

ENTRETIEN

Power Structure Research: An interview with G. William Domhoff, May 24, 2023

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G. W. Domhoff est professeur émérite à l'université de Californie à Santa Cruz, où il a enseigné la psychologie et la sociologie à partir de 1965. Il a publié de nombreux ouvrages, dont Who Rules America? revu et actualisé de nombreuses fois depuis sa première parution en 1967.

L'entretien s'est déroulé en visioconférence et la transcription a été ensuite révisée par G. W. Domhoff.

G. W. Domhoff is Professor Emeritus at the University of California, Santa Cruz, where he taught psychology and sociology from 1965. He has published numerous books, including Who Rules America? which has been revised and updated many times since its first publication in 1967.

The interview took place via videoconference and the transcript was subsequently edited by G. W. Domhoff.

CC: As a beginning, can you recall your initial personal motivations for working on the interlocking directorates and power structures?

GWD: I should begin with the fact that I had a very apolitical upbringing, so my motivations for studying power did not develop until I was in my mid-20s. Also, I went to high school and college in the 1950s, which are called the “Silent Fifties” in the United States. That’s because everybody seemed to be involved in everyday life and in their jobs in the aftermath of World War II. And there were no nationwide protests or demonstrations that I remember. The exceptions were civil rights demonstrations in two or three Southern cities in the mid-1950s, but they had no impact on me as a white teenager growing up in a western suburb of the then-large Northern industrial city of Cleveland, Ohio. So, in looking back on how I became involved in studying power I think it was most generally because I was always concerned when I saw anything I thought of as unfairness during my high school and college years. The various instances of unfairness involved small things relating to campus rules and regulations, the details of which are too minor to spend time on here. But they did matter to me. So, as I will explain in a minute, it was events in the 1960s that had the biggest impact on me as far as becoming involved in the study of the American power structure.

It's also relevant that I left Ohio to go to college at Duke University in North Carolina, which is part of the American South. This region has a slave-and-caste history it has never been able to overcome, and it is now the center of the Republican attempt to replace democracy with a white autocracy. It is therefore in many ways different from Ohio and the rest of the North. Back then white Southerners still had a burning resentment that their grandfathers and their great-grandfathers were defeated in the Civil War, and they often let "Yankees" like me know it. White Southerners no longer talk about Northerners as "Yankees," but their anger and resentment over losing their slaves, and then being forced to abandon legalized segregation in the 1960s, is just as deep.

I went to Duke only because I intensely disliked cold winters and snow, and I wanted to play baseball at a university that had a highly rated baseball team and a warm climate. So, as you can see, my motivations were far from being academic or political, and I had only a superficial understanding of what the South was like. However, even though I knew very little about American history, I was an excellent student. I had received high grades in all my classes, and was a co-valedictorian of my graduating high-school class. I also received high enough grades in my first two years of college that I was named to the Phi Beta Kappa honorary society at the end of my sophomore year. And even though I was a good student, I showed no signs of becoming a future researcher. And I had learned I was not very good at playing baseball.

I took a wide range of courses during my first two years of college, and settled on being a psychology major, with an interest in motivation, personality, and social psychology. However, psychology at the time was primarily focused on using animal models to study learning and memory. I did not like that kind of research one bit, so I had to endure several courses I did not enjoy. But I did learn a great deal about methodology and research in those classes, and I became very empirically focused and data oriented. By the time I graduated from Duke in 1958 and went to graduate school in psychology, I was glad I had received such rigorous training. As a result, after I received my M.A. at Kent State University in Ohio, I was asked to teach the introductory psychology course and the introductory statistics course at the University of Miami, where I did my last three years of graduate school. I taught those two courses for two years, and then began a one-year, dissertation-year fellowship. The teaching of introductory statistics caused me to become more quantitatively oriented.

That fellowship allowed me to focus fully on my dissertation on the quantitative study of—and this may come as a surprise—dream reports. At the time the study of dreams was a new and hot topic because the rapid eye movement stage of sleep ("REM sleep"), in which dreams most often occur, had been discovered just a few years before. And only a few researchers in psychology had taken advantage of the new possibilities that had arisen for the study of dreams. The dream reports I studied were collected by one of my mentors from participants in a sleep-dream laboratory, who were awakened to report their dreams. My dissertation therefore involved a detailed study of everything that is found in those dream reports, such as settings, objects, characters, social interactions, and various kinds of activities. It had a very rigorous-sounding title, "A Quantitative Study of Dream Content Using an Objective Indicator of Dreaming." I then

began my academic career in the Fall of 1962 as an assistant professor of psychology at California State University in Los Angeles, where I taught courses on introductory psychology, developmental psychology, personality, and social psychology, and did research on dreams. I've told the story of how I became a professor in detail in an oral history, so I won't dwell on it here. I mention all this because it shows how far I was from sociology until I first read C. Wright Mills and other sociologists in 1962 and 1963, at just about the time I was starting my first job as a psychology professor (Domhoff, 2014).

MO: How did you come to read C. Wright Mills? I'm interested in the reception of C. Wright Mills at the moment, people who are receiving and reading C. Wright Mills. You also talked about teachers who are rigorous in the sense that you have to have the data. In what kind of academic environment have you studied, have you worked?

GWD: Well, it's a circuitous story, and it doesn't start with anything that's related to my academic interests at the time. It begins in the summer of 1960, after my first year in graduate school at the University of Miami, when I met a woman from California in Copenhagen while I was on a two-month trip through several European countries, including France. This trip was possible for me to afford because I had free passage over and back on the S.S. Groote Beer, a Dutch passenger ship, in exchange for serving as the editor of the ship newspaper. (The ship newspaper was published almost every day of the ten-day trip, and it was widely distributed among the thousands of student passengers on board, several of whom I interviewed so there could be human-interest stories in each edition.) It was possible for me to be hired for this job because of all the work I had done in junior high school, high school, and college as a journalist. This point about how I was able to afford to go to Europe as a graduate student is relevant to my transitioning into sociology because that's how I learned to write quickly and directly in order to meet deadlines, and to become comfortable doing interviews. I also had worked during summer vacations for a large afternoon newspaper in Cleveland while I was in college. Also, I worked as a reporter and an editor for the college newspaper while I was at Duke, and even for six or seven months part-time for the downtown newspaper in Durham, North Carolina, during my senior year at college.

Moreover, the woman I met in Copenhagen and married the next summer carried me from Florida to the opposite side of the country, California, where she was born and raised in an intellectual and politically engaged family. She missed the mountains of California, and both of us disliked the hot and humid climate of Florida. It was this move to California that first brought me into a sociological and political realm. That's first of all because my wife's grandfather was a retired agricultural economist with an M.A. from Stanford University. He had worked in the New Deal in the 1930s, and read widely, including the work of C. W. Mills. So, too, my wife's stepfather, who had received his undergraduate degree at the University of California, Berkeley, was interested in politics and widely read. It was through them, and other of my wife's relatives, that I became more interested in current events and sociology. And it's also the case that the growing Civil Rights Movement had caught my attention. In addition, more and more of the students I was teaching were becoming political activists as the events of the 1960s in the United States unfolded. They were also concerned about the

extent to which there was free speech on campus concerning political issues, and I was drawn into that controversy to some extent.

Moreover, the Civil Rights Movement resonated for me because of my four years at Duke. Those years, 1954-1958, were years of total segregation, and it came as a surprise and somewhat of a shock to live under those conditions, even though there was plenty of segregation in the North as well. The South felt very different to me, and I soon learned how brutal and callous most of my white Southern classmates were in their beliefs about African Americans. It was a situation that made me uncomfortable, but I did not fully come to grips with my thoughts about all this at the time. I was too focused on trying to achieve my own goals. Once the resistance to the Civil Rights Movement began, though, I knew that many white Southerners were fully capable of the violence they soon carried out. I also believed there would have been even more bloodshed if federal troops had not been sent into Southern cities at critical moments.

So, that's the context in which I first read C. W. Mills. Since then his work has had an enduring impact on me, which included his insights about other levels of American society. But as important as Mills has been to all my subsequent work, I didn't just read *The Power Elite* and many of Mills' articles and essays. I also read all of Mills' critics, something I had learned was essential during my graduate-school training. I was by then a skeptical empiricist, although I had a growing interest in all the major ideas of that era. And I also read the researchers that Mills drew upon. By reading those various viewpoints, I found ways I could do my own independent research.

It is therefore literally true when I began the first edition of *Who Rules America?* with the assertion that the book was "inspired by the ideas of four very different" researchers (Domhoff 1967: 1). Besides Mills, the other three social scientists who influenced me were (1) a sociologist from the upper class who focused on the social ties within the upper class, E. Digby Baltzell (1958, 1964); (2) a Marxian economist born into the upper class, Paul Sweezy (1953); and (3) a middle-class pluralist political scientist, who focused on methodological issues concerning the study of power, Robert A. Dahl (1958). Dahl (1961) also did a study of political power in New Haven, Connecticut, which he claimed was a microcosm of American politics. By studying how decisions were made in New Haven on three different policy issues, by far the most important of which was urban renewal, he concluded that power is more widely dispersed than Mills claimed. His study had a big impact in the social sciences at the time, and supported a theory known as "pluralism." As a result, my focus was naturally on meeting the methodological standards that Dahl had put forth, and on seeing if I could determine if his criticisms of Mills were accurate.

Moreover, I had read all of the critics of those other three social scientists as well as the critics of Mills. I also read the social histories of the upper class as well as government reports on corporations. The very detailed notes for each chapter at the end of the first edition of *Who Rules America?* discuss every source I drew upon and the reasons why I had come to my various conclusions. Those notes include the fact that I disagreed with Mills on several issues, and perhaps especially on the decision-making importance he accorded to the military. My reading of Mills' critics and subsequent research studies convinced me that Mills was wrong on that issue, as well as a few other issues, and I later wrote chapters and essays to that effect (e.g., Domhoff 1968: Chapter 15; Domhoff 2006).

Put another way, I was mostly naïve about the historical traditions of the major theories, but I was well aware of the nature of the arguments, methods, and sources of evidence as of the year 1966, and I had gathered much new evidence on my own in the previous three years. In effect, even though my graduate training was in psychology, I was able to learn about the traditional theories that sociologists and political scientists were taught in graduate school by reading the current exemplars of those traditions. But I did not know, for instance, that Mills was in a Weberian and institutionalist tradition. Yes, contrary to what many historical institutionalists in sociology and political science seem to think, Mills mentions “institutions,” “the institutional order,” or “institutional hierarchies” repeatedly throughout his book. For example, he mentions one of those three terms 36 times in just the first 19 pages of *The Power Elite*, which I can confidently assert through a search of the pdf version of the book that has been available on the Internet for over a decade.

And there was one way in which my work was different from that of all four theorists I drew upon. Based on my reading of articles and books by Washington journalists, and the research I subsequently did, I was fairly certain that a variety of policy-oriented nonprofits—namely, foundations, think tanks, and policy discussion groups—were essential to understanding corporate dominance. By 1970, three years after the first edition of *Who Rules America?* appeared, I was conceptualizing these three types of policy-oriented nonprofits as part of a “policy-planning network.” This network is based on the corporate connections of many of their directors and the linkages created by money flows that I had detailed in *Who Rules America?* That policy-planning capability has been an ongoing theme for me ever since, and I will likely say more about it later in the interview if questions related to it arise.

Turning now to more directly answer the second part of Mohamed’s question, by 1968-1969 I had come to realize that Mills not only provided what I saw as a sensible starting point for me. By then I had come to know a wide range of sociologists and former members of the activist group Students for a Democratic Society (perhaps known to readers as SDS). I knew from them that Mills also had created an opening for young scholars and activists who had been raised in the Marxian tradition by their left-wing parents. Mills offered a new direction that was for them an escape from dead ends and old traps. Even if they remained loosely Marxist in a general sense, they admired the independent efforts by Mills, who was highly critical of Marxism, and especially of the Marxists of the Old Left (Mills 1962). They therefore found my non-Marxist work to be useful, too, even if they did not fully agree with me.

I mention this not only because Mohamed asked about Mills’ general impact at the time, but also because the reactions of this 1960s generation of young social scientists, many of whom characterized themselves as “Red Diaper Babies,” provides a huge contrast to how my work was regarded and treated by the structural Marxists who came of age in the early 1970s. For them, Mills was an “elitist,” and therefore had to be rejected. The often-cited article by David Gold, Clarence Lo, and Erik Olin Wright (1975) in the Marxist journal *Monthly Review* epitomized this contrast. In the process of their attacks on Mills, my work was mischaracterized as “Marxist,” and a very unsophisticated version of Marxism as well, but that gets ahead of the questions you may ask about later decades. So there were tensions between Marxist and non-Marxist power structure researchers within a few years after *Who Rules America?* was published. Those tensions

were often held in abeyance, or fully ignored in some specific research areas, but they never went away.

However, I also want to add that in my later work I did not simply build on my attempts at an empirical and conceptual synthesis of what I had learned from Mills, Baltzell, Sweezy, and Dahl. Instead, my further work was largely shaped by doing new research that would answer the main criticisms of *Who Rules America?* In that sense, my empirical work always has been part of a dialogue with other social scientists about current ideas and events. Generally speaking, my reviewers had three main critiques. First, they said I had not shown that the owners and managers of large corporations had the social and policy cohesion, and the self-consciousness of themselves as a unique collectivity of people, to dominate the government. Second, they claimed my framework could not explain several seemingly liberal legislative enactments, and in particular the Social Security Act and the National Labor Relations Act, both of which passed Congress in 1935. Third, they claimed that a book such as *Who Rules America?* could not explain the nature of the Democratic Party, which at the time had a highly visible liberal-labor alliance within it. I spent much of my time over the next several decades trying to improve my answers to those critiques.

To make another long story shorter, my empirical work detailed in books I published in the 1970s answered the cohesion and self-consciousness question (Domhoff 1970: Chapters 1-4, 1974), and my archival work related to the Social Security Act and National Labor Relations Act has been gathered together in voluminous detail in a recent book (Domhoff 2020: Chapters 2-8). My interview and archival work on the Democratic Party, which charts the ongoing changes in that complex and constantly evolving party, is best catalogued in the updated eighth edition of *Who Rules America?* (Domhoff 2023, Prologue and Chapter 6). I know it is immodest to say so, but I think my 60 years of studying the American power structure shows that I have constructed a more accurate account of how the United States is ruled than any of the other theorists, whether they are pluralists, Marxists, or the new group of theorists who came to the forefront starting in the 1980s, the historical institutionalists. I also think that events over the past 55 years have proven these other schools of thought to be wrong at every turn, whereas I have understood that corporate dominance in the United States has been *increasing* since a turning point that began with the Republican triumph in the 1968 presidential elections. Moreover, one key reason that the Republicans triumphed in 1968, and won more presidential elections than Democrats did between 1968 and 2020, is that the Southern rich, and white Southerners more generally, switched their allegiance to the Republicans as the best possible way to continue to marginalize and subjugate African Americans.

Here I want to add that my emphasis on policy-oriented nonprofits later led me to have a very cordial relationship with the Greek-French structural Marxist, Nicos Poulantzas, even though his theoretical views concerning state autonomy were very different from mine. I add the following account because I think it might be of interest—or amusement—to French social scientists. My brief acquaintance with Nicos occurred because in the spring of 1979 I was invited to teach a course in the Department of American Studies at the new University of Paris 8, which was then located in Vincennes. As French readers know, this university was moved to Saint-Denis in 1980 and was soon renamed University Vincennes-Saint-Denis. I was also asked to meet from time-to-time with the assistant professors in the Department of American Studies, which led to many lively discussions

with the Marxist members of the department, the most argumentative of whom was the daughter of a well-known history professor who was a prominent member of the French Communist Party at the time. Those discussions led to one or two pleasant dinner parties as well.

In any event, Nicos also happened to be teaching at the University of Paris 8 when I was there, and was very welcoming to me when we were introduced by a mutual friend, a completely bilingual American economist. Nicos did not seem to be bothered at all by our theoretical differences. Instead, he wanted to talk to me about my work on “clubs,” which I quickly learned from our mutual friend meant more like what I meant by a policy-discussion group than what “club” means in English (a strictly social group). Nicos had recently read a new book about France entitled *La République mondaine: Essai sur le giscardisme* (“*The Worldly Republic*”) (Bothorel 1979). It revealed the existence of a monthly policy-discussion group (Le Siècle) that included about 350 members and had been holding meetings since 1944. Its members were French business leaders, policy experts, leaders of political parties, and government officials. Nicos seemed surprised by this information and he seemed to think the book was important, and urged me to read it. At that point, still another bi-lingual friend visiting in Paris at the time, a sociologist, read through the book and summarized its key points for me.

After telling the students in my class about this book, one of my students told me after class that she wanted to see if she could find any other policy-oriented nonprofits in Paris for her research project for the class. Within a week she came back with the news that there was an organization named l’Institut français des relations internationales (IFRI), the French Institute on International Relations. When I then mentioned this information to still another American social scientist in Paris, a political scientist in this case, he told me that one of his French friends, Dominique Moise, was one of the co-founders of this organization, which had been established only recently. So I of course interviewed Moise, who later became a very well-known French expert on foreign policy. He was fully aware that there was a Council on Foreign Relations in New York, and thought of it as a forerunner and model. In fact, the American version of the Council on Foreign Relations had been the main source of the plans that led to an international economic system after World War II that was led by the United States (Domhoff 2020: Chapters 10-14, for the full story, based on research in historical archives).

My French student, whom I originally had thought was an American because she spoke English with a perfect Brooklyn, New York, accent, soon told me that she also had found the French equivalent of the Committee for Economic Development in the United States, a discussion group focused on economic issues. It was called the Institut de l’entreprise. I interviewed its chief economist at length, and learned that the Institut de l’entreprise was also modeled after its American equivalent.

Naturally, I wanted to write an article on all this for academic reasons, and to memorialize my incredibly enjoyable two months in Paris. I entitled it “Provincial in Paris: Finding the French Council on Foreign Relations.” It appeared in the journal *Social Policy* in the Spring of 1981 (Domhoff 1981).

And yes, when I picked that title for my article it was because I had learned while in Paris that the immortal French novelist Honoré de Balzac had used a very similar phrase in at least one of his books. I therefore borrowed the phrase

from him in a slightly edited form because I certainly felt like a provincial in Paris as a monolingual American who knew little or nothing about France. And I dedicated the article to the memory of Nicos Poulantzas, who had died tragically in the autumn of 1979. I dedicated it to him because he had made my stay in Paris even more memorable for having had the opportunity to talk with him on two or three occasions.

CC: You talked about the interviews you did and you also did an ethnographic study of the Bohemian Grove, which is very famous. I showed your documentary video of your work on this subject to my students and I wanted to ask you some questions about it. How long have you been studying this club? And what difficulties did you encounter?

GWD: Based on all I had learned about social clubs in the first several years of my research, I knew I wanted to study one or more social clubs at some point if the opportunity arose. I thought that such a study might contribute to my effort to show that social cohesion, a sense of group consciousness, and an ability to develop policy cohesion did exist among those I claimed to be the leaders of the American power structure.

But the reason I studied the Bohemian Club, and especially its two-week summer encampment at the Bohemian Grove, is that I was unexpectedly able to obtain a full membership list. I obtained this membership list in the process of one of the interviews I did for my research on exactly which wealthy people supported the Democratic Party. The interview was with a liberal lawyer who had written a book or two of his own. He knew of my work, so he was very welcoming to me. I had already noticed in glancing at his bookshelves that he had booklets that contained membership lists for both the Bohemian Club and the Pacific Union Club, the latter of which I knew to be an even more exclusive club in San Francisco than the Bohemian Club.

At the end of the interview, I asked him if it might be possible for me to make copies of those two lists, and he said yes. This made it possible for me, first of all, to compare the two membership lists, and I quickly learned there was a large overlap. This large overlap gave me confidence that the Bohemian Club was indeed a high-status club. Then I asked my research assistants to gather as much biographical information on as many members of the Bohemian Club as possible, using the reliable sources that were available in that day and age.

We relied especially on *Who's Who in America*, which I knew from Baltzell's (1958) work to be very valuable in learning about prominent people in the United States, including business leaders. *Who's Who in America* in that era often included information on the private high school ("prep school") that people had attended, and their work history, but also on their memberships in social clubs and the corporate directorships they held. Unfortunately, this useful source of information that was highly relevant to ascertaining a person's class standing became less useful a few years later because corporate leaders were less likely to list prep schools and social clubs due to the use of *Who's Who in America* by activists to find out if they belonged to clubs that discriminated against women, Jews, and African Americans. By the early 2000s, the Internet had made *Who's Who in America* even less useful, and hence less profitable to publish. It therefore became a vanity book that listed anyone who was willing to pay a large fee to be listed, and hence it became useless for power structure researchers.

At any rate, thanks to the study of *Who's Who in America* listings, I had good reason to believe that the Bohemian Club was well worth studying in more depth, so I turned to various business reference sources for more information. By then I was also accumulating membership lists for other high-status social clubs from a variety of sources—mainly libraries and people who had read *Who Rules America?* and wanted to contribute to my future research. This small number of people included a few club members themselves, or more often their college-age children who had read my book in courses at the universities they attended.

When all of this information was gathered together, I had a matrix that included the corporations, policy-oriented nonprofits, government agencies, and social clubs with which members of the Bohemian Club were affiliated. That matrix appeared at the end of the hardcover edition of *The Bohemian Grove and Other Delights* (Domhoff 1974) under the title “The Appendix of Heavies.” (“Heavies” is a slang term that was used in the 1970s to designate people who carried weight in the world—that is, people who were powerful.) By the way, the Appendix of Heavies is now available online as a spreadsheet for free download for anyone who wants to make use of it. There’s also a free pdf copy of the book online, which includes a new preface I wrote for it in 2021. This new preface provides background information concerning my research on the club. Then, too, the book is summarized and augmented by numerous photographs and tables in a document that I put on my website. (As mentioned by Catherine as part of her question about the Bohemian Grove, there’s also a 29-minute video presentation on the Bohemian Grove on the website).

Returning to the work I carried out in relation to the Bohemian Grove, I next did a large amount of archival research in various libraries in the San Francisco area, which gave me historical perspective. I even found a copy of the text for a ritual ceremony that is enacted at the start of each summer encampment, called “the Cremation of Care.” The ceremony is an enactment of the men’s desire to put aside all the cares and woes they deal with in their everyday lives, and to focus on having a good time. In fancier terms, it’s a classic rite of passage into another realm of existence. (I read a few passages from this script and displayed pictures of the Cremation of Care ceremony in the video documentary that Catherine mentioned.)

Based on my studies of the membership lists and archival sources, I also learned that the Bohemian Club had a unique dimension. It included many local musicians, actors, and artists, and even several dozen professors from Stanford University and two or three University of California campuses. From an early stage in its history, the club had included such members because of its emphasis on entertainment and leisurely activities at the Bohemian Grove. These members were responsible for creating and acting in the plays and concerts, and giving lectures on a wide range of topics, which are a regular feature of the encampment. These non-business members were designated as “associate members,” and they paid lower monthly dues to belong.

Within that context, I searched for informants to interview. Some were friends of colleagues with socially elite connections. Others were associate members of the club, or else employees or former employees of the club. One was a local pediatrician who had worked at the Bohemian Grove during the summer when he was an undergraduate at the University of California, Berkeley. Another was a graduate student in music at UC Berkeley whom I met through a friend of a friend. Obviously, I could spend hours telling stories about the haphazard and

accidental ways in which I found these informants. Suffice it to say here that this kind of research involves efforts that are also used by investigative journalists, with which I was familiar due to my earlier work as a journalist. Power structure research therefore can sometimes involve methods that are very different from those usually employed by social scientists. In fact, some social scientists don't approve of these more informal methods. They don't like the fact that such research sometimes has to be "opportunistic" when the occasion demands.

As part of my interviewing, I had lunch at the downtown "clubhouse" in San Francisco three different times. (This "clubhouse" is in fact an imposing four-story, red-brick building in the downtown area, with a theater in the basement for in-town performances, and with guest rooms for people from out of town on the fourth floor. On the second and third floors there are a big dining room, a big "reading room" [library], and an elegant bar room.) On each occasion I had lunch there, I asked my host for a tour of the building. And each time, as soon as I was alone after I left the premises, I would sit down and write out everything I could remember.

One of my informants offered to take my wife and me to the Bohemian Grove itself for what was called, at least back then, the "June Picnic." It was also the only occasion during which women and children were allowed into the Grove. If that makes the two-week stay in the cabins in the Bohemian Grove in mid-July sound like the men-only huts in indigenous societies, that may not be far from the mark. An anthropologist who had spent many years studying an indigenous society in the Brazilian Amazon wrote an article comparing the Bohemian Grove to what he had learned about male rituals and secretive huts in that society. He titled the article "No Girls Allowed" (Gregor 1982).

In the final chapter, I presented systematic evidence, including two tables, that the corporate leaders who were members of the Bohemian Club were also involved in the major policy-oriented nonprofits. I was confident that I had shown that the social cohesion that develops through being involved in social clubs furthers the development of policy cohesion when people then interact on the boards of the policy-oriented nonprofits. My claim was, and still is, that it is necessary to think of the American power structure as an ongoing series of face-to-face, small-group meetings, which involve an overlapping set of members in each instance. I then invoked the very fine experimental literature in social psychology on the social dynamics within small groups as further evidence on the ways in which cohesion develops in corporate board rooms and policy-planning discussion groups.

Since I was very satisfied with the results of combining biographical, archival, and interview methods in the Bohemian Grove study, I also used the same multi-methodological combination of membership lists, archival data, and interviews in another study I did in the 1970s after I finished the book on the Bohemian Grove. It entailed a restudy of Dahl's famous study of New Haven, Connecticut, where Yale University is located. It's titled *Who Governs? Democracy and Power in American Cities* (Dahl 1961). Even though his study was carried out in the late 1950s, there were still plenty of people around in the 1970s who had worked in the local Chamber of Commerce and related organizations, and were willing to talk to me. Among others, I also talked to a corporate lawyer who had done work for Yale, and he politely disagreed with Dahl on several crucial points. I also found one of the key aides to the former mayor who would talk with me. This aide had gone on to manage urban renewal projects for the state of New

York. He was completely frank in talking with me, and often contradicted the spin that the former mayor tried to put on his work when he talked to Dahl. I even interviewed Dahl himself. He was very cordial, and offered me access to all of his interview transcripts. By then I could also find archival records that proved to be very valuable. Based on all this new information, I wrote a very different account of events in New Haven than Dahl did, and my book stirred up a lot of controversy (Domhoff 1978).

Just as I continued to do updates on the Bohemian Grove book, I continued to revisit New Haven as more archival materials became available in the late 1990s and early 2000s. Then I wrote an even more complete account of power in New Haven, which among other things demonstrated that Yale University was far more powerful than Dahl claimed it was. I made this updated and extended version of the book available online at http://whorulesamerica.net/local/new_haven.html. I titled this new version “Who Really Ruled in Dahl’s New Haven?” It was updated again as recently as 2014.

In the process of studying New Haven, I also learned that American cities are not dominated by the same owners and managers that dominate at the national level. Instead, most American cities are run by land owners and real estate investors. The many cities that are dominated by downtown real estate interests are now called “growth machines” in the academic literature, and they include all of the cities that had been studied in the United States before 1980 (Domhoff 1983: Chapter 7, Domhoff 1986). There are by now a few exceptions, including the city I live in, Santa Cruz, which was the most progressive and leftwing city in the United States from about 1982 to 2010, with a city council that consisted of feminists, socialists, liberals, and environmentalists. One of my former sociology graduate students and I carried out a detailed case study of Santa Cruz, and included information on other cities that became more progressive in the 1980s and 1990s (Gendron and Domhoff 2009).

MO: I was just wondering if, because of the way you describe the Bohemian Club, it’s a lot more open than I personally expected it to be. I thought it was a little bit close to what you’re describing because I’m also doing some ethnographic work in Morocco on some clubs and there are kinds of different clubs. There’s the Sun Club, which is much closer than other kinds of clubs. When I started working on the bourgeoisie in Morocco, I found out that the most important things don’t happen in the clubs, but in the parties in some houses. Did you have access to these meetings, more closed clubs, whether in the West or the East, much at least these kinds of informal meetings that take place in houses or in closed areas like marriages? Or at least did the other people talk to you about it if you didn’t manage to get in?

GWD: I think most American clubs are closed in just like the way you described private clubs on the basis of your research, Mohamed. On the other hand, I don’t know of any examples of regular meetings at private homes by corporate leaders in the United States. There may be a few, but I am not aware of them. And it may be that there are variations from country to country on this and many other questions.

From what I have learned, I think that important informal meetings in the United States often occur in private rooms in the main downtown social clubs in the United States. These rooms can be reserved by members a week or so in

advance for holding these meetings. One of the best examples we have concerns the Links Club in New York, thanks to a retired corporate lawyer who was willing to talk candidly about the origins of the Business Roundtable. This retired corporate lawyer told me that the 35-40 men who worked on the formation of the Business Roundtable in the late 1960s and early 1970s were known informally as the “Links Group,” which refers to the fact that they met in one of the private rooms at the Links Club (Domhoff 2013: 186). More generally, this example fits with Baltzell’s (1964: 371) finding that the Links Club is “the New York rendezvous of the national corporate establishment.”

So, yes, as Mohamed’s question suggests, the Bohemian Club is a little different from most exclusive social clubs in the United States. I think that’s because it includes the associate members I mentioned. If corporate members of the Bohemian Club wanted to hold a private meeting with other corporate leaders in San Francisco, they would reserve a room at the more exclusive Pacific Union Club I mentioned earlier, which is a few blocks up a steep San Francisco hill from where the Bohemian Club is located. It is housed in a large mansion once owned by one of the richest men in San Francisco. Pictures displaying the building’s regal elegance and manicured green lawn can be seen on the Wikipedia entry on the Pacific Union Club and its history.

CC: In practice, did you have any funding to support your research? You were in a psychology department, weren’t you? Was this an advantage or a limitation for you? Were you able to get funding for your research or was it difficult? Did you just have to work with your own resources?

GWD: Funding was never an issue for me for a variety of reasons I’ll briefly explain, so I never bothered about grants. Grants were not as big an issue as they became later, and not an issue at all for me during the three years I taught at the state college in Los Angeles, which was overwhelmingly an undergraduate campus, except for a small M.A. program in psychology. As far as my time at UC Santa Cruz, the campus only had about 800 undergraduates when we opened in September, 1965. It grew very gradually, and we did not have a Ph.D. program in sociology until the middle of the 1970s, and that graduate program was small at first. As far as my being in the psychology department, that didn’t matter because the faculty in the social sciences in general was fairly small in the first few years when the campus was just getting started, and then I joined the department of sociology in the mid-1970s.

So, at first, I did most of my research on my own, but I also had many excellent undergraduate research assistants over the first 20-25 years of my research, mostly because the 1960s and 1970s were a time when students were more likely to be interested in the topic of power structures than they are now. In fact, I devoted my course on social psychology at Cal State Los Angeles almost exclusively to power in the Spring semester of 1965, and then taught a course focused on the American power structure in the Spring quarter of 1966 at UC Santa Cruz. Over a dozen of the students in those two classes did excellent original research that I could build on, and their work is cited in detail in the Notes at the end of *Who Rules America?* By 1966 I also had undergraduate research assistants who were paid out of small faculty research grants given to me by the campus Academic Senate, or else those students had government financial support to be research assistants on topics of their own choosing. They had these

government grants because they were from middle-income or low-income families. Several of my research assistants later went to graduate school or law school, and two or three became professors.

In the mid-1990s, just as I was preparing to take an early retirement at age 57 so I could concentrate more fully on my research, a student with outstanding computers skills showed an interest in working for me. His spreadsheets and statistical analyses became essential to my research, and he created the website at *whorulesamerica.net* to which we are still adding new information. We have collaborated ever since, even while he pursued an independent career as a graphic designer and wilderness photographer. As you can tell, I could talk forever about how much the work of each of my student assistants contributed to my understanding of the American power structure, including the interesting new ideas they came up with as they became involved in the research. In any case, the history of my research support, and my thanks to each of the students who helped me, can be traced by reading through the Acknowledgements at the beginning of each of my books.

Later, I had the good fortune to work with two sociology graduate students whose original work was very useful for my own purposes as well as theirs, and they both went on to be professors. And later on, I wrote books with both of them (Gendron and Domhoff 2009, Domhoff and Webber 2011). I was also very lucky to come to know a graduate student in social psychology on our campus, Richard L. (Richie) Zweigenhaft, whose interest in power and diversity led to a 50-year collaboration that resulted in four different books, two of which had multiple editions. Richie is an excellent interviewer due to his low-key and non-threatening style, and people are very willing to tell him their stories.

Richie also knows the relevant literature in social psychology far better than I do, and that gave our work together an added dimension. In addition, he liked organizing and analyzing numerical data as much as I did. Thanks to that collaboration, we were able to bring the issues of ethnicity, race, gender, and diversity into work on the power structure, which highlighted some of the divisions and tensions within the power elite. Doing this kind of work also gave us another window into the whole corporate community in general. Initially, we wrote on the highly sensitive issue of *Jews in the Protestant Establishment* (1982), which reveals some very real differences of opinion on social issues. Specifically, we were obviously signaling through the title of the book that we were picking up on the work Baltzell had done in *The Protestant Establishment* (1964) concerning the rampant anti-Semitism in the corporate community in that era, which has by now dissipated to a considerable degree.

Next, we broadened our approach by doing a study of *Blacks in the White Establishment?* which we updated with new interviews about 10 years later and gave it a slightly different title (Zweigenhaft and Domhoff 1991, 2003). After that came *Diversity in the Power Elite* in 1998, and we later updated it in 2006 and 2018 (Zweigenhaft and Domhoff 2018). That book widened our horizons by including the perspectives and backgrounds of women who served on corporate boards, as well as African Americans, Latinos, Asian Americans, and gays and lesbians. Finally, we wrote a book on the 109 CEOs of *Fortune* 500 companies between 1983 and early 2014 who were not white men. We found that the trend toward increasing CEO appointments for all previously excluded groups had peaked by about 2010-2012 at a very low percentage of all CEOs, and then declined for most groups, except for further increases for white women

(Zweigenhaft and Domhoff 2014). I have mentioned the titles of these diversity books to illustrate that my work on the power structure has not been one-dimensional, and to highlight the usefulness of collaborations that involve people with differing areas of expertise. Zweigenhaft (2017) has provided his perspective on the results of our collaboration in a chapter in a book that he co-edited on the pleasures and benefits, as well as the trials and tribulations, of carrying out co-authored research projects in the social sciences.

All that said, I realize that things have changed since I was doing this kind of research, and that one of the several factors in the decline in interest in power structure research involves the greater importance of outside funding. I know from colleagues that there's both an explicit and implicit pressure to obtain grant money. The pressure is explicit because big grants provide necessary resources for research. Grants also bring in overhead money that goes to the campus administration, and bring status to the university. And there's implicit pressure because it adds to your prestige to have a grant from a big foundation or a government agency.

For me, though, grants were not an issue because I was lucky enough to have enthusiastic and highly motivated students while teaching on a University of California campus that was focused in that era on undergraduate education, as well as to have brilliant co-authors.

CC: What do you think about the latest trends in this area of research and perhaps the use of big data from large commercial databases? Are you optimistic about this area of research or would you like to warn researchers? For example, the work of Eelke Heemskerk, about very large databases, there seems to be some successful research in this area.

GWD: I'm familiar with Eelke Heemskerk's work and I met him at the annual meetings of the International Network for Social Network Analysis in Anaheim, California, in 2016. I liked him and his mentor, the late Meindert Fennema, very much. I also think very highly of their co-authored article showing that resource-dependency theory cannot explain interlocking directorates, which is a liberating conclusion that fits with my longstanding emphasis on the role of interlocks in furthering social cohesion and consensus (Fennema and Heemskerk 2017). In addition, Heemskerk's very valuable recent co-authored article shows that at least 200 organizations are necessary for reliable network results. It also establishes that 250 organizations are sufficient for very good results, which provides everyone with a necessary starting point (Huijzer and Heemskerk 2021).

However, I still believe that network researchers who are interested in the issue of corporate power should use network analyses primarily as a starting point, and then make use of other research methods as well. I also think that power structure researchers who make use of network analysis need to include the policy-oriented nonprofits in their corporate network studies.

In that regard, a recent article by sociologist Tom Mills at Aston University in Birmingham, England, which I had the pleasure of co-authoring, has shown how important such studies can be. We carried out a network analysis of two different datasets that included the top 250 corporations (50 financials and 200 non-financials) and six major policy-oriented nonprofits. One dataset was for 1935-1936 and the other for 2010-2011. Using several different network measures

and descriptive statistics, our article demonstrates that the policy-oriented nonprofits are fully included in the overall main components of the two datasets. (We call the main components for each year “the corporate community.”) Among several things, our research establishes that corporate leaders have been involved in policy planning—and quite successfully, I should add—for at least the past 87 years. The article appeared in late 2023 in *Theory and Society* with the title “The Policy-Planning Capacity of the American Corporate Community” and it is published online with open access (Mills and Domhoff 2023).

Even though Mills and I emphasize our quantitative findings, in the Conclusion of the article we stress that other types of methods should be used in studying the work of the policy-oriented nonprofits. If sociologists are going to help us understand unfolding events over the next few years, Mills and I claim that they also should focus on one or another of these groups, such as the Business Roundtable or the Council on Foreign Relations. We suggest doing quantitative content analyses of their written reports and policy recommendations, as well as tracking the for-or-against testimony their members provide to Congressional committees on relevant legislative matters. We also recommend periodically interviewing their leaders and hired policy experts. Who knows, researchers might be able to find some informants if they have the patience to be persistent, especially if they keep an eye out for employees who quit, retire, or get fired from their jobs for one reason or another. Researchers should also realize that potential informants are usually willing to talk with them, especially if the conversations begin informally and remain anonymous.

In addition, it would be very useful for corporate network analysts to interview a large sample of corporate directors to see how well they know each other, and how often they communicate with each other on policy issues. The very great, but unfortunately marginalized and forgotten pioneer power structure researcher, sociologist Floyd Hunter, did just that in the middle of the 1950s. His results were illuminating, and he displayed them in the form of sociograms, which were forerunners of modern network analysis (Hunter 1958). Floyd Hunter was ahead of his time, but pluralists of the 1950s and 1960s derided his research, claiming it was based on “opinion” and “hearsay.” In their view, all Hunter did was to establish who had good “reputations.” This critique of his interview-based research was led by Dahl (1958) and his students (e.g., Polsby 1980, Wolfinger 1960), and they were highly successful in diminishing the impact of his work. As result, a very valuable literature, mostly focused on the local level, was lost from sight. In fact, Hunter and those who followed in his footsteps in studying power at the local level were forerunners of present-day network-oriented researchers, even though their networks were based on interview data about who works with whom on different policy issues.

MO: I have a question about your colleagues, about your relationship with the Stony Brook group, like Michael Schwartz and Beth Mintz, who worked on power elites in the United States (Mintz and Schwartz 1985). What was the discussion and exchange of ideas like in the 1980s and 1990s, and what is your relationship with the new researchers like Joshua Murray and Tarun Banerjee?

GWD: I think I first came to know and interact regularly with Michael Schwartz in the early 1970s. I also knew some of his early students quite well, especially Beth Mintz and Mark Mizruchi, each of whom went on to a highly

successful career. More recently, I came to know Tarun Banerjee and Josh Murray through seeing them at meetings of the American Sociological Association. The initial occasion for my becoming friends with Schwartz, which paved the way for all my subsequent interactions with him and his students, was a small informal conference that he organized in the spring of 1971, if I remember correctly. It was also at that gathering that I met and learned from sociologist Ron Breiger, then a graduate student at Harvard.

Breiger helped us to conceptualize our empirical data on individuals and their organizational affiliations as a “two-mode” network. Or, as he put it in the title of his seminal article published three years later, the kind of matrices I was using are “a duality of persons and groups” from the perspective of network analysis (Breiger 1974). That phrase practically became my mantra. As far as I was concerned, his work situated all the work I had done up until that time within the new and more rigorous realm of network analysis. I came to think of the power structure as a network of people and organizations/institutions, and I put that framework into action in a study of social clubs, corporations, and policy-oriented nonprofits, which showed the considerable overlap of members in all three types of organizations (Domhoff 1975). The findings in that 1975 article, which ranked the Links Club No. 4 and the Bohemian Club No. 12 on eigenvector centrality in a network of 31 social clubs and policy-oriented nonprofits, are now incorporated into the 2021 pdf version of the Bohemian Grove book. The results also have been confirmed in subsequent research that makes use of a longitudinal dataset that covers several decades (Barnes 2017).

Then another one of my superb undergraduate research assistants, Hal Salzman, who went on to earn a Ph.D. in sociology at Brandeis and is now a professor at Rutgers, cleaned, augmented, and updated a database I had obtained from a political scientist. The result was an independent corroboration of my earlier findings using a different dataset for a different year (1970) (Salzman and Domhoff 1983). And it was one of Michael Schwartz’ students, Mark Mizruchi, who helped us in analyzing the data using an algorithm for determining eigenvector centrality.

Although I and others worked very well with Schwartz and his students over several decades based on our common interests in corporate networks, we did have major theoretical differences. However, they always remained below the surface and caused no conflicts. Schwartz, for instance, was a strong Marxist, and had an interest in Marxian theories of finance capital, which led to his focus on banks as the most important organizations in the corporate network. In the case of Mizruchi, though, a retrospect he wrote on the first 20-25 years of his academic career makes it clear that his views about Marxism were more mixed in the early 1980s than many of us had realized. However, he did share a strong interest in commercial banks with Schwartz as the glue that held the corporate network together in terms of policy cohesion (Mizruchi 2003). Mizruchi also had an affinity for resource-dependency theory, which could be used to supplement Marxism in explaining why banks were important within the corporate community, as he later told Fennema and Heemskerk (2017), who cite at length from an email he sent to them.

Thus, when the commercial banks became less central in the corporate network in the late 1990s and early 2000s, Mizruchi decided that the corporate community was becoming more fractured. He further concluded that the top leaders could no longer be a force for moderate reform, as they sometimes had

been in the decades after World War II. Since he thought the looming crises facing the United States, and the world more generally, were so large that there was not enough time to develop a strong social movement for reform, he urged what he calls “the corporate elite” to “exercise some enlightened self-interest in the present” and thereby to “save the world as we know it” (Mizruchi 2013: 286). These and other views he presented in his 2013 book were not widely agreed upon by power structure researchers, whether they were Marxists or not.

I wrote a detailed critique of most of the claims in Mizruchi’s book (Domhoff 2015). I had never thought that banks had a central power role, and I had never agreed that the mainstream organizational theorists were right about the importance of resource dependency as the key to understanding interlocking directorships. Most of all, I thought that studies of policy-discussion groups had led us to an understanding of why the corporate leaders that I called moderate conservatives had shifted in a rightward policy direction on some issues beginning in the late 1960s and continuing ever since. This shift brought them closer to the views of the ultraconservatives in the corporate community, who had often opposed the moderate conservatives in earlier eras, but usually lost (Domhoff 2013, 2020). It is this shift by the moderate conservatives in the corporate community, incidentally, that is wrongly called “neo-liberalism” by the Marxists and ex-Marxists who blamed liberals and Democrats for the cutbacks in social spending and business regulation, and thereby overlooked the reasons why the moderate conservatives had changed their views (Domhoff 2020).

Moreover, I am not alone in raising questions about alleged fracturing in the corporate community. Tarun Banerjee and Josh Murray, the two recent Schwartz students Mohamed asked about, have presented evidence that does not support the fractured-elite view (e.g., Banerjee and Murray 2021, Murray and Jordan 2019).

Within this context, I would further note that the recent work that Tom Mills and I did on the centrality of policy-oriented nonprofits reveals that Mizruchi’s concern with the decentering of large commercial banks is irrelevant (Mills and Domhoff 2023). Fractured-elite theorists therefore ought to devote more attention to the right turn the corporate community has been taking over the past 55 years, despite the fact that commercial banks remained at or near the center of the corporate network until the past 25-30 of those 55 years.

MO: You talked about Nicos Poulantzas and Communists, how about Pierre Bourdieu?

GWD: My theory was fully developed when I first learned in the early 1980s about Bourdieu’s work related to power, so he did not have any major impact on my work. I also had by then decided that the complex social relations that workers and low-income people have with corporate leaders within the context of the highly individualistic American ideology had been well explicated in thoroughly researched books that had appeared in the early 1970s, such as *Blaming the Victim* (Ryan 1971) and *The Hidden Injuries of Class* (Sennett and Cobb 1973).

However, due to my longstanding emphasis on the social psychology of the upper class in terms of creating a sense of well-deserved entitlement, and an implicit demand for deference from working people, I did find his ideas related to the three forms of capital to be very useful. I cited one good overview he

wrote on that topic when I discussed the numerous ways in which structural economic power is turned into cultural capital, which I call “status power,” and also when I discussed the importance of being “well-connected” and having good “contacts,” which is how the concept of social capital is expressed in the everyday vernacular in the United States (Bourdieu 1986). Most recently, I used the 1986 citation to Bourdieu’s overview of the three forms of capital in the context of explaining how “polished” and “classy” former President Barack Obama is, and therefore how impressive he can be to both wealthy and non-wealthy people. I think Obama’s bearing and manner are due to the eight years he spent at one of the finest prep schools in the United States, that is, from the fifth through the twelfth grade. Moreover, he was able to attend this exclusive and very expensive school because of the social capital his grandfather had accrued due to serving as the manager of a furniture store owned by a wealthy man who was a strong supporter of that school (Domhoff 2023: 65-66). And Obama went to Harvard Law School, of course.

Although I think there are major differences between France and the United States due to their very different histories, I nonetheless think I overlap with Bourdieu to at least some extent in emphasizing the complex social-psychological relationships between the corporate leaders and those with little or no economic capital. That overlap also includes the complex relationship that the corporate leaders have with experts and intellectuals, which Bourdieu did a prodigious amount of work in explicating. And his overall impact on political sociology has been very large in that regard (Swartz 2013).

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