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Confessions of a Heretic

My Unlikely Career as a So-Called Psychologist

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Every work day, as I make the 10-second commute from the breakfast table to my home office, I am grateful for my life as an academic psychologist. To me, of all the ways that a person could pass through a lifetime, a career devoted to learning and to teaching others is surely among the best. I think of myself as fortunate heir to a long and venerable tradition of devotion to the life of the

mind and the world of ideas, going back at least as far as Socrates, and to a comparable tradition of searching for the ultimate meaning of things, going back at least as far as the Buddha. Like me, both of them seem to have avoided ever having a normal job.

Yet, even though I call myself a developmental psychologist and that is what I am considered by others, I feel less comfortable in the more contemporary tradition of psychology. I love psychology as the exploration and creation of ideas about human life and human development. I love listening to people talk about how they understand their development, their relationships, and their beliefs, and integrating what they tell me into my own ideas about who we are and how we develop. I especially love learning about the ways of people in cultures other than my own, and the challenge of trying to see life from their perspective.

However, I am acutely aware that my way of approaching psychology is not the dominant way, which is much more quantitative, less cultural, and less focused on ideas and big questions than I would prefer. This awareness has often made me feel like a heretic within psychology. I have been amply rewarded by the field of psychology, yet I have never really felt like I belong here. Although

I greatly enjoy what I do, I have the persistent feeling of being an outsider and of wishing the field of psychology were different than it is. I would say that the main theme of my career is this status as an outsider and a dogged insistence on going my own unorthodox way.

Early Years: Other Aspirations

I was born in Detroit, Michigan, in 1957. I came to developmental psychology relatively late. As a kid, I imagined becoming a sports star (football or basketball) and then president. Not very original, or very realistic, but something more related to my actual future is that I was writing from an early age. I wrote my first “book” (complete with illustrations) at age 5 (“I Saw a Hill”), and by third grade I had written a short story about a dog named “Legs.” So, a love of writing, and a literary way of writing, was evident in me from early on.

Psychology never crossed my mind in childhood or adolescence. I did take a psychology course in high school, but I don’t remember much about it except that a hypnotist visited class one day. Alas, like most of my high-school courses, it demanded little of me and I didn’t learn a thing.

I went to college at Michigan State University, a common path for someone growing up in suburban Detroit. Since I liked to write, I declared journalism as my major upon entering MSU. However, in my very first semester, one of my courses was introductory psychology, taught by Bertram Caron. He must have been about the last person in the country (if not the world) to be using Freud’s *Introductory Lectures on Psychoanalysis* as the main text for an intro psych class. Of course, I didn’t know that at the time. All I knew is that I loved it.

I didn’t find the Oedipal tale plausible even then as the foundation of early development, but I found the idea of the unconscious fascinating and illuminating (and I still do). That same semester I was taking a wonderful short story course, in which we read *The Dead* by James Joyce, among other classics. From *The Dead* I absorbed the

revelation that we can never truly know another person. From Freud I received an even more startling revelation: We never even know ourselves, because our most important motivations remain unconscious. Somehow these dual epiphanies were exhilarating rather than depressing or disorienting. They seemed to be essential keys to understanding an otherwise baffling world. I immediately changed my major to psychology.

The rest of my undergraduate psychology experience was considerably less exciting. I took great courses in economics, biology, chemistry, and history, but my courses in psychology left little impression. The only ones I remember at all are two I took in my senior year, on humanistic psychology and the psychology of religion, both taught by Benjamin Beit-Hallami, a visiting professor from Israel (and, I later learned, a major scholar in the psychology of religion). I loved both courses, and maintained an interest in both topics for many years afterward. In fact, I still have a great interest in the psychology of religion and include religious questions in all my interviews (although I am personally an agnostic).

I had my first experience with research my senior year, and it was not a pleasant one. I thought I might go into neuropsychology, which was even then (1980) an exciting and growing area, so I signed up as a research assistant in a project examining the role of the hypothalamus in hunger. My job was to anaesthetize rats, cut off the top of their skulls, insert an electrode, monitor the signals from the hypothalamus, then toss the rats in the garbage can. The poor rats! Whatever we learned from that dubious experiment, it can't have been worth their suffering, if you ask me. That was it for me and neuropsychology.

I graduated with no idea what I was going to do. I was aware I'd need to attend graduate school in order to go anywhere in psychology, but after that rat experience I didn't think I wanted to be a researcher, and the only other option seemed to be clinical psychology. I didn't want to become a clinical psychologist. I knew I was ill-suited to listening to people pour out their sorrows all day. The weight of those sorrows would soon break me.

Instead, I became a musician. During college I had taken up the guitar, and the summer after I graduated I played constantly. I played for the love of it, not with any idea of making a living off of it, but by the end of the summer, with no other job prospects in sight, I realized that I probably knew enough songs, and played and sang them well enough, to get paid for doing it. For the next 2 years I was a solo performer, playing and singing acoustic guitar songs (lots of James Taylor) in bars and restaurants. I didn't make a whole lot of money, but I didn't need a whole lot, as I had moved home after graduation and lived with my parents rent-free. And it was great fun, playing and singing all the time and actually getting paid for it. I had begun to write songs, too, and I poured a lot of my time and energy into writing and recording.

Meanwhile, I considered what to do next. I loved music, but I didn't have any intention of making a career out of it. I knew that if I did I wouldn't love it any more, eventually. I investigated the possibilities in psychology, by obtaining a book on graduate programs. I looked half-heartedly at clinical programs, but then I discovered something I had never heard of before, called *developmental psychology*. As described in the book, this was a branch of psychology that involved research on normal development, including intervention and prevention programs to enhance development. I knew instantly that this was for me.

It may seem odd that I first learned of developmental psychology only after graduating with a degree in psychology. It certainly seems odd to me now. How did I manage to get through a whole program in psychology without even knowing that developmental psychology existed? I guess the explanation is that this is what can happen when you are a student at a university that has (at the time) 45,000 other students and you are a psychology major among thousands of other psychology majors. I don't recall ever meeting with an advisor.

In any case, having discovered developmental psychology, I eagerly applied to graduate programs. Because I knew so little about the field, I didn't have a lot to say about what area I wanted to research. My application stated only some vague intentions of understanding the sources of problems by focusing on early development. Nevertheless, I was accepted at the University of Virginia and soon began the program there with great hopes and expectations.

Graduate School and Postgraduate Years: Still Searching

The developmental psychology program at the University of Virginia was (and still is) an excellent program, one of the best in the world. At the time I was there, it had many top developmental scholars, including Mary Ainsworth, Robert Emery, Mavis Hetherington, and Sandra Scarr. Still, I realize now that it was very conventional in its assumptions about development. The focus was entirely on early development—nothing on adulthood or even on adolescence—and on American children. The cultural issues that would later become the foundation of my own views of development were never mentioned and never occurred to me.

For my first 2 years I worked with Mavis Hetherington, as part of a big longitudinal study of stepfamilies. I was part of a graduate student team that coded videotaped interactions between parents and children. The videotapes were fascinating to watch. However, coding every "utterance" was tedious, and seemed to drain the life from the exchanges between parent and child. By the time we were done, all that was left was a series of numbers that was a mere shell of what we had watched. This was, I think now, the beginning of my disillusionment with traditional methodologies in developmental psychology.

By my third year I was tired of being a cog in a research machine, and looked around for other opportunities. Lucky for me, Sandra Scarr had just arrived as the new chair of the department, and we hit it off immediately. Sandra's approach to working with graduate students was the polar opposite of Mavis's. Like many prominent researchers running big projects, Mavis wanted a team of graduate students who would fit into all the different roles that would make the project go. Sandra, in contrast, wanted me to come up with my own idea for what to do, and that was a freedom I sought by now. I told her I was interested in doing something on early childhood, maybe related to day care, one of the many topics she had researched. She made a couple of calls, and soon I was set up to do an evaluation of day care quality on the island of Bermuda.

Yes, Bermuda. When I tell people this is where I did my dissertation research they usually respond with a smile and raised eyebrows, but it's not like I spent a lot of time snorkeling or lying on the beach. I worked from sunup to sundown every day during the week, visiting day care centers and rating the interactions between the caregivers and the children. On the weekends I sorted through the results of the previous week and prepared for next week. Nevertheless, I admit it was a glorious place to be for five weeks. By the time I returned home, I had all the data I would need for my dissertation.

My dissertation seems like a rather small thing to me now. Certainly there was no great originality to it, and no notable intellectual content. At that the time my main interest was in learning ways of enhancing the development of young children, and the dissertation focused on the practical issue of training caregivers so they would provide higher quality care. However, by the time I graduated, I realized it was not going to be enough for me to be involved in the practical application of research. I needed something intellectually meatier, something more creative and conceptual, something that would involve new *ideas*. But I did not know what that something would be.

I graduated in less than 2 years, and then as now the expected route was to find a postdoctoral fellowship and spend a few more years gaining research experience and publishing articles before seeking a tenure-track position at a university. However, I had no interest in following that route. Although I enjoyed my dissertation research, for the most part I didn't like what I had seen of research in developmental psychology so far. The dominant methods, especially questionnaires, seemed to me to hollow out rather than illuminate the human experience. The most esteemed status seemed to be to design a large study, obtain a multi-million dollar grant, recruit an army of graduate and undergraduate students, and wait for them to bring you the results after the people participating in the research had been reduced to mere numbers and displayed in a statistical model. I didn't believe this was the royal road to understanding human development, and I still don't. And I knew I didn't want to devote my life to it.

Well, then, what to do with my newly minted Ph.D.? I had taught a couple of courses in my final year as a graduate student, and discovered that I loved teaching. I sought and found a position at a liberal arts college, Oglethorpe University in Atlanta, where there was teaching galore—four courses a semester! I'm sure Sandra and my graduate student peers thought I was insane. Taking a teaching job like this must have seemed like a disastrous decision, or at any rate a decision that would put an end to any prospect of a research career. Who could find any time for research while teaching four courses a semester? But, not for the last time, choosing what appeared to be the unconventional, misguided, even bizarre path proved to be remarkably fruitful. In fact, going to Oglethorpe proved to be the turning point of my life, both personally and professionally.

At first it was all teaching, all the time. Preparing for, teaching, and grading four courses was all-consuming. However, by the beginning of my third year I had all my courses pretty well whipped into shape. I began to turn my attention to the possibility of doing research, but on what?

In one of my courses that year I had a student who was an avid fan of heavy metal music. He knew I was a musician, and after class he would sometimes try to persuade me of the musical virtues of heavy metal. I was skeptical of his argument—it is definitely not my kind of music—but I was fascinated by his devotion to the music. This was also a time (the late 1980s) when heavy metal was receiving a lot of negative media attention as a supposed peril to America's youth—all, I noted, without any basis in research. So, really just for fun, I decided to start interviewing heavy metal fans. I put up a sign in a music store offering a free heavy metal tape (this was before CDs, let alone iTunes) to anyone who would agree to be interviewed, and soon I had a substantial sample of young "metalheads."

What they had to say about the music surprised me. First, most of them were bright and thoughtful, a sharp contrast to the media image of metalheads as brutes and ignoramuses. They were deeply cynical about the adult world, and they loved heavy metal because of its blunt honesty in expressing that cynicism. Second, they saw heavy metal as great music and revered their heavy metal heroes as great musicians—again, a sharp contrast to the general portrayal of heavy metal as noxious noise. Third—and most compelling of all—when they talked about the effects of the violent music they loved, not only did none of them say it provoked them to violence, but many of them said that the primary effect of it was to *calm them down*.

I was excited to find something so unexpected and so contrary to what was generally assumed to be true. Even more exciting, however, and even more important for my future as a researcher, was that I had found a method, interviewing, that I loved and believed in. In contrast to the questionnaire and observational methods I had learned in grad school, interviews seemed to me to get to the heart of

the human experience and account for the whole person. For the first time, a career as a researcher appealed to me.

Something else happened that year that was even more pivotal for my future. Although it was a “university,” Oglethorpe was a small school, with barely over 1,000 students, and I was, personally, half the psychology department. Consequently, I came to know many of the students well, especially the psychology majors. One day a senior psych major, Lene Jensen, stopped by my office around the time she was to graduate. I knew her well from many classes she had taken with me, and we had enjoyed talking together about all sorts of topics, but I was totally surprised when she suggested we continue to meet and talk after she graduated. Twenty-three years later the conversations continue, and we are still happily together.

Lene has been the key to my life, as my marriage partner and as my partner in parenting our twins, Miles and Paris (now 12 years old). We have also been intellectual partners. She is the smartest and most thoughtful person I know, and we share a critique of the dominant methods in psychology, a critique we have no doubt developed together. The disparity between the scholarly approach we favor and the approach that dominates developmental psychology has been frustrating sometimes, but it has never been lonely because we have shared it together.

When we began living together she had already applied to graduate schools in psychology and philosophy, and at first we assumed our time together would be brief. In the fall she would head for graduate school in the Northeast or the Midwest, and I would remain in Atlanta. However, from day one we really, really, really liked living together, and by the time she was accepted into the human development program at the University of Chicago we were starting to think about ways to stay together. Serendipity intervened: I learned of a postdoc on adolescent development at the University of Chicago, applied for it and got it. This opportunity seemed to fit well with my new interest in adolescence, inspired by my interviews with heavy metal fans. Now I would be able to refrain as a researcher on adolescence, having learned nothing of the academic literature in the area as an undergraduate or graduate student.

Here was another unconventional career move that admittedly, as I look back on it, seemed to have rather slim chances of succeeding. Who would leave a fourth-year tenure-track academic position for an eight-month postdoc? What would you do at the end of those eight months? Nevertheless, I didn't hesitate. What I knew, unequivocally, was that I wanted to be with Lene, and that I didn't want to spend the rest of my career at a small college. (As with music, I knew that if I did nothing but teach for year upon year, I would stop loving it.) What would happen next, I had no idea. But I knew, from all my years as a musician, a student, and a penurious assistant professor, that I could live on very little if necessary.

Little did I know it when I arrived, but my period at the University of Chicago would be the formative intellectual experience of my career. The only reason I was there was

because Lene was there. Yet it turned out to be just the place for me, too.

The human development program at the University of Chicago is unusual, in that it especially emphasizes the fusion of anthropology and psychology. At the time I was there the faculty included two eminent anthropologists, Richard Shweder and Gilbert Herdt. I had never read a thing in anthropology, but now that I was at the U. of C. I decided to read Herdt's ethnography of adolescence among the Sambia, a tribal culture in New Guinea. Herdt described a series of remarkable rituals that Sambia males undergo from middle childhood until their first child is born. Highlights include ritual nose bleeding (based on a fear of female menstrual blood) and a period of fellatio of older boys by younger boys (based on a belief that younger boys need to ingest semen in order to grow into manhood).

That ethnography challenged and rearranged my worldview in ways that still endure. I had never read anything like it. In the course of my conventional training in developmental psychology, I had absorbed the conventional assumptions that the purpose of psychology is to search for universal principles, and that this can be done by studying Americans and then generalizing to the rest of humanity—so much so that I didn't even think of them as my assumptions. The stunning contrast between adolescence among the Sambia and adolescence as I knew it in the United States convinced me that these assumptions were false. I saw that I would have to rethink everything I had learned and come to a new understanding of human development. It was thrilling to learn a cultural way of thinking that seemed to illuminate so much more than what I had learned so far.

My 8-month postdoc passed quickly, but I found another postdoc through the U. of C. and Northwestern University that extended for two more years. The new postdoc was run by Daniel Offer, a psychiatry professor at Northwestern and a major figure in adolescent research. Most postdocs involve working on someone else's data, but Dan let us do whatever we wanted as long as we were productive. There were about eight of us, half postdocs and half doctoral students at the U. of C., and we had free-wheeling weekly meetings in which nothing was sacred. We all liked Dan, but we didn't hesitate to challenge him. I cringe when I think of just how much I challenged him—now that I'm the age he was then, I wish I had been more respectful!—but to his credit he welcomed it.

I'm also grateful for the freedom he allowed us. It was just what I needed in order to read widely and gain my footing in adolescence as a theorist and researcher. In thinking about possible research areas, I knew I didn't want to continue researching heavy metal fans. Fascinating as they were, I felt I had learned enough about them, and I didn't want to become known as “that heavy metal guy.” What, then? Reflecting on my own youth, what stood out most was all the reckless things I did. (A quick, G-rated example: One college summer I hitchhiked 8,000 miles, from my Michigan home to Washington state, down to L.A.

and then all the way back.) I decided to study risk behavior in adolescence. Lene and I were planning to travel to Denmark that summer, and together we devised an ambitious plan to survey adolescents in nine schools in various regions, which also gave her a chance to show me around her home country. We eventually published the results in *Child Development* (Arnett & Balle-Jensen, 1993), and I'd call that my first notable empirical paper. During the same time, dissatisfied with the existing theoretical models of risk behavior, I decided to devise my own, and it was published in *Developmental Review* (Arnett, 1992a, 1992b), my first notable theoretical paper.

Emerging Adulthood and My Own Emergence

My postdoc idyll had to end eventually, and in 1992 I took a position at the University of Missouri in the Department of Human Development and Family Studies. Lene had finished 3 years of her doctoral program, including all her course work, so she was able to join me and continue her dissertation research there, on the cultural basis of moral development. I was once again uncertain about which research direction to pursue. I found the risk behavior area interesting, but it also seemed to me like it was already staked out, already thoroughly plowed over, again and again, with hundreds of researchers building on a corpus of decades of research, making it difficult for anyone new to be heard. I wanted to find uncharted territory, something no one had done before, something fresh that would inspire me to create something valuable and enduring.

Thinking again of my own development, I reflected on how I finally felt I had reached adulthood, at age 35. I had at last finished my long education, I had what seemed like my first "real job," I was engaged to be married, and Lene and I had bought a house. How, I wondered, do other people experience the transition to adulthood? How do they define adulthood, and how do they know when they've reached it?

I began interviewing people in their twenties, to find out. I didn't want to focus on college students, easy as that would have been at a big university, because few are above age 23 and even among 18–22 year-olds they represent only about half. Instead, I obtained alumni lists from local high schools and had research assistants look up addresses and phone numbers in the phone book (this was in 1992, before the Internet). Eventually we put together a sample of 18–29-year-olds that was diverse in educational levels and work statuses. Then and ever since, I have learned the most from the people who were of lower socioeconomic status (SES) because their experiences were so different from my own. I still remember a lot of those interviews vividly 20 years later, such as the one with the truck driver who had grisly tattoos all over his body and dreamed of owning a tattoo parlor.

In response to the question about what it means to be an adult, the answers fascinated and surprised me. I was

expecting people to point mainly to transition events such as finishing education, settling into a job, and getting married, not only because these were important in my own life but because these events had been the focus of a large literature on the transition to adulthood, mainly in sociology. Yet in interviews, people almost never mentioned them! Instead, the word "responsibility" came up over and over again, and nearly always in the context of responsibility for one's self rather than for others. Making independent decisions was next most frequent, and it was closely related to responsibility for one's self, in that both reflected individualistic values and signified the goal of becoming self-sufficient. Financial independence was third, and here again was the individualistic emphasis. Although unexpected at the time, those three criteria—accepting responsibility for one's self, making independent decisions, and achieving financial independence—have been remarkably consistent as the Big Three criteria for adulthood, for 20 years now in studies across SES groups, age groups, regions, and ethnic groups in the United States, and in countries all around the world (e.g., Arnett, 2003; Macek, Bejček, & Vaníčková, 2007; Mayseless & Scharf, 2003; Nelson, Badger, & Wu, 2004). Just as with my earlier finding about the cathartic effect of heavy metal music, it was exciting to discover something new and unexpected, and to be the first to describe what had previously been uncharted territory.

Another surprising finding of the interviews was the ambiguity in how they described their own progress to adulthood. Most people didn't give a simply "yes" or "no" answer when I asked them if they felt they had reached adulthood. Instead, their answers were usually multi-faceted; they felt like adults in some ways but not others. It was this answer that inspired the term *emerging adulthood*, to describe their sense of being no longer adolescent but not yet adult, on the way to adulthood but not there yet.

However, I was not yet ready to propose the idea that there was a new life stage of emerging adulthood in between adolescence and young adulthood. Instead, I was enjoying the interviews and focusing on learning more. Although my initial interest was in how people think about the meaning of adulthood, my interview covered pretty much everything, from family relationships to religious beliefs to hopes for the future. In 3 years I interviewed about 150 18–29 year-olds in Missouri. Then I had a fellowship year in San Francisco where I focused on African Americans and Asian Americans (as my Missouri sample had been almost entirely White). Like the rural low-SES Whites in Missouri, the African Americans and Asian Americans in San Francisco taught me much that was invaluable and indelible. I still remember many of the people I interviewed there, even where we met, what they wore, and their distinctive ways of expressing themselves.

Two other things happened during our time in San Francisco that made it a pivotal year. One was that I became involved as an expert witness in litigation against

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the major tobacco companies. How this happened, and what it was like, is a long story, perhaps worthy some day of an essay in itself, but there is not room enough to recount it here. For now let me simply say that it was unlike anything I've ever done, in both its rewards and its challenges. If you think academia can be cut-throat, try the legal world some time. It's set up to be adversarial, with one side clearly winning and the other clearly losing, and when billions of dollars are at stake, as in many tobacco cases, both sides go all out to win and have no compunction about chewing up and spitting out a few expert witnesses along the way. But I've loved it, partly because it appeals to the pugnacious side of my personality, and partly because of the exhilaration of fighting for a good cause against the Evil Empire of Big Tobacco, and often winning. Since the multi-state settlement in 1998, which imposed marketing restrictions and required the tobacco companies to pay \$248 billion toward smoking prevention and treatment programs—the largest legal settlement in history—smoking among American high-school seniors has gone down nearly every year and is now barely half the rate it was then. I hasten to add, I have been only a bit player in the drama—the real heroes are the plaintiffs' attorneys, who took immense financial risks in what seemed at the time like a lost cause—but even so it has been a wonderful experience to be part of it, and I regard it as one of the most satisfying parts of my career.

The other big event of that year was that I wrote what became my first *American Psychologist* paper (Arnett, 1999). Since I became interested in adolescent research I had been struck by the oddness of the debate in the field over the concept of “storm and stress.” On the one hand were the views of G. Stanley Hall, who originally applied the “storm and stress” to the emotional and behavioral upheavals of the adolescent years, and Hall's successors today who were alleged to share Hall's alleged negative views of youth. On the other hand were today's adolescent psychologists, who had supposedly dispelled this negative stereotype and whose research had revealed the happy news that adolescents were doing just fine—no storm, no stress. It seemed to me that both sides were off the mark, and that the truth was both more complicated and more interesting.

After reading widely (including Hall's brilliant two volumes of *Adolescence*) and working on the paper for months, I submitted it to *American Psychologist*. Fortunately for me, it fell into the hands of a superb editor, Ann Masten. I'll always be grateful to Ann, and to the three anonymous reviewers, who recognized an amateur when they saw one but also perceived that there was the germ of a good idea in the paper that could potentially become a gem. To me this was the gold standard for the academic peer-review process: an attentive, insightful editor, and exacting but knowledgeable and fair reviewers, who, together, require and inspire the author to strive for excellence. When it works this way, it's an amazing and wonderful process.

Flapping Like Crazy: Becoming an Independent Scholar

Despite the fun of the year Lene and I had in San Francisco—or maybe because of it—as we returned to Missouri I felt deflated and dissatisfied. I had been granted tenure just before we left for San Francisco, and for most academics tenure is a major career milestone, a seal of accomplishment and a great relief for the security and stability it provides. However, it made me depressed and anxious. I was certainly happier to be awarded tenure than I would have been to fail to get it, but I didn't really feel like I had accomplished anything notable. Everything I had published seemed to disappear quickly in the vast ocean of the psychological literature after creating barely a ripple. Similarly, we liked our Missouri college town well enough, and we had a lovely home in a lovely neighborhood, but the town seemed smaller with each year, especially after our year in San Francisco.

Lene had now finished her dissertation, and during our San Francisco year she had completed a postdoc at the University of California-Berkeley, so she was ready to try the academic job market. She received an offer from the Catholic University of America in Washington, DC, and we had a decision to make. They had nothing for me, and there were no openings at other universities in the area. After a little but not much deliberation, we decided to go. I would leave my tenured Associate Professor position and leap into an unknown future.

This was another unusual decision, certainly, leaving a tenured position at a major university with no prospects in sight. However, like the earlier decision to leave Oglethorpe, it was not as difficult a decision as it may seem. First, Lene and I have always valued having an equal marriage, and after I had followed her to Chicago, and she had followed me to Missouri, it was my turn to follow again. Second, I wasn't giving up something I really valued. The security of a tenured position made me feel stultified, not secure. I was attracted to the prospect of having unbroken days for writing and research, without the obligation of teaching and the wasted time in committee work, and I thought the risks of my leap into the unknown might inspire higher accomplishments. “The prospect of being hanged in a fortnight concentrates the mind wonderfully,” opined the venerable Dr. Johnson a few hundred years ago. I thought that might be true for me, in a less dire, more academic way. Maybe if I leapt into the abyss, I'd learn how to fly. At the very least, I'd be flapping like crazy.

I had been invited to write a textbook on adolescent development, and I accepted the offer. I still didn't have any real training in adolescence—my exposure to adolescent research at the U. of C. was unconventional, to say the least—and I figured that by writing a textbook I'd at last gain a thorough grounding in the field. Now that I had left Missouri, I'd be able to handle the immense time demands of writing a first-edition textbook. I also imagined that, if

it sold well, I'd make enough income from the textbook that I wouldn't have to hurry to find another academic position. I could live as an independent scholar as long as I liked.

In 1998, during our first year in the DC area, I received word that my storm and stress paper had been accepted in *American Psychologist*. This was immensely inspiring to me. It made me feel I had at last broken into the top echelon in academia, and that psychology might be receptive to the kind of conceptual, big-idea papers I wanted to write. On the strength of this inspiration, I immediately began working on another paper intended for *American Psychologist*, this one a theoretical paper introducing emerging adulthood.

I had already been thinking of the age 18–25 period as emerging adulthood for years by this time, ever since I began my interviews in the early 1990s. I realized quickly that it didn't make sense to call them "late adolescents," as they were not going through puberty, not in secondary school, not minors under the law, and (mostly) not living at home. Nor did I find "young adults" appropriate, because this term had connotations of a more settled life stage lasting at least until age 40. I also rejected other terms that had been used occasionally such as "youth" (too vague) or "postadolescents" (defining them by what they were not). I had to call them *something*, at least in my head, and "emerging adults" seemed to fit as a new term for a new life stage that I viewed as developing only within the past half century.

I actually used the term *emerging adulthood* for the first time in an article published in 1994 in the *Journal of Youth and Adolescence*, at the end of a review paper on the transition to adulthood (Arnett & Taber, 1994). I used it again in 1998, at the end of a paper in *Human Development* on conceptions of adulthood (Arnett, 1998). However, both times I used it tentatively, as an afterthought at the end of a long paper, and no one seemed to notice. Now I was ready to shine a bright light on it and present it not just as a new term but as a theory of a new life stage, and I knew *American Psychologist* would be the place where it would have the best chance of gaining attention.

I wrote it quickly, having prepared the groundwork for years, and it zipped through the review process in short order and was published in 2000 (Arnett, 2000). I was happy to have it published, but I certainly didn't think that I had just published the paper that would change my life—as it did. I had grown accustomed to publishing papers that I thought were brilliant but that few other people seemed to notice. But this time it was different. It took several years, but gradually the paper gained momentum and came to be regarded as the spark that ignited the new field of emerging adulthood, even though it was really only a sketch of a theory and required a book (Arnett, 2004) to be developed fully. I think the timing was right, in that there were numerous other scholars who were fascinated by development during this age period but didn't have any term for it or any way of conceptualizing it. I proposed the idea that this

was a new life stage, and gave it a name that was new and distinctive, and soon many scholars who had an intuitive sense of this similar to mine embraced the idea. As I write this, in April 2012, that *American Psychologist* paper has been cited over 2,700 times, according to GoogleScholar, not just in psychology but in a wide range of fields.

Although not many people noticed the paper in the first months after it was published, one person who did became a crucial figure in the development of the field of emerging adulthood. Jennifer Tanner was a postdoctoral student at Penn State at the time. She was interested in relationships with parents during the transition to adulthood, and looking for a theory that would fit what she was seeing among the young people she was studying. She seized on the *American Psychologist* paper as the framework she was looking for, and contacted me with great excitement and eagerness to put it into action.

So began a remarkably fruitful collaboration that continues to this day. Although I proposed the initial idea, it is Jenn who is primarily responsible for developing emerging adulthood into an organized field of scholarship. It was Jenn who discovered the grant program through the *American Psychological Association* that provided the funding for the first conference on emerging adulthood, in 2003. It is Jenn who has been the driving force behind every biennial conference since then (the fifth conference was held in 2011). It is Jenn who is the main organizational force behind the newly established *Society for the Study of Emerging Adulthood* (SSEA; see www.ssea.org). I do not have the talents or the patience required for the kind of work involved in building an organization from the ground up, but Jenn does. We have very different personalities, but that is precisely what makes our partnership work. We develop ideas together, and Jenn is the one who translates the ideas into action.

In DC, I thrived on the independent scholar life. I didn't miss a thing about having an academic position. I had always preferred to do my writing and research mainly on my own, so I didn't miss having graduate students, and in any case Lene and I continued to be each other's main counsels in all our work. Now I had Jenn (and soon many others) as my collaborator in developing the emerging adulthood field. I sure didn't miss the politics and the committee work that are unavoidable in academic departments.

Being an independent scholar allowed me to take on a lot of new challenges and opportunities as they came my way. I became Editor of the *Journal of Adolescent Research* in 2002. I edited four encyclopedia volumes, two for the *International Encyclopedia of Adolescence* (Routledge) and two for the *Encyclopedia of Children, Adolescents, and the Media* (Sage), all four of which came out in 2007. I wrote another *American Psychologist* paper (Arnett, 2002) on an entirely new topic, the psychology of globalization. I worked on the second edition of my textbook *Adolescence and Emerging Adulthood: A Cultural Approach*, after the first edition came out in

2001 (Arnett, 2001). I continued to work as an expert witness in litigation against the tobacco companies.

In combination, this made for a full schedule, but it was also a flexible schedule, and this was especially valuable once our twins Miles and Paris were born in 1999. We had a nanny for 35 hours a week the first 2 years, and a wonderful child care program about 30 hours a week after that, but, as any parent knows, many an unexpected event can be expected in children's early years—from ear infections to snow days—and there has to be someone there to step in and take care of the kids. Lene and I were equal partners in parenting all the way, but I had the more flexible schedule because she was the one with the academic position.

I remain an independent scholar today. Lene and I moved to Clark University in 2006, after a fellowship year in Denmark, but I do not have a normal academic position there. My title is Research Professor, which means I teach one course per semester (at that level I really enjoy teaching) and have no committee responsibilities. I occasionally guide undergraduate and graduate students in research, but mostly I work on my own at home. I am happiest, and I think and write best, when I am working in my home office every day, with Lene as my mutual confidant, sounding board, and lunch partner.

My Uneasy Place in Psychology

If you were to look at the course of my career, say by scanning my CV, you might conclude that this has been the career of someone who has a comfortable place in academic psychology. Almost every article I've published has appeared in psychology journals. I am the editor of a journal that is classified as part of the psychology area. My book on emerging adulthood was published in the psychology section of Oxford University Press. My textbook on adolescence and emerging adulthood is used mainly in psychology courses. When I testify as an expert witness I draw mainly upon research and concepts from psychology.

Yet the truth is that I have never felt entirely at home in the field of psychology, and I have frequently experienced frustration with the dominant pattern of how scholarship is conducted in the field. For nearly my entire career, since my U. of C. days, I have felt like an unbeliever among believers, surrounded by people who share a view of how human development research should be done that I vehemently reject. I have touched on these issues earlier in this chapter, but let me now address three of them directly: the cultural narrowness of psychology, the sterility of the dominant methods, and the ill-conceived ideal for research programs.

Ever since the epiphany of my U. of C. days, when I read anthropological works on adolescence for the first time, I have considered myself a cultural psychologist. This means that I always see the psychology of human development in a cultural context. There are some biologically based universals in development, yes, but the differences

among cultural groups are vast, and where people happen to have been born makes an enormous and decisive difference to the course of their development. To me the huge, enticing, inexhaustible question at the heart of psychological research is the question of how cultural beliefs and practices shape the raw material of biological development into an amazing array of patterns.

Yet this question is, at best, only in the margins of the field of psychology. Cultural psychology has risen in influence over the past 20 years, and is now widely recognized by other psychologists, but the mainstream of the field continues to flow on as if cultural psychology—and culture itself—did not even exist. In 2008 I published an *American Psychologist* article entitled “The Neglected 95%: Why American Psychology Needs to Become Less American” (Arnett, 2008). As part of that article, I performed an analysis of the nationality of authors, samples, and editorial board members in a wide range of major journals in psychology. The results were depressing, though unsurprising: the leadership and content of all the journals was entirely dominated by Americans, with little attention to cultural variation even within American society.

It's not that I have anything against Americans—I am, after all, an American myself—but it seems bizarre to me that a field that claims to be devoted to understanding the psychology of humanity should be satisfied with a focus on less than 5% of the human population. The basis for this narrowness, as I pointed out in the article, is mainly the dominant philosophy of science in psychology, which emphasizes the search for human universals rather exploring the psychological basis of human cultural diversity. Now, with the surge of interest in neuroscience, the focus of psychology is likely to become even more focused on (alleged) universals and even more content to focus mainly on middle-class Americans. Along with a small cluster of culturally oriented colleagues, I spend a lot of my time and energy protesting this narrowness and advocating a more cultural and international approach to psychological research, but it often feels like howling at the moon. The moon is unperturbed.

The second aspect of my critique of psychology has to do with the dominant methods, specifically, questionnaires. My first experience with interviewing, in my study of metalheads, converted me permanently to a belief in the value of qualitative methods. I have used questionnaires as well, and I think the combination of questionnaires and interviews is especially fruitful. However, it is the interviews that have really taught me about human development. Questionnaires are rife with all sorts of assumptions about how people will respond to questions, and they presort people into categories determined by the creator of the questionnaire. I understand the necessity of this, especially in large-scale studies, but to me this approach is incomplete at best. Questionnaires provide the skeleton, but they need the flesh of interviews to come alive.

Yet in psychology, especially in adolescent psychology, large-scale questionnaire studies (with no qualitative

component) are by far the dominant method. In the flagship journal of the Society for Research on Adolescence, the *Journal of Research on Adolescence*, qualitative studies are scarce. The overwhelming majority of articles published in the journal consists entirely of questionnaire data, as if questionnaires were some kind of gold standard of scientific merit. In the course of writing this chapter I examined the most recent issue of *JRA* (Vol. 21, No. 4, 2011) and recorded the methods used in each study: one was an experiment, two involved interviews, one used interviews and videotaped observations, and 12 were based on questionnaires. Articles on adolescents and emerging adults in other “top” journals such as *Child Development* and *Developmental Psychology* are also mainly questionnaire-based (although for infants and young children studies more often include experiments or observations). Increasingly, various questionnaires are stuffed into elaborate statistical models, the results depicted in figures with arrows extending from one questionnaire construct to another, supposedly unveiling the complex relationships among them.

Virtually no one seems to ask if the questionnaires are valid, if they are actually measuring what they purport to measure (internal reliability seems to be enough to satisfy editors and reviewers). Few people seem to notice that by the time the questionnaires chop people up into assumption-laden variables and grind up the variables in a statistical model there is no life left to them—only fragments and limbs but no human beings.

My third critique of psychology concerns the dominant model for research programs. The ideal in the field seems to be to run a large research enterprise. Graduate students are encouraged to begin grant writing early, and early career researchers are told that obtaining external funding is one of the expectations of most entry-level academic positions. Sometimes success in obtaining a large grant is even required for promotion to Associate or Full Professor.

But this emphasis locks psychological research into a scientific model that is highly questionable. In this model, the main function of the researcher is to obtain money, in the form of a large grant. The researcher, as “Principal Investigator,” designs the study and writes the grant, often in collaboration with colleagues. If the grant is obtained, the researcher enlists graduate and undergraduate students to do the actual data collection, which usually consists mostly of having people fill out questionnaires. The only way the researcher ever has any contact with the participants in the study is in the form of numbers, as variables in summary statistics or statistical models. Then the researcher writes articles for scientific journals, drawing conclusions from the numbers—without ever having spoken to a single person who is the subject of the conclusions.

This strikes me as a misguided way to learn about human development. It is a model of scientific research drawn from other sciences, and maybe it works in biology or physics but it seems inadequate for studying human beings. The biologist can’t know what it is like to be a

fruit fly, and the physicist has no experience of being a quark, but all of us know what is like to be human. The most important asset, the most important research instrument, of scientists studying human development is their own essential humanity, their own insights and understandings wrought over a lifetime of their development. In interviews and in ethnographic research this humanity can be brought to bear, so that it illuminates and produces fresh insights and understandings. Without this, with only dead data from second-hand questionnaires, the results are sophisticated, complex, esoteric—and inert.

It is not questionnaires or statistics that I object to, per se, but the hegemonic dominance of quantitative methods, with anything qualitative—anything that involves listening to people and seeking to understand them as one person to another—shunted to the margins as “unscientific.” I don’t object to potato chips, either, but I wouldn’t want to eat nothing but potato chips for breakfast, lunch, and dinner. In adolescent psychology it is pretty much all potato chips, all the time, and that is not good for the health of the field.

Final Reflections: The Heretic Finds a Home

Although I have had an unorthodox career in psychology, and my awareness of the difference between how I see things and how most research psychologists see things has often made me uneasy, I have no regrets about devoting my work life to the study of human development. At its best, as exemplified in the mixed-methods work of psychologists like Philip Hammack, Lene Jensen, Reed Larson, and Niobe Way, sociologists like Mark Regnerus and Christian Smith, and anthropologists like Susan and Douglas Davis, human development research can be illuminating and mind-expanding. It is their standard that I have sought to emulate, and it is the companionship of them and others that has kept me from feeling lonely as I have traveled my unconventional path.

I am critical of the dominant approach to scholarship in psychology, but other than my critique in “The Neglected 95%,” I have sought to devote most of my energies to constructing rather than deconstructing, to building up rather than tearing down. In 10 years of serving as Editor of the *Journal of Adolescent Research*, I have given it a distinct identity as a journal where qualitative and mixed-methods research on adolescence and emerging adulthood is not only welcomed but highlighted (Arnett, 2005). In creating with Jenn Tanner the *Society for the Study of Emerging Adulthood*, I have sought to make it international and open to scholars from diverse disciplines with wide-ranging methodological approaches. My textbooks, the one on adolescence and emerging adulthood, as well as a recently published life span text (Arnett, 2012), have been written with a cultural approach, in the hope of encouraging new generations to think culturally about development from the very beginning of their careers.

The idea of emerging adulthood is my main claim to fame, and it has been amazing to see it grow in the past

decade to become the basis of a new field in psychology. I receive email messages nearly every day from scholars all over the world who ask for information and want to be part of the new field. The idea has also reached a broader audience. When the *New York Times Sunday Magazine* published a cover story on emerging adulthood in August, 2010, suddenly the idea reached a general audience of millions. In the barrage of emails that followed (and continues to this day), and in many comments that I have received in speaking before various groups, I have had many people tell me that this concept helped them make sense of their lives or their children's lives. This response has reinforced my belief in the value and power of ideas. The reason I have tried to revive the reputation of life stages, which had become unfashionable in developmental psychology when I proposed emerging adulthood, is that I have seen over and over again how valuable they can be in helping people understand development, scientists and nonscientists alike (Arnett, Kloep, Hendry, & Tanner, 2011).

I suppose I will always be known as “that emerging adulthood guy,” and it is a title I happily embrace. However, I do not believe that emerging adulthood is my last idea. Lately, I have been interviewing parents of emerging adults, mostly people who are in their late forties to early sixties, and it has provoked for me a lot of new thinking about their life stage (which also happens to be the stage that I, too, am now in). I think we are in need of a whole new model of life stages, one that is historically and culturally flexible. Stages gained a bad name in psychology because they were presented in the 20th century as universal (people everywhere experience the same stages) and uniform (people everywhere should experience them in essentially the same way). But if they are recognized as being historically and culturally variable, as helpful heuristics rather than natural facts, they can be extremely valuable for advancing our understanding of human development. That sounds to me like an idea worthy of some development of its own.

Despite my ambivalence toward psychology, I greatly enjoy being an academic psychologist. I love doing interviews, I love to write, and I love grappling with ideas and trying to create new ones. On the wall of my home office is a series of photographs of a monarch butterfly striving its way out of a chrysalis. That's the kind of challenge I feel every day, and there is joy and deep absorption in the strenuousness of it. Sometimes the result of my day's work looks like something quite other than an exquisite

butterfly. Nevertheless, devoting myself to it is an honor and a privilege, and I hope to enjoy it for many years to come. The renowned EA troubadour John Mayer wrote these words in his twenties, but I'm hoping they could apply to the life stage I'm now in as well:

I'd like to think the best of me

Is still hiding up my sleeve.

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