have written that "only when confidence has been restored does careful playfulness return." Are play emotions opposites of rage and fear and despair? Is play an antidote to these negative emotions?

Panksepp: We hope so. That quote you used came from a description of a study where we discovered that cat smell can dramatically reduce playfulness in rats—see the first figure in my *Affective Neuroscience* book, page 18 to be precise. This is an effect that Steve Siviy, one of my finest grad students, superbly analyzed in an article in *Neuroscience and Biobehavioral Reviews* in 2008.

Yes, lots of early play tends to satisfy youthful emotional needs, so such creatures who play should be happier and less angry individuals. We already know the latter. Play-enriched animals show less aggression in adulthood than play-impoverished ones. We are currently evaluating if abundant social play early in development makes young animals resistant to stress-induced depressive responses later in life. The results so far are promising. So much more needs to be done in this field of inquiry, and so few scientific investigators are studying such issues. A pity!

AJP: Does your work bear on new thinking about so-called positive psychology?

Panksepp: Enormously! Jeff Burgdorf and I wrote about this in 2006 in an article titled "The Neurobiology of Positive Emotions" for *Neuroscience and Biobehavioral Reviews*. There is so much more work to be done at the primary-process level, but most of the current work is proceeding at the highest levels of human psychology, where linkages to the brain are rather weak. We think the positive psychology movement needs to be grounded on a neuroscientific understanding of the primary-process positive emotions: SEEKING, LUST, CARE, and PLAY, as well as the many pleasures of sensation and homeostatic satisfactions of the body.

Just consider infancy. Our babies no longer get as much time at the breast as those of our ancestors. Close bodily contact was the norm throughout the ages, as mothers working might carry babies along with them in pouch slings, kangaroo style, so to speak. Babies used to sleep with their mothers, but now they rarely do. Experts advise parents that it is OK for

babies, alone in their own nursery rooms, to cry themselves to sleep. All this is very shortsighted for the future mental health of our citizens. I sometimes wonder if liberals and conservatives, as very different-minded groups, were differently reared. In any event, I helped a close colleague, Margo Sunderland, conceptualize many of these issues in her fine recent book, *The Science of Parenting*.

I think the future of the positive-psychology movement will be linked as much to primary-process affective issues as to the tertiary-process, thought-related ones that are currently most prominent in the field.

AJP: Undergraduate psychology textbooks have begun to feature play behavior. Does this indicate that interest in the study of play is growing?

Panksepp: One can only hope! Certainly it is long overdue.

AJP: As you have noted throughout our conversation, you have taught, guided, and conducted research with many students in your career. Would you like to say anything more about what some of them are working on now and where you imagine their research might take us?

Panksepp: Yes, none of the work I have described would have been as rich as it has become without the enthusiasm of many students too numerous to mention here. However, I would single out three stars, working backward from the present, which also happens to put them in alphabetical order.

One is Jeff Burgdorf, whose contributions I have noted several times already. Jeff joined me at the transition where our basic play research branched off into the study of playful vocalizations, just about the time I took my first retirement—from Bowling Green, in 1998. Jeff was my last doctoral student there, and after he finished his PhD in 2001, he moved on to a fine post-doctoral position at Northwestern University's Falk Center, where I spent a good chunk of my own time before coming to Washington State University. Jeff has provided a solid affective neuroscience bridge for continued work at the molecular-biological level. For the past sixteen years, he has been churning out one important study after another. His most recent work is devoted to gene-expression patterns within the juvenile rat brain as a result of abundant play. So far, about half of the twelve hundred genes we have monitored have changed transcription levels in the neocortex within one to six hours following a half-hour play bout. What a cornucopia of riches! And where to go next? After that, we simply pursued the gene exhibiting the biggest and most widespread changes, and as a result, Jeff has already identified new neurochemical avenues—such as Insulin-like growth factor 1—IGF-

1—for the regulation of positive affect. His behavioral studies indicate this growth factor promotes some kind of good feelings in the BrainMind. There are many more factors to be found and studied, and Jeff has the right stuff and the right techniques to chase them down.

The second student is Brian Knutson, who was a postdoctoral fellow in my lab in the early 1990s. He arrived as a social-personality psychologist who had never worked on the brain, and he left as an affective neuroscientist. He is the fellow who discovered that rats emit abundant 50 kHz chirps while playing, and he exploited that fine phenomenon toward a better understanding of SEEKING urges in the brain—that is, it's a behavioral sign of eagerness. After Brian left our lab for bigger and better things at NIH, he took the lessons of the SEEKING system along to develop paradigms with human brain imaging and indicated how this system lights up when folks are anticipating winning money. I joke that he can justifiably now be called the father of *neuroeconomics*. Brian well deserves the tenured position he now holds at Stanford University.

Last but not least, Steve Siviy, who did almost a decade of postdoctoral work before taking a position at Gettysburg College (currently the chair of the Department of Psychology there), was the first student who was profoundly captivated by play research proceeding in my lab in the mid-1980s. He tracked down touch as the main regulator of play in rats. Through skilled maneuvers, he also succeeded in identifying areas in the brain, such as the parafascicular nucleus of the thalamus, that really do regulate the play urge. It is easy to reduce play with many kinds of brain damage, but few of them are specific effect—namely lesions that do not damage other behaviors of similar complexity. Steve has continued a solid, consistently productive program of play research at Gettysburg.

Many others have participated in this journey too, and I thank them all for their dedication and enthusiasm.

AJP: What's next for your own research? Where will you take us next?

Panksepp: Well, I recently suffered through a four-month medical leave to help vanquish—hopefully—a recalcitrant treatment-resistant lymphoma. My wife was concurrently treated for a different type of lymphoma. We just found out that we are both in remission! The effectiveness of the stem-cell transplant procedure, which allowed the use of lethal levels of chemotherapy, followed by radiation, was apparently more effective than several earlier failed treatments. Thus, if the body prevails, so, hopefully, will the

soul. Right now our research program here at Washington State University is focused on the utility of playful activities in reducing depressive symptoms in animal models. Meanwhile, at Northwestern, where I sustain a collaboration with Jeff Burgdorf and Joseph R. Moskal, we are hoping to crack key biochemical issues about the underlying neural systems and develop new neuropharmacological ideas for treating depression and other psychiatric disorders characterized by affective imbalances.

If we could find the resources, and cultural support, it would be wonderful to evaluate the concept of play sanctuaries to counteract some of the psychological problems that are increasingly emerging during early childhood, such as ADHD. We ran a feasibility study with my last clinical student, Eric Scott, in the Bowling Green public school system, and it looked promising. Most folks, especially those who hold the purse strings for research, seem to be having a difficult time understanding the potential of such sanctuaries and how they would need to be run. I expect that when this utopian idea is finally well implemented, some visionary will even seek to extend it to the cross-cultural level—to have summer camps where young children from diverse cultures can be brought together to enjoy the blessings of free play with each other, under the watchful eyes of caring adults, of course. Perhaps our penchant for cross-cultural conflicts might diminish further if we nourished such playful friendships.

AJP: What breakthrough would you most hope to see in affective neuroscience?

Panksepp: It would be great to have more and more young neuroscientists investing their intellectual and research energies in this nascent cross-species field of inquiry, which is still struggling against the biases of their elders, too many of whom simply cannot envision the importance of understanding animal emotions for understanding our own. Too many still believe that the only way to understand emotional feelings is to talk to people.

The biggest questions in the field will remain for a long time to come. The MindBrain can only be approached through successive approximations. The most recalcitrant question will be to provide cogent, empirically testable visions of how the various affective states—the psychological aspects of the many rewards and punishments, the many positive and negative affects—are actually constructed from neural-network activities in the brain. To do this, we must develop techniques for monitoring distinct neural network activities in the brain as effectively as we can currently study activities of

single neurons and determine how various neurochemistries control specific emotional neurodynamics. We must also develop effective empirical ways to see how well animals discriminate the various positive and negative affective processes of their brain.

We have lots of ideas and so few skilled hands to carry out the hard research to answer them. At the moment, I am fascinated to see whether we can model anticipatory learning processes in brain slices in tissue culture—largely because one graduate student, with good enough eyes and hands and mind, has caught the bug on this one.

The most intriguing question of them all, for me, is the neural nature of the primal animalian *soul*—the psychomotor neurodynamics that help create the emotional organismic coherence that is so evident by looking at the other mammals with whom we are so fortunate, still, to share the earth.