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2022

Happily Ever After

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Publication Information & Recommended Citation

Ellsworth, Phoebe C. "Happily Ever After." In *Reflections from Pioneering Women in Psychology*, edited by Jamila Bookwala and Nicky J. Newton, 100-111. Cambridge University Press, 2022.

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Phoebe C. Ellsworth is the Frank Murphy Distinguished University Professor of Psychology and Law at the University of Michigan. She received her AB from Harvard (1966) and her PhD from Stanford (1970). She has two primary research interests. First, she studies the relations between cognition and emotion, including culture and emotion, and is an originator of the appraisal theory of emotion. Second, she is interested in the application of psychology to legal questions and has studied jury decision-making, the death penalty, and the use of social science in legal decision-making. She is a member of the American Academy of Arts and Sciences. For her research, she has been awarded the Society for Personality and Social Psychology (SPSP) Career Contribution and Legacy awards, the James McKeen Cattell Award for Lifetime Achievement in Applied Research, the Society for Experimental and Social Psychology (SESP) Distinguished Scientist award, and the American Psychological Foundation (APF) Gold Medal Award for Life Achievement. For advising students she has been awarded the Raymond D. Fowler Award for outstanding contributions to students' professional development, the SPSP Nalini Ambady award for mentoring excellence, and the Association of Psychological Science (APS) Mentor award.



*Happily Ever After**Phoebe C. Ellsworth*

I was born in Connecticut in 1944, and my world was one of privilege and plenty. Both of my parents had college degrees and believed in education, and there was never any question that their children would go to college. During my childhood, my father was at Yale, first as a graduate student, then a faculty member, then as an administrator. My mother had no job but was an avid student of history and archaeology, and not particularly subservient to my father. I was the first born of four, which may have been an advantage – just about every woman in my graduate school class was a first born. My father probably would have preferred to exercise his passion for teaching on a son, but my brother wasn't born until I was 8 years old, and by that time I had become the designated pupil. We made scrapbooks of animal species and prehistoric people, collected stamps to learn about the nations of the world, and read books, my father always choosing something at the edge of my capacity. In fifth grade, I read *Pride and Prejudice*, in eighth grade *Moby Dick*.

None of this was designed to prepare me for a career as an academic, nor indeed for *any* career. In that time and place and social class, the idea was that women should be well educated, well trained in social skills, and well prepared to become suitable wives to well-educated, successful men. This seemed perfectly normal to me, and I didn't think much about it. At the usual time, I became interested in boys and wanted very much to be attractive and popular (I wasn't), but at the same time I continued to love learning and to get top grades in school. I didn't see any contradiction here, but I knew that many people did, particularly the very boys I wanted to attract. Most of the girls who were doing well academically tried to avoid talking about it, steering the subject away from our SAT scores and, when cornered, sometimes lying about them.

I went to public school until high school, when my parents put me in a private day school for girls in New Haven. Almost no one from the high school in the town where we lived had ever gone to college, and they thought we needed a school where the chances were better.

I got into Bryn Mawr early admission, so I didn't worry about college during my senior year. I didn't choose Bryn Mawr because it was a women's school – coed private colleges were unusual in those days, so single-sex schools were the default. The classes were demanding, many of the faculty and almost all of the students were female, except for the occasional Haverford student, who never seemed particularly smarter than the rest of us. There was a tacit assumption that we would go on to have careers, that college was not the end of the line, but rather that our duty and our destiny was to make a contribution to the world. As Bryn Mawr's second president famously said, "our failures only marry." This idea was never explicitly preached, but it was part of the environment.

My plan to become a great fiction writer didn't last long. Reading James Joyce's *The dead*, I realized that there was a level of art that I would never attain. I also suspected that I probably wouldn't be able to support myself as a writer. The idea of supporting myself, rather than being supported by a husband, had not only crept into my world view but had become an absolute requirement. I don't know why, but the idea of one member of a couple getting an "allowance" from the other or having to ask whenever she wanted money struck me as both demeaning and risky.

I transferred to Harvard after my sophomore year, for ill-advised romantic reasons. I thought of anthropology as a major, because I loved it and also, probably, because it was a field where women had been successful since the early days. Despite the four anthropology courses I had taken, the Harvard anthropology department told me I didn't have enough courses and would have to stay an extra year if I wanted to major in anthropology. (I was later told that they were famous for discouraging women, but this never occurred to me at the time). So, I happily took as many courses as I could, piling up many more credits than I needed to graduate, but not enough in any specific field to constitute a major. Luckily Harvard had a major called social relations which was willing to count almost any course toward the major.

During this time, I also worked for a summer and a semester for three social psychologists at Yale – Chuck Kiesler, Barry Collins, and Norman Miller. I wasn't particularly looking for a job in social psychology, I just needed to earn some money, and knocked on doors until I found a job. I was hired as "office help." I was pretty much a failure in that role, but I was also constantly making suggestions about their research, so they made me a research assistant instead, letting me attend research meetings, and eventually help writing grant proposals and coauthoring manuscripts.

So, what with my social relations major and my work at Yale, I sort of sidled into social psychology. Various professors told me that I really had a superb talent for experimental design, and that I ought to apply for

graduate school. Having imagined myself as a writer or an artist, “a superb talent for experimental design” did not strike me as a glamorous skill, but, having no better ideas, I did apply to graduate school.

On the whole, the intellectual environment at Harvard was as stimulating as it had been at Bryn Mawr, but there was an undercurrent of sexism that was new to me and that I never quite recognized. First, there was the rumor about the anthropology department, then another female student took me aside and told me that really a woman should only talk once in a seminar, and of course in the background professors were always making sexual advances. But I was pretty oblivious and never thought of sexism as an issue. When I applied to Harvard graduate school and didn't get in, I was disappointed and also puzzled, because I was graduating *summa cum laude* and had near-perfect GRE scores. At some departmental gathering I told Amos Tversky, who was a postdoc, that I couldn't figure out what I had done wrong. He told me that the department had decided not to accept any women into the PhD program that year. Someone had conducted a study that showed that women “didn't have the stamina” to complete a PhD program. I felt contempt for Harvard and relief that the decision had nothing to do with me personally. A small group of us objected and eventually Harvard did admit one woman, but she wasn't me.

So I went to Stanford, where there were plenty of first-born women like me, and where there was little discrimination in the way we were treated intellectually. In my application, I said I was interested in nonverbal communication, psychology and law, and culture, but when I got there, I was told that none of these topics was really psychology. Every week I'd meet with my advisor, Merrill Carlsmith, and propose a topic for research, and every week he would tell me that the idea was dumb, juvenile, unworkable, or not psychology. First-year students were required to complete a study and write it up by the end of the year, and so by January I was getting pretty anxious and figured that I couldn't really wait for Merrill's approval – I'd just have to do a study on one of the rejected ideas so I'd have something to turn in. There was an assumption at that time that eye contact was universally good for establishing positive relationships, and the more eye contact you made with people the better they would feel. I thought that sometimes eye contact could be uncomfortable – for example if you'd just done something bad. So, I designed a study on the effects of eye contact on liking when a person was saying complimentary things or critical things to the listener. I found that speakers who were saying nice things were liked better when they made eye contact, but speakers who were critical were liked better when they looked away.

When I first got the data tabulated (in pencil, on large sheets of graph paper) and was sitting at my desk calculating means and variances

(by hand – we didn't even have calculators, let alone computers), Merrill stopped by and took a look. He loved data and was suddenly very interested, though he couldn't quite remember what the variables were. I said I didn't think the differences were very big, and he said "No, no – that'll be a t of at least 2. This is terrific!" And that was the beginning of an odd but fruitful collaboration, in which I planned and conducted studies while Merrill's emotions ranged from apathy to skepticism, and then as soon as there were numbers on a page he was galvanized into enthusiastic participation.

Still, none of the faculty was actually interested in my topics. Nonverbal communication was cute but not really theoretical, and as for psychology and law, that wasn't science at all – that was *applied* research and would be bad for my career. "We don't do applied research at Stanford," one said, clearly implying that that was for second-rate schools and second-rate minds. So, I looked around for other mentors. I had taken criminal law with a brilliant professor named Tony Amsterdam who was interested in the death penalty and people's attitudes toward capital punishment. I wasn't particularly interested in the death penalty, but Tony's intelligence was clearly so superior to mine – and everybody else's – that I would work on anything in order to have the opportunity to work with him, and I have continued that line of research ever since.

I also discovered Paul Ekman, who was at the Langley Porter Neuropsychiatric Institute at the University of California at San Francisco. Paul and Wally Friesen were studying facial expression and were willing to take me on as a research assistant during the summers. It was an especially exciting period in their lab, and I got to participate in designing and pretesting the famous study of emotion recognition in an isolated New Guinea culture, which provided evidence that emotions were more universal than people had thought. I also got to work on designing the first facial expression coding system based on pictures (rather than words). After I moved on, this system was replaced by a much more elaborate system (the Facial Affect Scoring System, or FACS), which has been the predominant method for coding facial expressions ever since.

In those days, most students from the top schools didn't apply for jobs – it was the Old Boys' Network. Professors would call up their friends in other top schools and ask whether they had any good students, or, if they had a good student, would call up their friends to ask whether they had a job. And it was rightly called the Old *Boys'* Network – it didn't occur to professors to make these calls if their successful student was female. It was assumed that women didn't want real jobs. One day Merrill told me that

he'd had a call from Bob Abelson at Yale, asking him if he had any good students, as Yale had interviewed several candidates and didn't like any of them much. "Oh, I have a real tiger," Merrill said, "but she wouldn't be interested." "Hey, Boss," I said, "where did that part about not being interested come from?" "You would?" he asked, and then he called Abelson back, and Yale invited me for an interview.

My job talk was very well attended, as a female job candidate was a curiosity, like Johnson's dog walking on its hind legs. Yale had Helen Block Lewis as a lecturer, and Ruth Day, whom they had hired from Stanford the year before as a tenure-track appointment in cognitive psychology, but that was it for women. I talked about my work on the meaning of the gaze in social interaction, since that's where I had the most consistent program of research, and it was the research that most reflected my own ideas.

They offered me the job. I expect that this was partly because Yale had become coed only the year before and the female students wanted more female professors, partly because I was such an interesting novelty, and partly because they were sick of interviewing people. Some other schools called me after they heard I was interviewing at Yale, but I liked Yale better. For one thing, all of the other schools assumed that I would teach a course on the psychology of women. Yale didn't treat me any differently from the other faculty, intellectually, assigning me to the same mainstream courses as other professors and letting me choose what else I wanted to teach and study, rather than assuming that because I was a woman I must want to focus on women's issues. They allowed me to teach an undergraduate course on psychology and law, which they had never offered before. This attitude made me feel like an equal, and I am eternally grateful to my colleagues for creating this atmosphere.

So, my career was largely good luck. I had parents who cared about education and I was familiar with academia, as my father worked at Yale; at Bryn Mawr, I learned that women were smart, that speaking in class was expected, and that it was right for women to have careers and make a contribution to society once they graduated. Harvard, Stanford, and Yale gave me the credentials to be a real player.

I graduated from high school in 1961 and got my PhD in 1970, so my academic training exactly matched the decade of the sixties. This was a period when students felt that we had the power to make lasting social changes, and that we had a duty to do so: "If you're not part of the solution, you're part of the problem." During the early sixties, the main issue for my friends and me was racism. There were stores and restaurants

and barber shops in Philadelphia that wouldn't serve Black customers, and we picketed and protested. We were members of CORE (the Congress of Racial Equality) or SNCC (the Student Nonviolent Coordinating Committee), trying to get more representation for African Americans, for example at the 1964 Democratic convention. Then came Vietnam and the draft, and protests became a regular part of university life. Stanford was not at the forefront of student activism, but Berkeley was, and many of us took time off for protests there. Soon enough, Stanford followed, and classes were cancelled in 1970 by protests against the bombing of Cambodia and the shootings at Kent State University and Jackson State University. This general sense that we had a duty to contribute to the solution of social problems probably strengthened my commitment to devote part of my research to working on social issues.

We spent surprisingly little time thinking about our careers or future jobs. Partly this was because we were busy changing history and righting the wrongs of previous generations, but partly because at that time if you were a graduate student at a good university, jobs were easy to come by. The Baby Boomers were reaching college age, and schools were hiring a lot of new faculty to keep up with the demand. Psychology and other social sciences were respected fields that were expanding. We didn't have to think about how many publications we had by the end of graduate school or how prestigious the journals were. If you made it through a top graduate school, you got a job.

At least that was true for the men. For the women, it was more complicated. It wasn't that the faculty thought we weren't smart. It was more an unexamined assumption that we weren't tough enough, that women wouldn't *want* a life that was intellectual and competitive, that we were too emotional and too socially oriented to thrive as full-time professionals. Several of the men on the faculty at Stanford had wives who were also PhDs, and some of them had jobs that were sort of like today's lecturer jobs – they did a lot of teaching and some of them did a little research, but they weren't full-fledged tenure-track faculty members and they weren't expected to have research careers. They raised their children, they didn't protest about discrimination, and the arrangement *seemed* to be what they wanted.

We female graduate students weren't entirely clear about what we wanted to end up doing, but we really wanted to show that we were tough, that we had the stamina to thrive in an academic environment. We wanted to prove that we were not controlled by our hormones or our emotions. We accepted whatever tasks we were given, however unreasonable; we did the extra-credit questions; we volunteered for difficult jobs; we

never whined, complained, asked for extra time, or made excuses when we were late – we never *were* late. Paradise was to accomplish a task that a man had refused or failed. No one would ever be able to point to a time when we had shown weakness.

This seemed like the essential strategy at the time, and it may have been. But in retrospect I see that there were probably costs. We spent a lot of time working on tasks that weren't particularly worthwhile and perfecting manuscripts that were already good enough. And today's women would say that there were many things that we *should* have been complaining about. When a woman had a good idea, it was often ignored until a man said the same thing; mansplaining was unnamed but pervasive; at political rallies, we rarely got microphone time. We grumbled about these things to each other, but rarely made a fuss.

Professors had few qualms about making sweeping criticisms of women, about telling demeaning jokes, or about making sexual advances to undergraduates, graduate students, or (when they existed) women junior faculty. We didn't complain. We didn't feel violated. It was just part of the normal background environment. Sometimes we even liked it. Few women graduate students had been popularity queens in high school or college, and it was flattering to have men buzzing around us treating us as attractive. Often it was just annoying. It didn't occur to any of us to complain to higher authorities unless it was some truly contemptible move, like making a high grade contingent on sex.

Complaining would suggest that we were weak, emotional creatures – *victims* – and we feared that it would instantly undo all the effort we had put into convincing others and ourselves that we were at least as tough and invulnerable as men. It would also mean that we would never reach our goal of being defined as scientists, but would instead forever be “the woman who was molested by Professor X and made a fuss about it.” At least this was true of me and my friends. There probably were women who did feel violated and who found the atmosphere intolerable. If there were, we didn't notice. Quite a few of our fellow graduate students – men as well as women – were dropped from the PhD program, and others dropped out voluntarily. Unless we knew them well, we often didn't know the reasons, and it's possible that for some women it might have been due to unwelcome sexual advances.

Times changed quickly. When I applied to graduate school, many women felt that getting a PhD was not appropriate for a woman, or at least meant giving up all ideas of a happy marriage and children. By the time I started my first job, feminism, which had been more or less dormant

since World War II, had revived, and it had its own set of definitions and demands for women. I mentioned earlier that many schools just assumed that I would teach a course on the psychology of women, and this was not just a matter of male stereotypes. Many women also thought that I should be studying women. That was hard for me. I would have liked to have something like a support group; but I mistrusted any group that required a uniformity of views.

In any case, I wasn't much interested in studying the psychology of women. For a while I continued my research on nonverbal communication, especially the gaze, and also my research on psychology and law. I had written a book with Paul Ekman on facial expression of emotion, and also, oddly enough, my research on attitudes toward capital punishment was pushing me toward emotion as a topic, as Lee Ross and I had done a study that suggested that people's reasons for supporting or opposing the death penalty were based more on emotion than on rational arguments. I felt that emotion was centrally important in influencing people's attitudes and behavior, and I also felt that current theories of emotion were unsatisfactory.

There were two dominant theories of emotion at the time, but little research. Schachter and Singer's constructivist theory (1962) argued that emotions arise when a person experiences an undifferentiated physiological arousal and looks to the immediate situation to figure out the meaning of that arousal. Basic emotion theory, repopularized by Paul Ekman (1971), argued that there are six basic categorically distinct emotions that are innate and culturally universal.

I agreed with elements of both theories, but felt that neither was really adequate. Like Ekman, I believed that some aspects of emotion were innate to the human species and culturally general. As a teenager, I had been powerfully moved by Edward Steichen's Museum of Modern Art exhibition *The family of man*, a celebration of the universal experiences and emotions of human beings everywhere. I could not accept the prevailing constructivist view that emotions were learned in a particular culture and that nothing much about them was universal. On the other hand, I deeply believed that people's emotions are fundamentally perceptions of their world, perceptions that motivate them to respond appropriately to its dangers and opportunities. Thus, I also agreed with Schachter. The problem with Schachter was that the theory said nothing about what *kinds* of situational perception aroused emotion in general or particular emotional responses like fear or anger or sorrow. Schachter's method was to come up, ad hoc, with a situation that would plausibly arouse an emotion

and see how people behaved. I wanted a theory that would specify the kinds of situational perceptions that corresponded to different emotions.

With the collaboration of a series of brilliant graduate students – Roger Tourangeau, Ira Roseman, and Craig Smith – I came up with appraisal theory (Smith & Ellsworth, 1985) and found six different situational appraisals that were important in differentiating emotions: novelty, valence, certainty, goal conduciveness, agency, and control. The idea of appraisal was in the air in the 1980s, and other psychologists came up with similar models, most notably Klaus Scherer. I remember a day in the early 1980s when Klaus gave a presentation to my lab at Stanford. We had been working on an experimental investigation of appraisals and emotions for some time, and we were absolutely flabbergasted when Klaus presented his component process model (1984) which was practically an identical twin of our appraisal theory.

At that moment, something important happened, which I am proud of to this day. Klaus and I could have been overcome by a sense of competition, worrying about who would get credit for the ideas, and trying to show that our model was the superior one. I really didn't want this to happen, and I said, "Klaus! This is amazing! If we both came up with the same ideas, there must be something right about them – we should work together to make this point of view known to the world!" Klaus agreed. We felt exhilarated, we felt that our ideas must be right, and we joined forces in promoting the appraisal perspective in general as an important new theory.

During this period, my work in psychology and law also took a new direction, one that allowed me to do basic research that had immediate applications to legal issues. The question was whether excluding all strong opponents of the death penalty from juries in capital cases resulted in juries that were unconstitutionally biased toward finding the defendant guilty, compared to the juries that tried all other kinds of crimes. That is, were attitudes toward punishment closely related to attitudes toward guilt? A group of lawyers in California, headed by Sam Gross and my old mentor Tony Amsterdam, had decided to make a test case of this issue and felt that they needed more empirical data. I had taught classes in my psychology and law course at Yale on this exact question and had even begun designing research on it; I was on sabbatical that year, so I could spend the fall semester at Stanford. The years 1978 and 1979 were spent working round the clock on this project.

Again, I was fortunate to have a group of excellent graduate students – Bob Fitzgerald, Bill Thompson, and Claudia Cowan, and we worked intensively with the lawyers designing a set of studies that were not only

methodologically adroit but also captured the legal issues in a way that would be acceptable to judges. We had a survey to determine the proportion of potential jurors who would be excluded and how they differed in their demographic profiles and their attitudes from the included (“death-qualified”) jurors. We had an experiment in which we showed eligible jurors a videotape of a homicide case and then divided them into juries that excluded people who were strongly opposed to the death penalty (the normal “death-qualified” juries) or included them. And we did smaller studies examining specific issues like perceptions of the credibility of defense and prosecution witnesses and perceptions of the insanity defense. We found that the jurors who were excluded because of their opposition to the death penalty were more likely to be Black, more likely to be female, and more likely to favor the defense. Including them on juries resulted in discussions that were more balanced and more thorough.

The lawyers and the experts worked closely together on the design of the research and on the preparation of testimony for an evidentiary hearing in a murder case in Oakland in August 1979. I worked most closely with Sam Gross. We worked day and night going over every measure and procedure in the research, working with the graduate students and law student assistants, attending strategy meetings with Tony and the other lawyers, preparing me for testimony, and figuring out how I could cover all the background and methodological information we needed to build a comprehensive record for the test case litigation. We argued, we persuaded each other, and the research and case preparation got better and better. In August, I testified for three days and then went back to Yale for the fall semester. Sam stayed on until the end of the hearing and then joined me in New Haven, since during our intense collaboration we’d developed a mutual admiration that quickly revealed itself as true love. We were married in November, and I was promoted to full professor that year, so I felt that the basic milestones of love and work had somehow miraculously been accomplished.

Throughout my career, I continued to work on both emotion and psychology and law, as entirely separate lines of research. Many advisors and colleagues advised me against this, saying that it was important for my career to be known for one thing. In fact, many people over the years *have* known me for one thing – either the emotions research or the law research. When I moved to Michigan in 1987, I had a joint appointment in the Law School and the Department of Psychology. My law colleagues thought of me as an expert on juries, my psychology colleagues as an expert on emotions. I actually found this somewhat liberating. Sometimes in one or the other line of research I would run into a problem I couldn’t solve or

a question I didn't know how to answer, and the more I thought about it the more frustrated and incompetent I felt. But this rarely happened in both areas at once. So, if I was stuck in my emotions research, I could work on a law project and vice versa, and could always feel that I was making progress. And, months later, I would come back to the original frustration with new ideas and could move forward.

Marriage and motherhood added a third tremendous interest to my already eclectic mind. Sasha was born in 1981 and Emma in 1986. People speculated about whether I would drop out, as there were hardly any role models of mothers in academia, but I didn't. Like switching between two different research topics, switching between the roles of scientist and mother was often a relief: After a horrible day at the office – a journal rejected my article, a student spent our meeting in tears, my ideas were totally ignored during a faculty meeting – I could pick up my daughters and be greeted with wild enthusiasm by people who knew that I was the most wonderful person on the planet. Likewise, after an evening of bickering and whining and general defiance, once the girls were in bed it was a pleasure to sit down at my desk and prepare a class about control groups.

I had a husband who thought it was great to be married to a professor and who loved working as an equal partner taking care of the girls. We had worked together for nine months before falling in love, and had come to respect and trust each other, and to feel that together we could handle any problem that came up. We were often tired, of course, and our cultural and social lives practically disappeared for a while, but our lives were rich with variety and always interesting. Who knew that the story would end “and they lived happily ever after”?

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