47 Always Buy the *Handbook of Social Psychology* (1968) at a Railway Station in India

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Prologue

If social psychology in the twentieth century has revealed anything stunning about human nature it is this: that individuals are created and shaped by material and social forces more than they or their observers recognize. I see my life as a textbook case of the responsiveness of bystanders who eased the path for my growth. I wrote words to this effect in the mid-1990s in an application to the John Simon Guggenheim Foundation. Notwithstanding the influence of those individuals who shaped my development (their influence even more burnished by the added twenty-five years), I neglected to mention the influences that are the hidden levers afforded by collectives – communities, institutions, governments – to regulate, both up and down, life's opportunities and outcomes. I try to rectify that lapse here.

In *Requiem for a Nun*, William Faulkner (1951) says, "The past is never dead. In fact it's not even past" (p. 73). Personality psychology (not the individual difference kind but the kind that grapples with self, consciousness, goals, motives, values) handed me a manual to make sense of the past as it has corporealized into the present. I attend to these two truths in homage to what I have learned from my fortuitous apprenticeship in these sciences.

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When Drew Faust was named Harvard University's 28th and first female president in 2007, 371 years after it was established, a small group of her friends had a dinner for her. She asked me how I came to be an academic and I had to tell the story of wanting to be a secretary but being lovingly tricked into going to college. To our collective surprise, we discovered that the other four women (besides Drew), each had roughly the same story to tell. A well-known novelist reported having in fact been a secretary for a few years; an eminent historian noted that in her high school yearbook, she had in fact written that she aspired to be an "executive secretary," the "executive" she added had been emphasized because she knew she was better than the other girls. After joking that President Faust could count on a steady secretarial pool, it became clear that this discovery held deeper meaning. First, although varied in age and cultures, our

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similar choice of that single common path to economic freedom showed just how limited our options as middle-class women had been. But the good fortune of being where I had landed up, doing work I loved enough to do it for a nickel, made me intensely aware of all the potential, of many more amazing minds than my own, that are lost to us because they didn't experience the right intervention.

Having been "a sick child" born in 1950s India and who had largely been home schooled, I had no interest in attending college, which I saw as an extension of my intellectually dissatisfying years in high school. My mother had not attended college and although she did not speak about that loss to us, she was adamant that I go. Knowing that I had a will to match hers, she used a routine right out of social psychology's playbook: "Mahzarin, you just have to attend for a semester, to get your shy sister settled in. Then you may go off to your wonderful job." I grudgingly agreed to fulfill a filial duty for one semester, after which I would, by contract, be free to pursue the patently more daring life of a secretarial assistant.

I selected Nizam College in Hyderabad (the twin city from where we lived) not because it was the school my grandfather and father had attended nor from the recognition that my sister and I would be the first women to attend. Rather, its lure was that it was located next door to the largest cricket stadium in the city, and it was coeducational (unlike my high school). Even from the first few weeks, it was clear that college was not to be a hardship to be endured for twelve weeks before beginning a long career of snapping my secretarial heels to a boss's call for coffee. The end of the first semester came and went. Mother did not bring it up. I didn't have to admit that I had changed my mind.

It is rare that a course of action presents itself with such clarity that there is nothing to do but to follow it. While in a Master's program, I was traveling home from New Delhi to Hyderabad. At a major railway juncture, I stepped off the train to visit a bookstore on the platform where I bought a set of books that changed the course of my life. Five volumes of the *Handbook of Social Psychology* (1968) edited by Lindzey and Aronson, were being offered for the equivalent of a dollar a piece.

The printing of the *Handbook of Social Psychology* by Addison-Wesley that I bought was an independent Indian imprint. I still have the set (the binding is in red not blue, the paper is thinner and yellower, the gold lettering of lower quality). While I was the one who bought those books, no such possibility would have presented itself if the governments and publishing houses of two countries had not worked to make the handbooks economically viable in a country where academic psychology did not in any real sense even exist.

I bought the *Handbooks* out of mild interest in their content, but mostly because it seemed like a lot of book for the money. By the time I reached home twenty-four hours later, I had polished off a volume and knew with blunt clarity that this form of science was what I wanted to do. What attracted me was the combination of a focus on social processes but with an experimental approach, and this blend had a power and an appeal that I had missed in my previous encounters with psychophysics on the one hand and sociology on the other. I returned to finish the year but focused on applying to American universities. A few years later, I took great pleasure in

showing Elliot Aronson the actual volumes when he visited Ohio State, where I was now a graduate student. So moved was he as he examined the imprint – he had no idea such a set even existed – that he claimed me as his student. I was delighted to accept. It would connect me symbolically to the greats whose names, like Festinger, had acquired a god-like quality while I was struggling to figure out how to get myself to graduate school.

Knowing nothing about American universities or the process, I did ask a few male engineering students in India who seemed to know exactly where they were heading, about it. Perplexed that a field called experimental psychology even existed, one of them passed on a secret to me. He had heard that American schools with the word "state" in the name "pretty much take anybody." That was all I needed to get to work. I wish I could say that I selected Ohio State over other schools because I was aware of their program in experimental social psychology. I wish I could say that I knew I wanted to go to the program that had graduated Claude Steele, Rich Petty, John Cacioppo, Gary Wells, etc. But alas, I chose Ohio State because in the letter of acceptance Tom Ostrom had included a copy of that week's college newspaper on which he had scrawled a handwritten note "I hope you will come. Tom." That pretty much sewed it up. Little wonder then that I yawn when I encounter the hyper-planning and admission carnivals that I participate in today.

An international fellowship from the American Association of University Women (AAUW) made it possible to attend graduate school in the United States, because although Ohio State had admitted me, there was so little they knew about me, and my training was so far from what was needed (I had never had a course in social psychology). AAUW covered my first year's living expenses. Why did AAUW dedicate funds each year to supporting two women from outside the United States for graduate study? In any analysis of how institutions of good will can up-regulate life's opportunities, AAUW is a front runner in my book.

It was in September 1980 that I arrived in Columbus with $80 in my pocket and no paycheck until month's end. The elderly couple (Ohio State alumni) who were there to pick me up at the airport held a sign with my name on it, but so mangled was its spelling that they and I continued to exchange smiles until nobody was left around us. I had one suitcase containing my life's belongings, mostly filled with the five volumes of the *Handbook of Social Psychology* (1968) but also one long-sleeved cotton shirt, because I had heard that it could get cold in Ohio. I cannot say that the transition to the academic environment of the United States was easy where I felt reward structures were geared toward doing things fast rather than well. As it turned out, these pains of adjustment were superficial because in spite of what today would be considered a shockingly brutal environment, I was intellectually quite happily sliding down the rabbit hole of grad school.

What made graduate school endurable were deep friendships I formed: Michael Lynn, a Texan with social and political views that were hardly "academic mainstream" and far from my own became an immediate friend and confidant and we remain close today (listen up, country!); Trish Devine, my officemate of four years, was a force of nature and outrageously fun (when I had extensive work to complete on

a second-year project with a looming deadline, she persuaded me that we should first paint our office, located under the bleachers of the football stadium); Sharon Shavitt, a talented attitude theorist and on the ball in every way, could rattle off Cheese Shop and Dead Parrot sketches on cue, and she made me laugh a lot.

At the time, I was unaware of just how excellent a graduate program I had randomly landed into. Quite fortunately for me, a new approach linking the study of cognition to the study of social behavior was being formulated even as I arrived, and my advisor, Tony Greenwald, was a key player in this movement. The counter-intuitive idea underlying this approach was the notion that by studying the cognitive processes of perception, memory, and judgment, we could robustly understand representations of individual and group social behavior. It was the next step after attribution theory. This was not Leon Festinger's social psychology, and although it made for less good storytelling, it was far more to my taste of engaging more directly with tractable processes yielding reliable and robust effects.

Completing the PhD in 1986, I felt I was ready to move on to a job, but no place with a job seemed to share that opinion. I did have an offer for a one-year position from a small teaching school near beautiful Lake Seneca in upstate New York, and I would have happily taken the position and tried my luck again if not for a chance encounter with Tom Ostrom: "If you are genuinely attracted to such a job, by all means take it; but don't take it because you feel you won't be able to pursue a career in research," he said. Chance encounters like these seem so ordinary when they happen that it keeps their significance from being acknowledged. Tom's intervention was pivotal (at the level of reading of the *Handbook*).

Tom was not the only influence on this decision. My spouse, R. Bhaskar, lived in Westchester County in New York and although the job would be six hours away, it would have put us in the same state. The night before I had to make a decision regarding that idyllic college, Bhaskar spoke seriously: What if we are not together twenty years from now (we had been married for about a year) what then? How will you feel then about this choice of a one-year teaching position over taking a postdoc? What an amazing gift that long view was, shaken as I was by the suggestion of possibly not being together. But I knew immediately that I should turn down the position at that lovely college. (I should add, given the curiosity I've aroused, that Bhaskar and I have been together now for forty-one years.)

At the time, postdocs in social psychology were not considered the plum jobs they are today. You did a postdoc because you had sadly failed to get a job straight out of graduate school. But that's what I did, and of three postdoc options I had, I couldn't have chosen better. The University of Washington in Seattle was as far away as I could have traveled from New York, but the combination of collaborators in social and cognitive psychology transformed my thinking yet again and I developed a new confidence. Elizabeth Loftus, who has done pioneering work on the malleability of memory, opened her heart and her laboratory to me. Earl (Buz) Hunt reminded me that I had always been interested in the relationship between language and thought. Rumor had it that Art Lumsdaine (who had worked with Carl Hovland on experiments in

mass communication during World War II) had been on Nixon's enemies list and that made him enough of a hero that I assisted with his surveys of attitudes toward nuclear disarmament.

Claude Steele was my primary mentor, and the grant that supported me was from the NIAAA and we did studies on the effects of alcohol on self-concept. After a hypermasculine and interpersonally tough environment at Ohio State, Claude provided a different model of training – that good training need not come at the expense of damage to self-worth. I would have been content to remain there, but a happy surprise came in the form of an offer of an assistant professorship from Yale University, allowing me to simultaneously be at one of the great centers of psychology and to live with my spouse of three years.

I call this offer from Yale a happy surprise because I came close to never receiving it. I had not applied for it, believing that I was not worthy of a job at such a place. But Bhaskar, had, in effect, mailed my CV to Yale (I was vaguely aware that he had, but considered it to be sufficiently a joke that I did not ask for letters of recommendation to be sent there). Yale had interviewed seven candidates before me and decided they would likely close the search for the year but decided to bring one more candidate out (me) if my unsent letters were found to be supportive. Letters were rushed, I got the job. I've encountered similar hesitancy in candidates today who do not apply for jobs they may be competitive for. I always tell the Yale story and add that in the twenty-first century women ought not to rely on their feminist husbands to mail in their CVs!

The benign neglect of junior faculty was often regarded as an unpleasant aspect of life at Yale, but for me it turned into much-needed freedom to select problems and methods without the burden of worrying about the fluctuating opinions of senior colleagues or about tenure, for Yale did not hire assistant professors into a tenure track. In 1988, an idea for an experiment on unconscious discrimination came from an unlikely source. In a weekly brown bag on memory research organized by Robert Crowder, an idea emerged that led to experiments that put me on the research path I've been on, in one form or another, ever since then.

An experiment by Jacoby and colleagues (Jacoby et al., 1989) on implicit memory used first and last names to show that familiarized names would later be mistakenly identified as famous because subjects would mistake perceptual fluency for the attribute of fame. I replicated that experiment but with carefully matched names of women added to the set of male names that Jacoby had used. The assumption was that all names should produce the false fame effect. My attempt to add female names had been motivated by a desire to simply make the stimuli more representative. But counter to expectation, the attribution of fame did not accrue to female names in the way they accrued to male names. These experiments rekindled a collaboration with my mentor from graduate school, Tony Greenwald (Banaji & Greenwald, 1995), which turned into a collaboration that then lasted for the next thirty years.

In a symposium at APS on unconscious cognition I described what was the crux of those experiments in the early 1990s:

It is remarkable that quizzing over 400 participants across six experiments about what may have caused errors in their memory elicited many hypotheses, but never, not once, the possibility that the gender of the name may have played an influencing role ... Indeed, they seemed downright surprised by the suggestion. If this is true, harm can occur without the perpetrator being aware of harming and without the target becoming aware that she was harmed. If such is the power of unconscious cognition, if the source of influence on our thoughts and actions so deeply eludes us, results like these must call into question existing notions of equal treatment, individual responsibility and social justice.

I was surprised at the blatant disparity between explicit and implicit beliefs. Subjects had no idea that whatever their values may have been, the knowledge of the world they had acquired – the thumbprint of culture on their minds – had determined their behavior, rendering it to be in opposition to their own values. That these small results from laboratory experiments were speaking to the illusiveness of a just society may seem surprising. But not if you had read the *Handbook of Social Psychology* (1968) on a long train ride in India.

Of course, something called a research program had only just begun. I was surprised when in 1988 a graduate student arrived who said that he wanted to work with me, rather than all the luminaries at Yale. That was the iconoclastic Curtis Hardin. Along with him and Alex Rothman I conducted studies to show that what were assumed to be universal effects in person perception were in fact moderated by social category. We first used the term "implicit" in the title of a paper on social cognition (Banaji, Hardin, & Rothman, 1993). Later, Greenwald and I wrote a chapter for the Ontario Symposium (Banaji & Greenwald, 1994), followed by a *Psych Review* paper in which we laid out the concept of implicit social cognition and used the term "implicit bias" for the first time in our own work (Greenwald & Banaji, 1995). Toward the end of this paper, we mentioned that the field awaits a method that would allow implicit social cognition to be effectively tracked. That method turned out to be the IAT and it changed both basic research (and far more) for us and many others. It also led me to request that both universities I've been at should support an educational project that was created in collaboration with Tony Greenwald and Brian Nosek where any person with Internet access could measure their own implicit attitudes and stereotypes. That both Yale and Harvard invested substantially in this project is yet another indication of the power of institutions to step in to respond to a research need that is also a societal need. Each year over a million completed tests are collected; after the murder of George Floyd, that number more than doubled.

The singular feature of my research career and the one from which I derive the deepest gratification is the group of twenty-four PhDs and several postdocs who developed their own first research preferences and styles in my lab, and who have made formidable contributions to this and their own research programs as well as contributions well beyond academic ones. It has also been my good fortune to work with people in areas some distance from me and with whom the work on implicit social cognition advanced faster and farther. The neuroimaging work I did with Elizabeth Phelps (Phelps et al., 2000) seemed to put a stop to an odd question we

were constantly being asked at the time the IAT first emerged: Why is what you are measuring an attitude? (cf. Banaji, 2001; Banaji & Heifetz, 2010).

In January 2002 I moved to Harvard, which provided me with colleagues whose work I found fascinating, especially the work on cognitive development by Liz Spelke and Susan Carey. I had thought woefully little about where implicit bias comes from, and we did studies to understand its origins by studying the minds of toddlers. My own first foray (Baron & Banaji, 2006) required us to first build a child-friendly IAT. When the results repeatedly showed that the youngest children and adults showed similar levels of bias, I had to change my mind about the nature of implicit cognition.

Being in the Boston area with its sixty-plus colleges and universities also allowed the research to reach into professional schools and my collaborations with legal scholars (Kang & Banaji, 2006), business school scientists (Banaji et al., 2003), and medical colleagues (Green et al., 2007) allowed the work to be tested in settings I could not have imagined when we first began work that I thought would teach us about the basics of implicit cognition.

It is not surprising that today, the data sets we work with are massive. Data from the public website featuring demonstration IATs (created in 1998 at Yale and now residing at Harvard: implicit.harvard.edu) continue to produce an abundance of data from volunteer participants and are available to any research scientist wishing to further analyze them. These data are unlike any other on social group attitudes as they have been collected continuously with analyzable data since 2007, and today they allow time-series analyses of attitude change. These data have produced unexpected evidence that some implicit biases are changing toward neutrality (Charlesworth & Banaji, 2019), suggesting malleability that can be detected over long time periods. Another quite different approach, one using word embeddings utilizing massive language corpora allows us to measure the presence of social group attitudes and stereotypes that are hidden in plain sight in our language (Charlesworth et al., 2021).

To think that all this began with being mesmerized by reading the 1968 *Handbook of Social Psychology* in a place far from where I ended up is unlikely and therefore surprising, but obviously possible. The creators of that possibility, I hope it is clear, are embedded in the social and political/economic networks consisting of individual others, communities, and institutions – the regulators of life.

Suggested Reading

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