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A Clinical Psychologist  
Who Studies Alcohol

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**Abstract**

In this article, I describe why I believe the study of alcohol use and its consequences is a rich and rewarding area of scholarly activity that touches on multiple disciplines in the life sciences, the behavioral sciences, and the humanities. I then detail the circuitous path I took to become an alcohol researcher and the various challenges I encountered when starting up my research program at the University of Missouri. A major theme of my journey has been my good fortune encountering generous, brilliant scholars who took an interest in me and my career and who helped guide and assist me over the course of my career. I also highlight selected, other professional activities I've been involved in, focusing on editorial work, quality assurance, and governance of professional societies. While the focus is on my training and work as a psychologist, the overarching theme is the interpersonal context that nurtures careers.

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## INTRODUCTION

In this article I hope to accomplish three things: (a) give a sense of why clinical psychologists (and scholars more broadly) should consider being familiar with research on alcohol, the focus of most of my research career; (b) provide a glimpse into my own entry into this world of study and those who guided me in my journey; and (c) note some professional issues that arose in the context of my career that proved challenging. I have opted not to focus on the specific nature of my research contributions since these are already in the printed record and are easy to find (<https://scholar.google.com/citations?user=O51bc7QAAAAJ&hl=en&oi=ao>). I will briefly note that most of my alcohol work has focused on the following issues: (a) individual differences in reinforcing effects of alcohol (e.g., Sher & Levenson 1982, Sher & Walitzer 1986), (b) context and alcohol effects (e.g., Sher 1985), (c) intergenerational transmission of alcohol use disorders (AUDs) (e.g., Sher 1991; Sher et al. 1991, 1997), (d) developmental aspects of AUDs (e.g., Sher & Gotham 1999, Sher et al. 2004), (e) personality and AUDs (e.g., Littlefield et al. 2009, Sher et al. 2000, Winograd et al. 2017), (f) comorbidity (e.g., Helle et al. 2021, Jackson et al. 2005, Kushner et al. 1990), (g) college student drinking (e.g., Park et al. 2009, Sher & Rutledge 2007), (h) high-intensity drinking (e.g., Jackson et al. 2001, Rutledge et al. 2008), (i) diagnosis of AUDs and other conditions (e.g., Lane & Sher 2015, Steinley et al. 2016, Vandiver & Sher 1991), and (j) longitudinal research methodology (e.g., Sher et al. 2004, 2011). Virtually all of my work has been highly collaborative with trainees and colleagues with whom I've been privileged to work.

## WHY STUDY ALCOHOL?

Sir William Osler (1897) said, “Know syphilis in all its manifestations and relations, and all other things clinical will be added unto you.” Syphilis was a highly prevalent, serious disease during Osler’s lifetime whose manifestations were many and varied. As Harrington (2019, p. 436) wrote, “In the nineteenth century, neurosyphilis was one of the most ubiquitous and fatal forms of degenerative mental illness known to psychiatry. . .[and] as many as one-third of patients in mental hospitals had symptoms. . .traced. . .to syphilis.” Additionally, there was a strong behavioral component to the disease, whose transmission was based on intimate human contact and stigmatized like other sexually transmitted infections (Hood & Friedman 2011).

The more I studied alcohol, the more I became convinced that a similar, if not bolder, statement could be made about alcohol consumption and AUDs. The determinants of both consumption and its complications are wide-ranging, spanning genetics to culture. Like syphilis, alcohol has effects on multiple organ systems (especially gastrointestinal, cardiovascular, and neurologic) but also on social behaviors, including crime and violence, sexuality, and public safety. Evolutionary theories (e.g., Dudley 2004, Slingerland 2021) highlight the role of alcohol in the survival of some of our human forebears and, indeed, the shaping of civilization. Alcohol has played an important role in various cultures throughout recorded history (e.g., Hanson 2013, Vallee 1998). Virtually all religions have stances about the use and misuse of alcohol, which likely contribute to global differences in rates of alcohol use (Manthey et al. 2019). Additionally, alcohol has figured prominently in the arts—for instance, in the works of impressionist and early abstract painters (e.g., Edgar Degas, Édouard Manet, Edvard Munch, and Pablo Picasso all produced paintings of absinthe drinkers) and in film (see, e.g., Cornes 2006). Many of the leading American authors of the twentieth century (e.g., William Faulkner, F. Scott Fitzgerald, Ernest Hemingway, Dorothy Parker, John Steinbeck, Tennessee Williams) suffered from AUDs (Goodwin 1988, Laing 2014). One could construct an undergraduate curriculum with courses spread across the humanities, social sciences, and life sciences based upon alcohol: its manufacture, its economic impact, why people (and some animals) drink it, its effects on the drinker and those they encounter, and how artists and writers have come to describe both acute intoxication and its long-term consequences. Someone might say today, “Know *syphilis alcohol* in all its manifestations and relations, and all things clinical *and then some* will be added unto you.” I feel enriched by studying alcohol because it’s been a doorway to a number of different areas into which I might have never ventured.

## ON BECOMING AN ALCOHOL RESEARCHER

### Undergraduate Days: Prequel

My high school chemistry teacher told me that if I didn’t change my attitude I’d never get through college. I proved him wrong by going to a college that was difficult to get thrown out of. Antioch College, which I attended from 1970 to 1974 (BA, 1975), used a credit/no credit grading system. That is, as far as I know, you couldn’t flunk out. I look back at my time there as being in a socially stimulating but protected environment, a metaphorical holding pen, keeping me afloat until I could gain academic footing. My first quarter I took four classes: two on Buddhism (taught by a visiting Nichiren Buddhist bishop), a contemporary culture and technology course (taught by a technophile faculty member with a love of multimedia presentations), and calculus (which was unfortunately an early morning class, and thus I didn’t often attend). I subsequently did take some “traditional classes” in psychology, as well as in history and philosophy.

**Work-study.** Notably, Antioch was a work-study program. As such, twice a year I’d pack up my duffel bag and head out of Yellow Springs, Ohio, to work in a co-op job for 3 or 6 months. My very first co-op job was supposed to be as an orderly in a public hospital in Boston. Unfortunately, in the winter of 1971, Boston was going through a fiscal crisis and the placement disappeared the day after my arrival. I spent the next 3 months exploring and visiting friends in the region, especially my friend Ricky Reibstein, a freshman at the newly opened Hampshire College.

Neither of the next two co-ops fell through. One was in radio (KRAB-FM in Seattle; <https://www.krabarchive.com>), where I was a production assistant and sometimes announcer. The other was in television (KQED-TV in San Francisco), where I worked as a still photographer for what was then a nightly news program, *Newsroom* (<https://www.kqed.org/pressroom/public-television/kqed-newsroom>). Both positions reflected my early interests in broadcast media. My

time at KQED-TV led me to conclude that I was ill-suited for nightly news or similar endeavors; the pressure to meet a broadcast deadline each night, followed by starting over again from scratch the next day, was too Sisyphean for my tastes. I wanted a position where I could take my time and create something lasting.

By my third year I had developed a strong interest in psychology. I headed back to San Francisco to work as a research assistant for Mardi Horowitz, a psychiatrist at University of California, San Francisco (UCSF), who was broadly knowledgeable in psychological science. Despite being a physician and card-carrying psychoanalyst, he was publishing frequently in our best psychology journals (e.g., Horowitz & Becker 1971, Horowitz et al. 1973). With Mardi I undertook literature reviewing, data analysis, content analysis of narrative data, and copyediting of his papers and book drafts under the supervision of him and his associate, Nancy Wilner. Nancy was a mother figure to me and made what would have otherwise been an intimidating setting comfortable. If Nancy was a mother figure, Cathy Schaeffer, another Antioch work-study placement working with Mardi, was my big sister. She provided both camaraderie and technical guidance. I was grateful when she chose to pursue her graduate work in public health in Berkeley. Since she was still in the area, I could see her from time to time and she could still be my big sister.

After 3 months with Mardi at UCSF, I went back to Yellow Springs for my next academic quarter. When I arrived, the situation was unsettling. The campus was tense and on the verge of a student strike. I opted to become one of the approximately “one-third of the college’s 1,100 on-campus students. . . [who] left for ‘extended vacations’” (New York Times 1973, p. 54). I called Mardi and asked if I could come back for the next several months, and he agreed. I resettled in San Francisco through August. During that time, I dove deeper into various projects assigned by Mardi.

**University of Sussex.** Despite the work with Mardi, I would never have been in a position to be competitive for a good graduate program (or succeed in one if I’d been accepted) had I not spent my fourth undergraduate year abroad (1973–1974) at the University of Sussex. Sussex employed a tutorial system, and the standard load was two tutorials per trimester. Tutorial groups were small (two to four people), and the goal was weekly deep dives into the material. The required reading and writing for each weekly essay were a lot of work. Fortunately, I had sufficient time to do a good job on them. I’ve since reflected on the common American practice of five or more classes per semester being a barrier to a deeper understanding of individual topics. This American norm also implicitly promotes the value that more is better rather than, well, “deeper” is better. Even though I was only a visiting student, my personal tutor (advisor), Bob Boakes, was extremely supportive of my efforts.

For each tutorial, I’d defend my essay head-to-head against the tutor, in each case an imposing scholar. For example, in Maggie Boden’s tutorial (philosophy of mind), I’d find myself trembling as I anticipated her probing and pointed critiques of my thinking. Knowing what I was going up against—not just getting a written critique but an oral one in real time—was highly motivating. But she also reached out to me personally, even inviting me to a cocktail party at her house and introducing me to her colleague, a practicing psychoanalyst. It made me believe I could find a comfortable room in the Ivory Tower. While I took tutorials with only a handful of faculty members, the extended intellectual environment was exceptional with a number of eminent psychologists (e.g., Hans Furth, Phil Johnson-Laird, Nick Mackintosh, Stuart Sutherland) on staff. Sussex had been founded only a dozen years earlier, and this distinguished faculty had been assembled by Sutherland, the preeminent scholar in the area of discrimination learning (e.g., Sutherland & Mackintosh 1971). However, to the wider world he was best known for his personal account of struggling with bipolar disorder, *Breakdown* (Sutherland 1976). His breakdown was the reason I didn’t see much of him during my year there, although I heard quite a bit about him.

**Yellow Springs and San Francisco, again.** I returned to Antioch in the summer of 1974 to complete my remaining requirements. I was a very different student than when I had left, now possessing much greater confidence and a love of intellectual engagement. Upon finishing my final requirement, an independent research project, I went back to work with Mardi in the fall. He provided me with continued opportunities for intellectual growth through the various projects I worked on, both standard (e.g., analyzing data on the Impact of Event Scale, published after I left) and highly exploratory (e.g., relating real-time assessments of defense mechanisms and electroencephalographic patterns in collaboration with Enoch Callaway). He also included me in high-level research meetings with residents and senior UCSF faculty, something I found both validating yet extremely anxiety-provoking.

Throughout my work with him, Mardi encouraged me to attend psychiatry grand rounds, a number of which I still remember: Paul Ekman (facial expression), Henry Lennard (tardive dyskinesia), Jerome Motto (suicide prevention), and Joseph Wheelwright (Jungian psychoanalysis). In 2016, I was invited back to UCSF to deliver my own grand rounds. I was deeply moved speaking from the same lectern that I'd seen luminaries speak from 40+ years earlier. And Mardi, now in his eighties, was still on faculty and in the audience. I was delighted to get to spend some time with him and a young assistant who was considering graduate studies afterwards. Mardi was a true polymath and an accomplished painter and pianist. He had given me a sumi-e painting in the 1970s that still hangs in my house, and I used it as a background for some of the PowerPoint slides in my 2016 presentation. The grapes in the painting had faded, and Mardi explained that they were "fugitive" paint colors and that he'd be happy to give it a touch-up!

In 1975 I informed Mardi I was going to apply to grad school (as opposed to medical school); he told me I was making the "biggest mistake of my life" (n.b., at various points in my life others have made similar declarations; I have no idea if any of them were right). Nevertheless, he thought that if I did go to grad school, I should attend UCLA, where a friend of his, Michael Goldstein, was on faculty. I did get into UCLA (perhaps because of his recommendation). However, I opted to attend Indiana University (IU), in large part because of the fellowship I was offered but also because I was intimidated at the thought of living in Los Angeles. I was disappointed that I didn't get offers from the more psychodynamically oriented programs I applied to.

### Graduate Study

When I arrived at IU in 1976, AUDs and alcohol effects weren't on my mind. I was more interested in studying phenomena that I was exposed to by Mardi (stress response, defense mechanisms, object relations). But when it came to psychodynamic thought, IU was a desert and the epitome of "dustbowl empiricism."

In this era before mentor-based admissions, a first task after arriving on campus was to find a mentor, at least one who'd see you through your required first-year project. I systematically conferred with all of the IU clinical faculty my first week on campus and elected to work with a relatively new assistant professor, Robert (Bob) Levenson. I chose Bob because he seemed to understand my interests; at least he nodded his head and smiled a lot. He didn't dismiss me when I told him I was interested in studying transferential processes. I mused about whether we could find trainer effects in the research in heart rate biofeedback that he was conducting at the time. And it was clear he was extremely smart.

Bob possessed great technical skills in electronics, programming, electrophysiology, and data analysis and demanded that things be done carefully. I also had the privilege of working with a grad student one year ahead of me, David Newlin, who shared a number of Bob's talents and love of technology. David ultimately became a lifelong friend up until his untimely passing. I learned a lot from Bob, and I will still turn to him for advice nearly a half century after our first meeting.

I was delighted when he was awarded the Association for Psychological Sciences' (APS's) inaugural mentoring award in 2013 (<https://www.youtube.com/watch?v=blEaZKFh5OU>). Like my good fortune with respect to Mardi and attending Sussex, working with Bob was once again being in the right place at the right time. He was an excellent guide to the next stop on my journey.

The work I conducted initially with Bob and the following year under Ken Heller's supervision had nothing to do with alcohol. Ken, a community psychologist, was very supportive of my work with Bob and brought me on as a coauthor for two of my earliest publications (Heller et al. 1980, 1982). He was also a great person to hold down the advising fort with a primary advisor on a 1-year leave of absence to play in a rock band.

Before the era of the Internet and cellphone, getting Bob a manuscript draft while he was on leave was a bit involved. I'd have to call his agent to get his itinerary, and then arrange to FedEx my draft to Bob at a nightclub or motel, and follow up with a call to a landline at a motel once he'd had a chance to read it.

**Coursework.** While currently there is a tendency to minimize graduate coursework and to specialize early in order to have students jump-start their research program, that was not the ethos at IU while I was a student. My training involved extensive coursework in statistics and psychometrics as well as rigorous coursework in learning theory, sensory psychology, physiological psychology, psychopharmacology, social psychology, developmental psychology, psychopathology, and comparative psychology. While these were standard requirements in many departments, when comparing my coursework to those of colleagues and reviewing curricula when I later served on the American Psychological Association's (APA's) Commission on Accreditation (CoA), I gained a better appreciation for the rigor and depth of the instruction at IU. While the value of some of the topics in my core courses was immediately apparent, it was only many years later that some other seemingly obscure bits of knowledge proved helpful. For example, in my sensory psychology class we read Cornsweet's (1970) *Visual Perception*, which introduced me to the concept of modulation transfer functions in the context of lateral inhibition in *Limulus* (horseshoe crab) eye. I found this seminal work to be elegant (it had earned Keffer Hartline a Nobel Prize). However, the ultimately useful nugget I gleaned was that if you have an input gradient and an output gradient, understanding their relationship can provide deep insight into what is going on under the hood. Many years later, I applied this basic reasoning to postulating the "meanings and implications" of the trajectories of time-varying covariates of disorder trajectories (Sher et al. 2004) to help parse factors relating to acute exacerbations, chronicity, and stages of development. In my animal learning class, poring over every page and figure of (Sussex's) Mackintosh's (1974) text prepared me for reading dense conceptual material and complex figures in other contexts. It also enhanced my appreciation for many of the paradigms used to study alcohol's anticonflict effects (e.g., passive avoidance). These experiences cemented my belief in constructing a strong foundation of knowledge in a discipline to build upon and highlighted the idea that seemingly unrelated material could provide good food for thought via analogical reasoning, which is a primary reason I've always encouraged my trainees to read widely.

**Intellectual milieu.** Beyond the rigorous coursework at IU were the lively symposia, providing a chance to hear from current and future leaders in the field. As important as the speakers (and the Q&As that followed) were the informal discussions that took place in the hallways afterwards. I vividly recall talking with Jeff Alberts, my comparative psychology professor, after a job talk. The presenter was an obviously brilliant, young social psychologist who gave a provocative presentation of his exceedingly clever research. I was super excited by the research. I turned to Jeff, who was standing next to me in the entryway, and asked him what he thought. Thinking he'd be equally

thrilled, he flatly said he wasn't impressed. In Jeff's opinion, the speaker was trying to prove a pet theory and not understand nature as it is. That casual comment had a profound impact upon me: It helped to rein in my easy fascination with theoretical shiny objects and reinforced the value of anchoring oneself to real-life phenomena while considering a range of plausible alternative hypotheses. At a process level, it's a reason why I believe that being in extended, in-person residence is important and that doctoral programs that are largely "distance" or virtual have a difficult time creating the ambient learning environments where knowledge comes from both targeted learning but also spontaneous encounters with people who know more than you do. Approximately 40 years later, when I was president of the Research Society on Alcoholism (RSA), I had the privilege of inviting any scientist I wanted to deliver a plenary at the annual meeting. I invited Jeff, who accepted, and he gave a highly engaging talk on physical contact in immature rodents and premature human infants, a topic appropriate for the diverse audience of preclinical and clinical researchers. Simply stated, IU provided a great learning environment for those with a similar bent and emphasized foundational disciplinary knowledge.

**Getting a good taste of alcohol.** Osler (1903; quoted in Charalambous 2015, p. 198) also wrote that the "very first step in any occupation is to become interested in it." In my third year at IU, I was offered a choice between being a teaching assistant or taking a position on a National Institute on Alcohol Abuse and Alcoholism (NIAAA) Institutional Training Grant (T32) as a predoctoral trainee. I didn't want to be a TA and so I started studying alcohol and alcoholism.

Although new to alcohol research, Bob (having returned from touring) assembled a group of four graduate trainees to help him conduct a study on the effect of alcohol on stress response (Levenson et al. 1980). Compared to my earlier experimental work at IU, which focused on much more subtle and inconsistent effects on behavior, I was very impressed with the effect of an intoxicating dose of ethanol on emotional responses. Despite my own passing familiarity with intoxication, getting someone drunk was a very dramatic laboratory manipulation based on both casual observation and the multimodal psychophysiological assessments we employed. My dissertation (Sher & Levenson 1982) involved a replication of that first study combined with the study of individual differences of those effects.

I started reading more about alcohol and AUDs. To gain a more intimate knowledge of relevant phenomena, I spent time working in the local mental health center's addiction treatment center. There I learned a lot of inside baseball on the drinking culture from the clients in a therapy group I observed. I recall a couple of clients discussing how they loved to get drunk and fight—that was part of the fun of going to their favorite bars. Growing up in an upper-middle-class, Jewish suburb of New York City, I hadn't been exposed to people who thought physical fighting was fun. And it's not that I didn't know that people drank and fought, but I never contemplated that getting good and drunk and initiating physical violence might be appealing to someone. Indeed, Elton John's "Saturday Night's Alright for Fighting" suddenly made sense. Many years later I coauthored a paper on the problem of determining whether an ostensible consequence of alcohol (e.g., fighting) was truly a consequence or something else (e.g., a motivating factor or spurious) (Martin et al. 2014). Clearly, those early experiences opened my eyes in ways that mere book learning didn't.

A particularly memorable experience was the chance to meet a visiting IU alum, Alan Marlatt. I had sent him a reprint request for a chapter on alcohol and stress (Marlatt 1976) and was delighted to receive a handwritten response where he mentioned his excitement over an upcoming visit to IU. Alan was an early role model for me. He had revolutionized the field by demonstrating the important role of cognition in addiction, developed principles of relapse prevention, and later became one of the leading lights in both early intervention for drinking problems and harm



reduction approaches (White et al. 2011). As I noted on the occasion of his passing, “No other scientist has introduced more innovation to addiction theory, research, and practice, successfully challenging orthodoxy and demonstrating how addictive behaviors can be viewed in the context of core psychological and social processes” (Sher & Larimer 2012, p. 321). His initial visit while I was a student turned into a lifetime friendship. The one sabbatical I’ve taken in my 40+ years in academia was with Alan, 10 years after that initial meeting. He was the first alcohol scholar to take an interest in my career and I remain grateful for his early interest in me as a person and as a scientist. Being named a corecipient (along with George Koob) of the first G. Alan Marlatt Mentoring Award in 2012 from RSA was one of the most validating experiences of my professional career.

The alcohol training grant at IU brought in many other senior scholars from various disciplines who were inspiring in their accomplishments. This included psychiatrist Don Goodwin (who first established a strong genetic foundation for alcoholism in adoption studies), learning theorist Shepard Siegel (whose seminal work on conditioned compensatory responses continues to influence my thinking), and sociologists Lee Robins and Joan McCord, who (along with McCord’s first husband, William Maxwell McCord) pioneered longitudinal studies of the relations between externalizing behavior and substance misuse and other problems. I also had the good fortune to spend time with Peter Bentler, the distinguished psychometrician. I was delighted when I got to host a small dinner for him at my house. Peter was conducting longitudinal studies of substance use with adolescents at the time and introducing clinical and health psychologists to structural equation modeling in the process. Exposure to Bentler’s early work was a goad to me (and to the field more generally) on the value of multivariate longitudinal research.

A legacy of those early experiences with scientific luminaries is that I’ve always tried to help my own trainees forge strong bonds with leading lights in the field. No question my IU experiences clearly brought me over the threshold of Osler’s first step; I was more than merely interested. When I left IU after 4 years of graduate study to complete my predoctoral internship in clinical psychology at Brown University, I went with the express purpose of learning more about AUD treatment. There I had the opportunity to work with Barbara McCrady. She had an alcohol treatment grant and directed the day treatment program that I was initially placed on. When I went on the job market during internship, it was as someone who intended to pursue alcohol research. In 1981, I was offered a position at the University of Missouri (MU) in Columbia and accepted it. I felt fortunate to have landed the position having had only 4 years of graduate training, no postdoc, and no first-authored publications. The world has changed!

## GETTING ESTABLISHED

Well, the first days are the hardest days. . . .

—The Grateful Dead, “Uncle John’s Band” (lyrics by Robert Hunter)

When I arrived at MU in August 1981, I was intimidated (especially about teaching, where I had no experience). Nonetheless, I was excited to set up a lab and begin my career as an assistant professor. However, I soon learned that the start-up funds promised to me (and to another new faculty hire, Randy Frost) had evaporated over that summer due to “financial exigency,” a phrase I hadn’t heard before.

My first faculty meeting remains the most depressing one of my career, which is saying a lot! The chair, Sam Brown, told the faculty about the cost-cutting measures that would be in place in the current and coming years. After the faculty meeting Sam could see my dejected, pathetic



look. In a paternalistic gesture, he put his arm around my shoulder and in a faux-reassuring voice said, “I have three words of advice for you Ken: ‘rent, don’t buy.’” Then he gave me a big, broad, crooked smile and laughed hoarsely! That was Sam’s style. While I learned to appreciate his dark humor, I was less appreciative of it at that moment. I glumly retold the story of the encounter to my partner, Holly, later that evening, explaining why we might have made a huge mistake coming to Missouri.

Sam had earlier told me they’d find a way to get at least some of the start-up funds from various sources but not enough to complete my lab. He suggested that I put in for an internal research council grant. He confidently mentioned that new faculty in psychology always got what they requested. So, I put in for a grant to complete my request for a polygraph and some used breathalyzers and subsequently broke the department’s winning streak in getting research council support. Fortunately, I was successful on a resubmission. I was able to set up a lab, even if some of the polygraph modules would have to be purchased later and the used breathalyzers required more attention than I would have liked.

Seemingly, no one on campus had conducted alcohol administration research before, and consequently, there were no procedures in place to procure alcohol. Rather than just going to a local retail outlet, I was told I had to go through food purchasing (which was distinct from “normal” purchasing). They didn’t want me to buy the 80 proof Smirnoff that I planned to use in my first study but instead wanted me to purchase alcohol that they had already procured at an extortionate price for some new venture that hadn’t materialized. I balked. I’m not sure exactly how or why, but my little skirmish with food purchasing got kicked up to the level of the university’s chancellor. She phoned my chair and pointedly asked him if he was aware that one of his new faculty members was trying to purchase alcohol that would be administered to students as part of his research. Sam told her, “Yes, that’s why we hired him.” I was then able to procure the alcohol and was well on my way to being set up. However, the MU Institutional Review Board (IRB) wanted to know what safeguards were in place to deal with various emergencies that might arise when getting research participants intoxicated. I wrote to one of the leading alcohol researchers in the country, who was a psychiatrist by training. He wrote back that, among other safeguards, he thought it would be important to always have a physician on-site. As an assistant professor in a psychology department in a college of arts and sciences, I did not find this advice to be feasible! We ultimately convinced the IRB that we could use the emergency room across campus as our physician backup. Note that 8 years later, NIAAA’s Advisory Council (NACAAA 1989) drafted guidelines for administering alcohol to research participants, making the process of becoming a researcher who administered alcohol to human participants much less ad hoc than when I started out. Years later, I was part of a group that updated these guidelines (NACAAA 2005). I believe, as of the time of this writing (in October 2022), these remain as the most current guidelines. I (e.g., Wood & Sher 2000) have always been committed to advancing discussion on the responsible conduct of research administering alcohol to human research participants. This commitment, in part, is to help new investigators navigate the complexities of this area of research with attendant risks to the participants, the researchers conducting the work, the host institution, and ultimately the discipline of psychology and health research more broadly.

So, I was finally up and running—or nearly so. Owing to steep steps in the staircase leading to the restroom, the research space I was initially assigned was problematic and potentially unsafe for intoxicated participants. Fortunately, there was a somewhat better space available; unfortunately, there were no renovation funds to make it suitable for my planned experiments. I ultimately moved into that space. However, I had to sneak in on weekends to put up drywall because of union rules about who could do what, and I was told I needed to avoid being spotted doing that kind of “construction” work.

One day Sam stopped by my newly refurbished lab to check it out and asked me how long it took to do an alcohol study. I explained that it would typically take a year or more to run a single study. He listened politely and then opined that I would never get tenure at that pace! I took his concerns to heart. I began a secondary line of research where I could run multiple studies a year focused on cognitive aspects of compulsive checking behavior (e.g., Sher et al. 1983, 1984, 1989) as well as other psychological correlates (Gershuny et al. 2000). This secondary line of research was clearly of the “research, me-search” variety; I was a compulsive checker and wanted to know what about me led to checking (and others not to). It is gratifying to see that the research question we began addressing 40 years ago with studies on reality monitoring, memory for actions, more traditional memory tests, and other traits (e.g., perfectionism) in compulsive checkers remains an active and engaging area of research (e.g., Kalenzaga et al. 2020).

### Getting (Externally) Funded

There was one upside to having lost my start-up funds: I had to start writing grants to support my work soon after I got to MU. During my second year at MU, the department hired two psychologists I had known from my graduate school days, Laurie Chassin (who had been a postdoc at IU and then an assistant professor at Arizona State University) and Clark Presson (who had been a visiting assistant professor at IU and then retrained at a VA in Phoenix). Laurie, Clark, and I were usually the only faculty who worked in the evenings in McAlester Hall, the building housing the MU psychology department. We’d often go out to dinner, retire to our respective offices, and then periodically pop in on each other to either borrow wasp spray (to combat a persistent infestation), schmooze, complain, or give feedback on each other’s work. Although still an early-stage investigator, Laurie had already established herself as a successful grant writer at Arizona State, and her guidance and tutelage were critical to my own early success in securing a New Investigator Research Award (R23AA6182) in 1983. This funding supported my first independent laboratory studies of alcohol effects and served as a foundation for subsequent federal funding.

With a lab set up, vodka and tonic in the fridge, federal funding, and a secondary line of research, I was ready to put down roots. With the exception of 6 months at the University of Washington with Alan Marlatt in 1989, I’ve spent the past 41 years at MU. I’m deeply grateful to the university, colleagues, staff, and trainees for creating a facilitative and supportive work environment.

### Other Early Informal Mentors

Beyond Alan, I am indebted to a number of other senior scholars I interacted with early in my career. Peter Nathan, then at Rutgers, encouraged me to submit my first R01 grant in the mid-1980s after he became aware of my contribution to an unsuccessful NIAAA center grant application headed up by colleagues in the MU vet school. That first R01, a spinoff of the project I had written for the center grant, was ultimately funded in 1987. Peter also encouraged me to write my first book (summarizing what was known about children of alcoholics; Sher 1991), which was subvented by the MacArthur Foundation, where he worked as a project officer. Only later, when I understood how rare it was for a book like mine to be reviewed in over a dozen scientific journals, including a review in *Nature* by the world’s leading alcohol scholar (Edwards 1992), did I suspect that Peter likely played a role in promoting my work. Twenty or so years later, Peter asked me to take on the task of trying to systematize what we knew about substance use by editing a comprehensive handbook on substance use disorders (Sher 2016a,b) when he was editor-in-chief of the Oxford Library of Psychology series.

Although I had less personal acquaintance with him than others I mention in this article, Henri Begleiter is another I owe a debt of gratitude. When I attended my first annual RSA meeting in

1984, there were fewer than a dozen psychosocial researchers out of what I would estimate to be a few hundred scientists. The membership of the society was largely biomedical at the time, and it was hard not to feel like an outsider. Begleiter, a larger-than-life, French-born neurophysiologist, was a dominant leader of RSA. He had edited influential volumes of reviews of important areas of alcohol research and conducted seminal work on premorbid and clinical neurophysiological correlates of AUDs. He was interested in broadening the research constituency of RSA by getting more psychosocial scientists engaged, and he viewed me as someone who would be helpful in this regard. He encouraged me to become increasingly involved in multiple ways. Specifically, he (a) facilitated scientific engagement (including me in an invited symposium with distinguished senior scientists that promoted dialog between biomedical and psychosocial scientists; Sher 1988), (b) pushed for recognition of my work by nominating me for the first RSA Young Investigator Award, and (c) advocated for me to become the program chair for the annual meeting (1990–1991). While I benefited personally from Henri's support, I believe I was helpful in achieving his vision of making RSA into a truly multidisciplinary organization with broad representation of the biomedical, behavioral, and social sciences. When I was program chair, the program had gone from being overwhelmingly biomedical to being roughly balanced across multiple disciplines.

Others who were very helpful to me early in my career were journal editors. As I had not first-authored any publications as a graduate student, my skills in writing scientific papers were undeveloped. Two editors of the *Journal of Abnormal Psychology* (*JAP*), Don Fowles (Iowa) and Susan Mineka (Northwestern), stand out as being exceptionally hard-nosed but constructive editors who took the time to help me get my work into the best possible shape I could. They also served as role models for me when I assumed editorial positions of my own.

### Research (and Training) Funding

The initial support from the University of Missouri and NIAAA [then part of the Alcohol, Drug Abuse, and Mental Health Administration; it didn't become a part of the National Institutes of Health (NIH) until 1992] helped establish two different types of research programs, one largely experimental and based on laboratory studies on alcohol effects on mood and physiology (R23AA06182; 1983–1986) and one largely correlational, longitudinal, and based on objective personality tests, neurocognitive assessment, and structured diagnostic interviewing (R01AA07231 and R01AA13987; 1987–2002). A MERIT extension from NIH (2002–2008) of the earlier R01 grant (R37AA07231) funded a second longitudinal, cohort study of college student drinking. Later funding (R01AA016392 and R01AA024133) was used to support secondary analyses of large, national epidemiological data and allowed us to extend our work primarily with college students to more diverse populations.

While Laurie helped me learn to write grants and Peter encouraged me to repurpose my failed center grant component, Andrew Heath, a psychologist based at Washington University in St. Louis (WUSTL), taught me the value of creating synergies across different funding mechanisms to foster a more comprehensive research and training environment. I hadn't known Andrew well, having only met him a couple of times at meetings. However, he reached out to me when he moved from Virginia Commonwealth University to WUSTL to begin some collaborations. This resulted in the successful funding of an NIH research center he directed from 2001 to 2015 (P50/P60 AA11998) that was a joint effort of WUSTL and MU, involving projects based at both institutions. Once we had center funding, Andrew encouraged me to write a T32 Institutional Training Grant to expand our funding base for predoctoral and postdoctoral training. We were successful (T32AA013526) and started our formal training program in 2002, supported by both the training grant and some additional internally funded postdoc lines supported by MU. I served as training

grant director from 2002 to 2022 and now serve as associate director, with my colleague, Denis McCarthy, assuming the helm in the next funding period. The training grant has provided a strong foundation for broad-based education of our trainees. These trainees receive specialized coursework in alcohol studies and grant writing and participate weekly in a seminar that provides opportunities to develop presentation skills and obtain feedback on current and planned work from both faculty and other trainees (see McCarty et al. 2020).

Andrew also encouraged me to extend my training activities in alcohol studies beyond graduate students and newly minted PhDs to both more advanced scientists (early-career, midcareer, and senior faculty) and undergraduates. I had been serving as a mentor or co-mentor on K awards (career awards) for junior faculty at MU and WUSTL and elsewhere and even for some senior, distinguished researchers (Stephanie O'Malley, Yale; Bengt Muthen, UCLA) on their K awards for established scientists. This served as a strong foundation for mentoring faculty at MU in alcohol research, and for 10 years I held a Senior Scientist and Mentoring Award (K05AA017242) from NIAAA to provide mentoring to both tenured and untenured faculty in my own department (2007–2017). These mentees were successful in obtaining their own funding. This, consequentially, strengthened the resources and intellectual environment of our university and expanded the research activities of our faculty. During the last funding cycle of our center with WUSTL, I directed a summer internship program for undergraduates from across the country as part of its “dissemination core.” The Alcohol Research Training Summer School (ARTSS) consisted of a week of didactics at WUSTL followed by an 8-week internship either at WUSTL or MU in the lab of one of our affiliated faculty. My primary motivation for starting the program was to help improve the pipeline for alcohol researchers. Given the range and diversity of the alcohol research programs based at MU, after the initial funding through the center grant, I applied for, and received, funding for a similar program based solely at MU. This program, MU\_ARTSS, which was funded by an NIH R25 grant (R25AA023687), more actively targeted underrepresented minorities. To date, the program has been hugely successful with respect to the quality of the applicant pool and the educational outcomes (i.e., pursuing graduate and professional training) (<https://www.psychologicalscience.org/policy/summer-program-provides-undergraduates-with-hands-on-training-in-alcohol-research.html>). Extending our training and mentoring activities in alcohol studies to diverse trainees from campuses on the US mainland and American territories that lacked strong research infrastructure was one of the more gratifying mentoring activities I've undertaken. While my MU colleagues and I have done the heavy lifting for these training programs, without Andrew's gentle encouragement these programs would not have come into existence.

## PROFESSIONAL ACTIVITIES

### Editorial Service

In contrast to my own graduate students, who reviewed for as many as two dozen journals while still in training, I didn't write a single review until I was a faculty member. Despite the slow start, editorial work became a major part of my professional life and identity. In my career, I've reviewed for over 60 scholarly journals, served on 19 editorial boards, and worked as an associate, field, or acting editor for five scholarly journals. A number of people and events stand out.

*Journal of Abnormal Psychology.* I began my first appointment in 1991 at *JAP*, under Editor-in-Chief Susan (Sue) Mineka, where I tried to emulate the rigor, fairness, and helpfulness that she and Don Fowles had shown me. Sue was a great “boss,” supportive and broadly knowledgeable in the field, and she helped me build confidence in my editorial approach and backed me up when I made difficult decisions.

***Psychological Bulletin.*** My next major editorial stint was as associate editor at *Psychological Bulletin*, under Nancy Eisenberg. Unlike my experience with *JAP*, I had never published a paper at *Psychological Bulletin*, and I was initially uncomfortable in that role for that reason. Despite my initial lack of confidence, I do think I was reasonably good at calling the editorial balls and strikes expected of me and that I provided valuable guidance to authors. However, I had to work hard at it. *Psychological Bulletin* papers were often quite long, and maintaining the gestalt of the entire paper sometimes took multiple readings. Because of the broad disciplinary content of *Psychological Bulletin* and the depth of the review articles I handled, I learned a lot about a broad range of topics within psychology. Mark Twain allegedly said that “a classic is something that everybody wants to have read and nobody wants to read.” Many of the papers I read as action editor must have been “classic”; I was often reticent about picking them up at first but pleased to have ultimately read them. I began to really enjoy serving as associate editor, but then something happened that sucked all the joy out of the position and was professionally traumatic.

A paper that I ultimately accepted and that was published in the winter of 1998, by Rind et al. (1998), reported a meta-analysis of “assumed properties of child sexual abuse” (CSA) in college students. The review concluded, “Self-reported reactions to and effects from CSA indicated that negative effects were neither pervasive nor typically intense, and that men reacted much less negatively than women” (Rind et al. 1998, p. 22). The review didn’t seem to get any more or less attention in the days immediately following publication than is typical for review articles in *Psychological Bulletin*. However, approximately 9 months later, a radio personality, Laura Schlesinger (“Dr. Laura”), catapulted this paper into the national spotlight by implying that it was pro-pedophilia and that the journal’s publisher, APA, by not renouncing the paper, was condoning pedophilia. On the other side of the discussion, the North American Man/Boy Love Association was promoting the study’s findings on its web page. As noted by Garrison & Kobor (2002, p. 165), “The strong public reaction in the spring of 1999 to the Rind et al. article marked the first time that the APA had been called on in the public arena to explain and defend the publication of a scholarly research article and the peer review process itself.” The “Rind et al. affair” is discussed at length from multiple perspectives (including my own: Sher & Eisenberg 2002) in a special issue of *American Psychologist* dedicated to the controversy (Albee et al. 2002), and the interested reader can delve into the myriad issues there. [See also Sher 2015 and Alice Dreger’s (2015) book *Galileo’s Middle Finger* for additional discussion.] Suffice it to say that having my editorial decision unanimously condemned by both houses of the US Congress was upsetting. I take some solace in having received an APA Presidential Citation in 2009 supportive of my editorial efforts and in the thought that probably no other editorial decision (including editorial correspondence and the like) has been as scrutinized as mine and that appropriate standards of peer review were deemed to have been followed. I also learned two valuable lessons: (a) attempt to divine if a paper you accept is likely to generate controversy, and (b) if you think it might, solicit commentaries and provide the original authors an opportunity to write a rejoinder. I subsequently used that strategy (Arkin & Hermann 2000, Hill et al. 2000) after accepting a paper by Anita Kelly (2000a,b) which suggested that clients should probably think twice about revealing too much to their therapists, and I have done similarly on other occasions at other journals. But in the case of Rind et al., the reviewers gave no inkling that there was anything controversial, and my own alarm bells didn’t go off, perhaps because I believed that nonclinical samples will tend to show more benign correlates than clinically ascertained ones (i.e., the “clinician’s illusion”; Cohen & Cohen 1984).

***Clinical Psychological Science.*** I was honored when Alan Kazdin asked me to serve as an associate editor at *Clinical Psychological Science* (CPS) when he became its founding editor in 2012. I don’t think Alan ever slept because I’d get a quick response from him at any time of day or night

(including the wee hours) after emailing him on one issue or another. He is an extremely humorous man who might have been a stand-up comedian in an earlier life, and almost every email, even something routine, contained some gem that made me smile or laugh out loud. But aside from being entertained by my interactions with him, I learned a lot about the editorial process. This was despite the fact that I was 60 years old when I started and had already served as an action editor at four journals over two decades [*JAP*, *Psychological Bulletin*, *Alcoholism: Clinical and Experimental Research (ACER)*, and *Journal of Studies on Alcohol and Drugs (JSAD)*] and thus had probably a thousand or more decision letters under my belt. After Alan's successful editorship, Scott Lilienfeld ascended to the helm of *CPS*, and at his request, I agreed to stay on to help with the transition. Scott really didn't need my help, but in 2019 he was diagnosed with an ultimately fatal illness, and I was asked by APS to take over for him. I was comfortable in that role (which ended in 2021) given my experience as an action editor at *CPS* across its first two editors, my concurrent decision to drop editorial service at *JSAD* and *ACER* to accommodate the increased responsibilities, and my belief that I could "channel" Scott or Alan when the need arose given my knowledge of their values and standards. And things were going just fine.

Then the pandemic broke out a few months after I had started serving as acting editor-in-chief. The associate editors, like most of the rest of the world, were hit with a number of new roles and responsibilities (working remotely, homeschooling children), and some were in administrative positions and trying to develop policies for managing the impact of the pandemic on their workplaces. The same was true for the editorial board and the ad hoc reviewers. And the pandemic, in addition to straining our review capacity, simultaneously goaded a huge increase in submissions, a phenomenon noted at many journals worldwide, presumably due to increased writing time while research labs were temporarily shut down. Working at home in my basement, I definitely felt challenged as I tried to "desk reject" a higher proportion of manuscripts than usual as well as handle more myself to keep the associate editors from being overwhelmed. With the support of journal staff, the addition of a couple of new associate editors towards the end of my stint, and the goodwill of both authors and those at the reviewing end, we were able to manage fairly well. It was gratifying to be working with such a dedicated team who rose to the challenge of the novel circumstances to make sure this important work was done and done well.

Looking back over my career, I'm grateful for the opportunity to have worked with so many junior, midlevel, and senior colleagues in an editorial capacity. In the long term, Dr. Laura and the other critics don't tarnish that part at all.

### Service to Professional Organizations

I spent many years working as a volunteer to various professional societies, especially RSA, APA, and APS. If I were to do my career over again, this is something I might do differently. While I feel many of the efforts I was a part of were well motivated and successful at achieving their aims, other efforts did not result in the desired outcomes. I'd like to highlight a few activities and events that remain most salient to me.

**Society for a Science of Clinical Psychology.** In 2002 I was asked to run for president of the Society for a Science of Clinical Psychology (SSCP) and, much to my delight, was elected. I had been an enthusiastic supporter of SSCP when it was organized as a section of APA's Division 12 (Clinical Psychology) and an admirer of those who led the movement to make scientific psychology the foundation of our subdiscipline. While I actually had an agenda I wanted to pursue with respect to strengthening clinical psychology's ties to the broader discipline of psychology, this agenda was hijacked by a mini-crisis that was confronting the society when I took the helm. The previous president, Scott Lilienfeld, had written a letter to the governor of New Mexico as SSCP



president opposing prescriptive authority. (In case you, the reader, is wondering: Yes, Scott and I crossed paths in many different ways.) As I wrote in 2004:

Although it might be argued that Scott's letter did not take a stand against prescription privileges per se but rather the specific legislation under consideration, APA did not look kindly on his behavior and informed him that because: (1) SSCP is a constituent part of APA, (2) APA has a policy that no Division, Section, or Chapter can adopt policies that contravene APA policy, and (3) the promotion of prescriptive authority for psychologists was an official policy of APA, then Scott's actions constituted a violation of APA policy. According to various senior staff and legal counsel at APA, the issue was not Scott as an individual taking a personal stand. Rather, the problem, they argued, was that he was taking a public stand in his role as President of an APA Divisional Section. (Sher 2004a, p. 2)

So, my first order of business as SSCP president was to prevent SSCP from being thrown out of APA. I spent considerable time with APA staff leaders including Norman Anderson (CEO), Kurt Salzinger (head of the Science Directorate), Nathalie Guilfoyle (general counsel), and APA leadership including Bob Sternberg (president). Needless to say, SSCP remained in APA. I (Sher 2004b) argued for continued affiliation, and Scott (Lilienfeld 2004) argued against. Nearly 20 years later, tensions between APA and SSCP remain in some circles, perhaps reflecting an abiding tension between professional psychologists and those who identify as clinical scientists.

Unlike Scott, I was not against prescriptive authority in principle. However, I was concerned about the intensity and quality of training necessary for prescription privileges (Sher 2005). I subsequently served as a member of the task force representing Division 28 (Pharmacology), which helped develop guidelines for recommended postdoctoral education and training programs (APA 2009; see also updated guidelines in APA 2019). As always, I remain convinced that appropriately trained psychologists can responsibly and effectively utilize pharmacological agents as part of treatment. However, the core issue was, and remains, the quality of the training and the associated quality assurance (QA) procedures in place for both training programs and practitioners. With respect to programs, the intent of the task force was to develop a QA mechanism for a short period of time that would not be too onerous and then transition to the more rigorous QA afforded by the CoA. Despite 14 years having passed since that intention was expressed, QA continues to be done under the auspices of a "program designation" process (<https://www.apa.org/education-career/grad/designation>), and not the higher standard of QA afforded by CoA "accreditation."

**Quality assurance.** The focus on curricular QA issues in clinical psychopharmacology increased my broader interest in QA in doctoral and postdoctoral training in professional psychology. I was asked to serve, representing APA's Board of Scientific Affairs (BSA), on APA's expanded CoA in 2008. The experience was a decidedly mixed one for me. The work was more onerous than the editorial work at leading psychology journals. Most program self-studies and site-visitor reports were not "classics" that I was grateful to have read; only rarely did I come upon some useful nuggets of wisdom or get ideas for how to better train the next generation of psychologists. I'm grateful to my colleagues who've been willing to take the essential work of serving as commissioners because for most academics like me, there was only opportunity cost and few direct benefits. I recall pulling multiple all-nighters, looking up HR policies on a school's website at 3 AM to complete my evaluation of a program. I found it irritating since it had nothing to do with my (or my fellow commissioners') expertise, and such tasks would be better performed by professional staff. Still, I found the QA work important. When I later cochaired BSA, one of my responsibilities was to recruit someone representing science interests to serve on CoA. I went through dozens of potential commissioners, often pleading with them to serve (with little success). However, other psychologists (especially representing professional interests) would literally campaign to be appointed.



Although I opted to serve only 2 years of my 3-year term, I remained invested in the QA aspects. When the new Standards of Accreditation (SoA) were initially posted for public comment, I tried to get CoA to change what I saw as some of the most egregious guidelines that undermined quality training. This included the minimal requirement of only 1 year in residence or “the equivalent thereof” in the doctoral program (APA 2015, p. 7). As bad as 1 year sounded, the phrase “or the equivalent thereof” made me cynical of colleagues who clearly had a different vision of what doctoral training meant. My perspective is that in human service psychology, there needs to be ample opportunity for assessing a trainee’s conduct in multiple contexts over time as well as having appropriate behaviors role-modeled. Perhaps equally irksome was the requirement that “core faculty must be composed of individuals whose primary professional employment is at the institution in which the program is housed. . . [where] . . . [a]t least 50% of core faculty professional time must be devoted to program-related activities” (APA 2015, p. 12). While sounding fine in principle, the effect (if not the intent) was to allow a given institution to house two doctoral-level degree programs with the same faculty counting as core in each. While on CoA and when the SoA was open for public comment, I argued that the threshold should be 51%. I viewed these “standards” as little cheats in the service of programs that gamed the system. In my judgment, it’s extremely unfortunate and reflects the enduring political successes of the Dirty Dozen (a group of 14 psychologists who moved APA away from being a scientific organization and towards being more of a professional guild) and their successors in minimizing the influence of scientists on professional training (McNally 2003, Wright & Cummings 2001). Technically, APA is a scientific organization that operates as a charitable organization serving the public good, and its “companion organization,” APA Services, Inc., represents the guild interests of psychology. However, my perspective is that the governance structure of APA evolved in such a way as to minimize the influence of scientific psychologists on practice-related issues and the association as a whole.

**Trying to reform APA governance.** When I attended my first APA Council of Representatives (CoR) as a member in 2007, I had a difficult time understanding the nature of this primary governing body. It was challenging to have sufficient time for useful discussion as many of the representatives seemed more invested in pushing an agenda than in engaging in meaningful dialog. It was also intimidating to address the large assembly. Indeed, taking the microphone after a series of other, highly politically charged statements in favor of a position that I disagreed with made me feel like I was in a Solomon Asch conformity experiment. At the time, I was the sole representative of Division 28 (Psychopharmacology) in a room with approximately 162 other representatives. While most divisions were like Division 28 in having only one CoR representative, some had many more. Even today, Psychoanalysis (in my opinion, an important but niche subdiscipline) has five representatives in comparison to foundational disciplines like Experimental and Cognitive Psychology (Division 3), Developmental Psychology (Division 7), and Social Psychology (Division 8), each with two. Additionally, beyond the divisions, there were representatives from 60 state, provincial, and territorial associations (SPTAs). Unlike the divisions, they were technically not part of APA and not subject to its by-laws. Having had to fight to keep SSCP in APA after Scott Lilienfeld’s letter to New Mexico’s governor on prescriptive authority years earlier, it irked me that SPTA presidents could have done the same thing Scott had done with no formal sanctions and remain in good standing with APA. Moreover, unlike the divisions, which were, like their parent organization, scientific organizations chartered by the Internal Revenue Service as charitable, public-serving 501c3 organizations, the SPTAs were 501c6 organizations. That is, the SPTAs were organized as guilds set up to advocate for the needs of their members and not the good of society, yet these guilds control 37% of CoR votes. In practice, it’s even worse: Many of the divisions’ interests are largely guild interests even if they are technically 501c3 organizations.

In sum, the governance structure was too large to allow adequate dialog and too dominated by guild interests, and the representatives did not reflect the diverse interests of the broad discipline adequately. In my opinion, CoR heavily weighted niche practice interests while underweighting basic science interests. Worse yet, only half of APA members belonged to a division or SPTA and, thus, were functionally disenfranchised from directly voting for representation. Additionally, some members were hyperenfranchised and belonged to multiple big divisions (as well as SPTAs) and could vote for a number of representatives each year. I was therefore pleased when I was asked to serve on the Good Governance Project (GGP), whose charge was to examine APA's governance structure and make recommendations to CoR in governance reform. From 2011 to 2013, the GGP was quite active and conducted extensive evaluations, which were used to formally make recommendations on possible reforms (see <https://www.apa.org/about/governance/good-governance>). While most recommendations were adopted, addressing the most critical one—the size and structure of CoR—was postponed and never taken up seriously by CoR. Since this issue was still on the table, I elected to pursue it by running for a position on APA's Policy and Planning Board (P&P), on which I served from 2015 to 2019. Under APA's by-laws, P&P has a unique role in APA governance and has the authority to bring by-law amendments directly to the membership and bypass the APA Council. Towards the end of my term, P&P worked long and hard putting together a proposal that would enfranchise all members equally for voting on council reps, ensure representation from underrepresented groups, represent the broad interests of the discipline, and be flexible with respect to changing association needs (see <https://www.apa.org/about/governance/bdcmte/2019-annual-report.pdf>). Unfortunately, after I rotated off P&P, members elected not to bring the proposal forward to the membership because they didn't feel like there was sufficient support from the council. This was despite the fact that the point of having P&P shepherd this effort was precisely that the council had a conflict of interest. In my opinion, the Dirty Dozen's influence still persists in that it has created a system that is self-perpetuating and difficult to reform. I genuinely appreciate much of the good that APA has done and continues to do for the profession and society, and I have great admiration for many of its current and past leaders and staff. However, I believe that much needs to be done to make it an organization that effectively serves and champions all of psychology. I hope that others will continue the efforts that I and many other colleagues have started, and hopefully they will be more successful.

## CONCLUDING THOUGHTS

I'm grateful to the *Annual Review of Clinical Psychology* for inviting me to write this piece. I've felt a strong connection to Annual Reviews over the years through both writing for two of their journals—the *Annual Review of Psychology* (Sher & Trull 1996) and the inaugural volume of this journal (Sher et al. 2005)—and serving on the Editorial Committee of this journal from 2008 to 2012. It is a great honor to share my experiences and thoughts here.

The process of reflecting back on my career has been a valuable exercise. The hardest thing was picking what to focus on for this article. My first, overinclusive draft was about specific research accomplishments, but as I noted at the outset, these are easily accessible. I chose to focus on why alcohol research is an important area of study and on those who were instrumental in fostering my career. Much of my career has been a random walk, although an extremely lucky one where I kept bumping into generous, brilliant scholars who took an interest in me and elected to give of themselves to help me in my career. Missing from my story here, beyond the specific nature of my research contributions, is the immensely gratifying and productive interactions with my colleagues at MU (especially Tim Trull, Phil Wood, and Doug Steinley) and elsewhere (especially

Laurie Chassin, Andrew Heath, and Chris Martin), my truly exceptional predoctoral and postdoctoral trainees who've been reshaping the field for decades and are too numerous to mention, and dedicated staff (especially Gail Raskin and Carol Waudby). Each group deserves my recognition and I hope to provide it in another context. At every step it's been a team effort and a true privilege.

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