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Wandering into Psychology and Law

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Some people have a passion for a single topic that motivates and engages them for life. I'm not one of them. I can get interested in almost anything, and my career looks more like a random walk through a candy store than a single-minded pursuit of a goal. I am both a theorist and researcher in the field of emotion and a contributor to the application of psychology to legal issues. In this piece I will focus on my work in psychology and law. A review of my research on emotion can be found in Ellsworth and Scherer (2003).

I was the first-born child in a family that believed in education, and my father had a passion for teaching. He probably would have been happiest as a teacher in a high school or prep school, but instead, during my childhood, he was first a graduate student in sociology, then an assistant professor at Yale, where his passion for teaching so consumed him that he failed to publish enough to get tenure. He taught me to be interested in everything. He and I made scrapbooks of animal species, collected stamps and learned about their different countries, studied prehistoric hominids, and diagrammed football games. In the meantime, both parents gave me works of literature that were generally at the edge of my capacity – *Pride and Prejudice* when I was ten, *Moby Dick* when I was thirteen. That was what was fun: learning stuff.

My family assumed that their children would go to college. They also pretty much assumed that the girls wouldn't have jobs but would be well-educated, suitable wives to well-educated, successful men. So when I was asked what I wanted to be when I grew up, I would say an artist or a writer – something where you worked from home and didn't actually have a job. This was basically the world view for upper middle-class women in the 1950s and it didn't occur to me to question it.

I started college at Bryn Mawr in 1961. Bryn Mawr women were seen as high on intellectual skills but low on social skills, and that was true of me. At Bryn Mawr there was a tacit assumption that college was not the last step in becoming a well-educated interesting wife, but the first step toward making a contribution in the world. Its second president, M. Carey Thomas, famously said, "Our failures only marry." I don't remember ever having a moment of sudden insight when it occurred to me that I might have a career; it just gradually came to feel like a normal thing to

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do. I thought I'd major in anthropology, perhaps because it had long been a field where women had succeeded.

After my sophomore year, I transferred to Harvard, following an unsuitable man. The relationship didn't last long and neither did my major, as the Harvard anthropology department didn't think I was qualified. I was later told that it was a notoriously sexist department and would do anything it could to discourage women. So I floated along, replaying my childhood by always taking more courses than were required, but in such a wide range of topics that I didn't have enough to make up a major in any field.

I was not particularly interested in either law or psychology. I thought law was dull, and I was suspicious of psychologists. Like most undergraduates, I thought of psychology as clinical psychology. Several of my folk-singing, counterculture, would-be beatnik friends had been pushed into therapy by their parents, sometimes inpatient therapy, and I thought it was evil to equate nonconformity with mental illness.

Back home for the summer, I looked for a job, knocking on doors at Yale and asking whether anyone was looking for an undergraduate assistant. In the 1960s, funding for psychological research was easy to come by, but I was hired by the psychology department to work as "office help" for three social psychologists – Chuck Kiesler, Barry Collins, and Norman Miller. I had neither the skills nor the temperament to make a good secretary. When I proofread a manuscript, I was more likely to question the ideas than the spelling, so I was repurposed as a research assistant. I ran subjects, I attended research meetings, and soon I was helping to design studies and write grant proposals. They were working on cognitive dissonance and attitude change, and while I wasn't deeply interested in the topic, I was excited by the idea that you could answer questions by designing experiments. I loved the formal design part of it – figuring out what control groups and control measures were important, and I loved the procedure part – figuring out how to turn your concepts into events and measures that would be meaningful and involving for the participants.

Back at Harvard at the time, social psychology (along with sociology and cultural anthropology) was part of a department called social relations. They were extremely liberal about the courses they would accept as part of the major, allowing not only my course on physiological psychology, but also my course on *Beowulf*, so all I had to do was take a few more psychology courses and write a senior thesis. In my always passionate perusal of that garden of earthly delights – the new course catalogue – I came upon a course that combined psychology and law, taught by a visitor named Hans Toch. I probably took it because I had a law school boyfriend at the time. Although its approach was fairly clinical, I loved it, and it opened up a whole new world of questions I had never thought about. Most of all, I was shocked by the realization that legislators and judges regularly made decisions based on their understanding of human behavior without bothering to educate themselves about what was *known* about human behavior.

This first struck me when I discovered that in creating laws involving defendants who were mentally ill, judges didn't bother to learn anything about mental illness, but

devised a rule of their own that defined "legal insanity." This meant that even if psychiatrists and psychological researchers agreed that a person was seriously adrift from reality, he might be "legally sane" and could be sentenced to prison or death. The common legal definition of insanity, the M'Naghten rule, was established in the 1840s, and remained the standard despite over a century of scientific and medical research on mental illness. When I asked how this could be so, I was told about the idea of *precedent*, by which courts are bound by previous legal decisions. This struck me as a crazy requirement for any decisions that involved science.

In the fall of my senior year my advisor, Ken Gergen, suggested that I apply to graduate school. The idea hadn't occurred to me, but I'd always liked being a student and had recently come to like research, so I thought, why not? I thought I'd probably stay at Harvard, but it turned out that they had decided not to accept any women that year because someone on the faculty had done a study showing that women did not have the stamina to complete the PhD. So after graduating from Harvard in 1966, I went to Stanford. On my application I said that I was interested in nonverbal communication of emotion, cultural differences, and psychology and law. No one at Stanford was interested in any of these topics, but I had very high GRE scores and had graduated *summa cum laude*, and they assumed that if I was that smart, I would soon learn to recognize an acceptable topic.

I never did. I did experiments on nonverbal communication, and once I had actual data, the faculty seemed to think that was acceptable enough. I ran studies for my advisor, Merrill Carlsmith, and he let me run studies of my own. Psychology and law stayed in the background until my second year of graduate school, when I met Robert Levy, a lawyer who was at the Center for Advanced Study in the Behavioral Sciences. He was working on drafting a uniform code for child custody adjudication. This struck me as a topic that cried out for collaboration with developmental psychologists, and I asked him whether he was talking to any of them. He said no, but to his credit, he said he thought it would be a good idea. I told him I'd ask Eleanor Maccoby, who was in the Stanford psychology department. I bounced into her office, full of enthusiasm about this exciting opportunity to influence the law, but her response was one of disdain. "This is Stanford," she said. "We don't do *applied* research here." Later, in the 1990s, she became an important contributor to the literature on child custody.

So I offered to review the developmental research myself, and Levy and I ended up writing an article together in the *Law and Society Review* (Ellsworth & Levy, 1969). That was my first publication in psychology and law – way out of my field of expertise, but not bad anyway. The psychology department seemed to be pretty much unaware of this work, and I had pretty much given up on getting any guidance from the faculty. On the other hand, I was doing well in my other research, and neither Merrill nor anyone else did anything to dissuade me. I think this must have been partly because I was a woman, so it didn't matter what I did. Although the department was very good about accepting women graduate students (unlike Harvard), they didn't actually expect us to get jobs, so they didn't make any effort to make us marketable. They might have paid more attention if I were a man, and worked harder to persuade me that getting involved in psychology and law would hurt my career chances.

I took courses at the Law School. My professor in criminal law was Tony Amsterdam, and meeting him changed my life. Once or twice in your life – if you're lucky – you meet someone who seems to exist on a whole new higher level of intelligence. That was Tony. I knew immediately that I wanted to work with him and learn from him. He was working on developing test cases to challenge capital punishment. I neither knew much nor cared much about capital punishment, but that didn't matter. Some of the issues at the time, such as whether the death penalty deterred homicide, involved empirical evidence, and I offered to review these studies for him. He agreed, and I became an acolyte in the community of death penalty litigators and scholars and a Tony Amsterdam disciple.

One day in my fourth year of graduate school, Merrill mentioned to me that Bob Abelson had called him because Yale was looking to hire an assistant professor and he wanted to know if Merrill had any good students. That was how people got academic jobs at elite schools in those days – through the old boy network. Merrill told Abelson that he had a real tiger but she wouldn't be interested. "Hey boss," I said, "where did that part about not being interested come from?" It came, of course, from the general assumption that women would not want full-time jobs. Only one female Stanford social psychology PhD, Elaine Hatfield, had ever done so. To his credit, Merrill called Abelson back right away, I was invited to give a job talk, and was offered a job at Yale. I started there in 1971.

Yale was good to me. They treated me like any other assistant professor, never assigning me courses like "Psychology of Women" but giving me the same graduate and undergraduate classes as anyone else. I taught the graduate social psychology course my first year, and quite a few senior graduate students decided to take it, probably figuring that they might have to teach it very soon in their jobs and that they could save themselves a lot of work by using my course as a template. For me it was a classic case of Impostor Syndrome, as Reid Hastie, Shelley Taylor, Ellen Langer, and I think Carol Dweck all took it, and most knew at least as much as I did. Yale also let me teach a new course on psychology and law, which quickly became hugely popular, probably because the undergraduates thought (falsely) that it would help them get into Law School.

The Russell Sage Foundation had a Law and Society program with a branch at Yale, and there I met Neil Vidmar, and we wrote an article reviewing all the research on public opinion and the death penalty, and concluded that very little of it was relevant to the issues raised by *Furman v. Georgia* (1972), a highly controversial death penalty case. In *Furman*, the US Supreme Court invalidated all the death penalty laws in the country, but held that capital punishment might be constitutional if jury verdicts were guided by rules that limited the unguided discretion they had previously exercised. This meant that simply expressing support for the death penalty was uninformative, because there was no way to tell whether it signaled support for legal or illegal capital punishment.

In 1973, I was back at Stanford as a visiting professor. I'd become good friends with Lee Ross, who had joined the Stanford faculty when I was a senior graduate student. When I went to Yale, Lee took over my role as on-the-spot consultant to Tony

Amsterdam. By then Tony's group had decided that we weren't likely to make much progress in the abolition of capital punishment as long as it continued to have such strong public support (around 60 percent in 1973). So Lee and I decided to do a survey to find out the reasons that people supported or opposed the death penalty, figuring that knowing the reasons for the attitude would make it easier to identify strategies that might change it.

At that time, surveys mostly just asked whether people favored or opposed the death penalty. We asked which crimes they favored it for, whether they favored mandatory or discretionary capital punishment, how well-informed about the death penalty they were, and several other questions designed to assess their attitudes in the light of *Furman.* We listed every reason we had heard of for favoring or opposing the death penalty, and asked if their opinions were based on that reason. It was a wonderful survey, far more complete than any other that had been done, but our effort to find out why people favored or opposed the death penalty completely failed. People who favored the death penalty endorsed *every* reason for favoring it; people who opposed it endorsed every reason for opposing it. This led us to conclude that the attitude was more important to people than the reasons, that death penalty attitudes were symbolic, emotionally based elements of people's ideological self-image (Ellsworth & Ross, 1983).

In the fall of that year a much smaller, more personal incident arguably had a greater impact than our research on the field of psychology and law. A defense lawyer in San Jose called the Stanford psychology department asking for someone who could serve as an expert witness in a case that he thought involved eyewitness misidentification. The department referred him to me, since I was the psychology and law person. I had never heard of anything like this, but since I was such an advocate of basing legal decisions on psychological research when it was relevant, I agreed. Working from the literature on person perception, I presented data on how expectations influence perception and on stereotyping (the defendant was a Latina accused of shoplifting).

When I got back to Stanford after testifying, I ran into my old friend and Stanford classmate, Beth Loftus. I told her about my courtroom experience, and after a long pause, she said, "Phoebe, why did they ask *you*? You don't know anything about perception or memory." I said I supposed it was because of my interest in psychology and law. "Well, *I* really want to do that," she said. "How do I get them to ask *me*?" I told her that there was hardly any research on eyewitnesses, that I'd just cobbled together research from person perception in general, and that I'd love it if she did work that focused more specifically on sources of eyewitness error. The rest is history.

Along with eyewitness testimony, one of the first big topics in psychology and law at that time was juries. In the early 1970s the Supreme Court handed down two decisions on juries that galvanized the young field of psychology and law. In *Williams v. Florida* (1970) they held that six-person juries are constitutional, in part because they saw no difference in how deliberations would play out in groups of six versus twelve. Then in *Apodaca v. Oregon* (1972), they used similar reasoning to hold that

non-unanimous juries were fine – deliberations would be the same whether or not the jury was required to reach unanimous agreement. Research psychologists were shocked because these were empirical questions, and the Court had set binding legal precedents without considering any empirical evidence. On the other hand, there actually wasn't much empirical research on group size or unanimity, and a number of psychologists quickly began to study these questions. If judges had an obligation to consider empirical evidence relevant to legal questions, we had an obligation to provide it.

In 1968 the Court had decided *Witherspoon v. Illinois*, a case that involved both juries and the death penalty. In capital cases, people who said they opposed the death penalty were automatically disqualified as jurors for fear that they might be unwilling to sentence the defendant to death. Since the same jury decides both guilt and punishment, these jurors were also excluded from the jury that decides whether the defendant is guilty or innocent.

In Witherspoon's case, nearly half the prospective jurors were disqualified because of their attitudes about the death penalty. He argued that such a "death-qualified" jury was unconstitutionally biased toward a guilty verdict, compared to the juries that try all other crimes. His lawyers actually presented evidence from three unpublished studies that supported this claim. The Court concluded that this evidence was "too tentative and fragmentary" to reverse Witherspoon's conviction; however, in an unusual move, they also acknowledged that the question was an empirical one, and that their decision might be reversed if future research showed that death-qualified juries were "less than neutral with respect to *guilt*."

Of course the Court's suggestion that it might be influenced by future empirical research was catnip to me, and I regularly asked my psychology and law class to design research that might be persuasive. In 1978, my old hero, Tony Amsterdam, along with Sam Gross, a young lawyer who worked with the National Jury Project, and other abolitionist lawyers were gearing up to prepare a comprehensive test case on death qualification. Obviously a centerpiece of that case would be providing new research. They asked me to organize the research effort, and I agreed.

Several extremely smart and dedicated graduate students were committed to the project – Bob Fitzgerald from Berkeley, and Bill Thompson and Claudia Cowan from Stanford – and we worked intensively with Tony and Sam on designing research that would satisfy not only scientists, but judges. For example, it took us more than a month to come up with the crucial question that would identify the people who would be excluded as jurors. The lawyers thought my initial question was way too simple and conversational to match the legal definition; I thought that their legally correct question would be incomprehensible to survey respondents, so their answers would be worthless. But in this context, more than in any work I've done as an expert, the lawyers and the social scientists were enormously respectful and patient, listened carefully to each other, and never gave up until we were all satisfied.

We began with a survey to find out how many people would be excluded and how they differed in their demographics and their attitudes from the death-qualified jurors.

(This was done in the county where we planned to hold the evidentiary hearing, so the judge could not dismiss it as irrelevant to that jurisdiction.) The other really important study was an experiment in which we showed eligible jurors a videotape of a homicide case and then divided them into juries that excluded strong opponents of the death penalty (the usual death-qualified juries) or included them and looked at their tendencies to vote for guilt and at the quality of their deliberations. And we did smaller studies on specific issues like perceptions of the credibility of defense and prosecution witnesses and perceptions of the insanity defense. We found that the excluded jurors were more likely to be Black, more likely to be female, and more likely to favor the defense. Including them on juries produced discussions that were more balanced and more thorough. Craig Haney joined us and did a study that showed that the very process of being grilled about their attitudes toward the death penalty made people think the defendant was probably guilty.

In August 1979, the evidence was presented at an evidentiary hearing on death qualification in Oakland. It was a very big deal for all of us. It went on for over three weeks and almost everyone who'd ever done any research on the topic testified as an expert witness. The idea was to create a complete record of the evidence which could then be used in future appellate cases all the way up to the US Supreme Court. In addition to presenting my own research, my job was to provide a mini course on research methodology and statistics, so that future judges would have the information they needed to evaluate the research and the arguments. Having recently co-authored a textbook on research methods in social psychology (Carlsmith, Ellsworth, & Aronson, 1976), I had plausible credentials. Sam had the task of preparing me. We spent weeks planning how to fit all the background material in, arguing over whether to include information that was scientifically essential (I thought) but legally not persuasive (he thought), creating graphs and other materials in big notebooks for the judge and opposing counsel, practicing my testimony over and over again, and buying impressive outfits to wear in court.

When I took the stand, the judge said, "You're much too young and pretty to be a doctor." I was not about to assert women's rights – I took it as a sign that he might let me go on and on about research methods and data analysis, and he did. Most people in the courtroom seemed pretty bored – I could tell because at that time the sports pages of the *San Francisco Chronicle* were green, and during that part of my testimony I felt as though I were in a bowl of lettuce. But the important thing was to get it into the record, not to amuse the present audience.

The lawyers on the other side had no idea that this hearing was going to be such a big deal, so they had to scramble at the last minute to dig up experts to refute our evidence. We were worried that experts sometimes relax their standards of accuracy when they get into a courtroom and say things they would never say if they were being judged by other scientists. So, to keep them honest, I asked Merrill Carlsmith and Lee Ross to come up from Stanford and sit in on the hearing during their testimony. Merrill and Lee were well known and respected in the field, and I hoped their presence would raise the experts' standards of scientific accuracy. So, on the day that they took the stand, Merrill and Lee introduced themselves and said that they were interested in

observing the presentation of scientific evidence in the courtroom. It worked. The criticisms were mild, and mixed with praise.

As I said, Sam and I spent hundreds of hours working together as a team, and we both felt that we had never worked with anyone so competent and so rewarding. One day I said I wanted to spend a few hours not talking about the case or the research – and that's all it took for us to realize that we were in love. Then we went back to working on the case, which took so much time and effort that any sort of prolonged courtship or preparation for a life-long relationship was out of the question. As soon as the hearing was over, Sam moved in with me at Yale.

The issue of death qualification slowly moved through the lower courts. We kept working on them, though of course I was no longer available for expert testimony, as marrying Sam had reduced my credibility to zero. In the meantime we expanded our collaboration to include our whole life. In 1981 our first child, Sasha, was born, and we took jobs at Stanford. My colleagues and I continued to research and publish data indicating the biasing effects of death qualification (e.g., Cowan et al., 1984; Thompson et al., 1984).

In 1986, the issue of death qualification finally reached the US Supreme Court in the case of *Lockhart v. McCree.* Sam argued the case, and I worked on an *amicus* brief for the APA. We lost. Rehnquist, ignoring the concept of convergent validity, managed to find a flaw in fourteen of the fifteen studies we presented, dropped them from consideration, and concluded that the single remaining study was not sufficient basis for a constitutional decision. And just for good measure, he held that future empirical evidence that death-qualified juries were biased was irrelevant: As long as the twelve people on the defendant's particular jury were impartial, it didn't matter what groups in the population were left out.

That was disappointing, although not unexpected, given the Court's support for the death penalty. In the meantime, I was pregnant again, though I didn't notice for the first couple of months because we were so busy preparing the case. Emma was born in August of 1986, and a year later we moved to the University of Michigan, where we've been ever since. Our collaboration, in work as well as life, has remained as intense and rewarding as ever.

Suggested Reading

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