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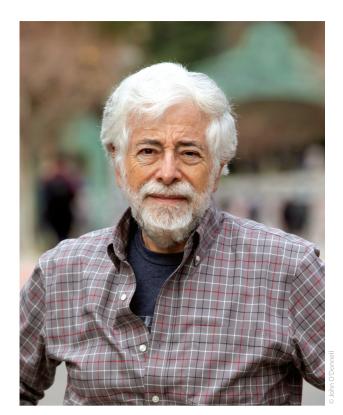https://escholarship.org/uc/item/3r1764hz

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Of Modernity and Public Sociology: Reflections on a Career So Far

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Abstract

My research so far has followed an interest in the classic concern about the social consequences of modernization, which led me to study urbanism, personal networks, the history of technology, and most extensively, American social history. A commitment to public sociology led me to a book on inequality, *Contexts* magazine, contributions to general media, a blog, and badgering sociologists about their writing. Some consistent themes include trying to address big questions with middle-range empirical work, focusing on ordinary lives and living, insisting on rigorous evidence whatever the method, and communicating with as wide an audience as lucidly as possible. The article closes with a few lessons learned.

INTRODUCTION

Sociology's founders were centrally concerned about how structural modernization in the West—mass production, rapid technological change, speeded-up travel and communications, urbanization, bureaucracy, a growing bourgeoisie, and connected developments—was altering community and mentality, how it was creating modernity. Durkheim, Weber, Marx, Tonnies, Simmel, and their American heirs such as Park and Parsons all explicitly or implicitly contrasted the world they saw immediately around them to a "world we have lost" (Laslett 1971), a world known to them largely through travelers' accounts of non-Western cultures and hazy recollections of an earlier, folk Europe. I have pursued this question for most of 50 years through seemingly disparate topics such as urban social psychology, network analysis, the history of the telephone, religious affiliation, and consumption patterns. My interpretations of modernity largely differ from those of sociology's founding fathers.

I thank the *Annual Review of Sociology* (*ARS*) Editorial Committee for providing this opportunity to reflect on my career in sociology. I focus on my pursuit of the modernity question and then address my involvement in public sociology. (Readers who might be interested in a more personal memoir can turn to a brief addendum at the end.) Through reflections for this exercise, I found a few continuities across my work: a preference for Mertonian middle-range theory (Merton 1968) over grand, all-encompassing philosophies; an insistence on robust measures over impressions, deductions, and wishes; a focus on the quotidian experiences of average individuals; and an assumption that, barring evidence otherwise, people act autonomously and reasonably in response to their circumstances rather than as cultural dopes.

THE PERSONAL IMPLICATIONS OF MODERNIZATION

The modernity question not only was central to much of the sociology I learned in the 1960s, it also underlay much of American literature in the 1950s and 1960s. Works on mass society and alienation—ranging from Reisman et al.'s (1950) *The Lonely Crowd* and Whyte's (1956) *The Organization Man*, to critical essays based on rediscovery of early Marx, to novels such as *Catcher in the Rye*—were everywhere. They made me wonder, Had modernization really created alienation from others and from one's true self?[1]

Urban Sociology

My first approach to an answer followed the path that many sociologists had followed: to assume that urban is to rural as today is to the past. In papers emerging from my dissertation in the early 1970s (including one in the first volume of this journal, Fischer 1975a), I tried to make explicit the classical theory that there is a distinct urban way of life (Wirth 1938) and, scavenging several existing general surveys, to test whether there was an independent association between size of place and various indicators of alienation. Although the answer generally was "no," the patterns were more nuanced. Other differences held constant, urbanites were not more estranged or anomic than rural people. Notably, however, urbanites were less traditional in their values than rural residents. Also, many city-dwellers would have preferred to live in smaller places, sharing, to some degree, a view of city life as threatening. (Importantly, much of the data was from a specific period of urban crisis for the United States, roughly the last third of the twentieth century.) Trying to make sense of my and others' results led to the subcultural theory of urbanism (Fischer 1975b, 1995). I argued

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[1] University of California, Los Angeles (UCLA) sociologist Melvin Seeman introduced me to empirical studies of alienation in the mid-1960s. A vain effort to bridge that literature and the Marxist literature on alienation appears as Fischer (1976a).

that, although the standard, deterministic model of how city life affected social ties and mentality is probably wrong, concentrating population nonetheless has important consequences.

Population concentration permits small cultural groups—ethnic, occupational, lifestyle, whatever—to attain critical masses sufficient to produce defining institutions, internal networks, and solidarity. (One can hear echoes of Durkheim's dynamic density.) These emergent subcultures and their encounters, both of conflict and of mutual influence, make city life different. The production of subcultures explains why cities generate much organized crime and much organized ballet, many gay bars and many executive suites. They also explain why public life in cities seems particularly alienating while residents' private lives are not (Fischer 1981). People who have supportive friends and cohesive families nonetheless meet discomfortingly different others when they step into public spaces. The varied subcultures that gratify cosmopolitans' taste for diversity also create feelings of disquiet and alienation for many others. I expanded on these arguments in my textbook, *The Urban Experience* (Fischer 1976b).

Though tempted to move on to other topics, I felt compelled to more thoroughly test subcultural theory. I turned to social network analysis, which in the 1970s was a relatively novel way to make concrete and measurable the abstraction "community." In preparation for future data collection, several Berkeley graduate students and I reanalyzed two existing surveys that had data on personal networks, notably Edward Laumann's (1973) 1965 Detroit Area Study. Our explorations resulted in *Networks and Places: Social Relations in the Urban Setting* (Fischer et al. 1977). Besides demonstrating the empirical payoff to network analysis, the book offered some conceptual developments: an argument—also made by others, such as Barry Wellman—that the network formulation helps instantiate and measure community; a choice-within-constraints framework for understanding how individuals form and sustain ties (it helps explain, for example, why people's most intimate ties tended to be their farthest—they maintain long-distance ties only if those people really matter); and a first sketch for a social history of changing patterns of personal connection and of community.

Networks and Places laid the groundwork for a grant application to the National Institute of Mental Health that, largely thanks to its director Elliot Liebow, led to the Northern California Community Study (NCCS) (Fischer 1977), which in turn resulted in *To Dwell Among Friends: Personal Networks in Town and City* (Fischer 1982). In the NCCS, Berkeley graduate students and I developed a survey instrument for measuring personal networks that relies on multiple name-eliciting questions to generate a list of the people important in respondents' lives. It asks, for example, for the names of people who would check on respondents' homes if they went away, whom they regularly confided in, and who had recently provided some practical help, and then asks respondents to describe those specific people in several ways. [For more information on the method, readers are directed to McCallister & Fischer (1978) and Fischer (1982, 2019).] Interviewers visited more than 1,000 residents living in 50 neighborhoods ranging from central San Francisco to small towns 200 miles in the hinterland.

The data permitted testing some ideas from subcultural theory about the correlates of urbanism as well as exploring general aspects of personal networks. The theory largely held up: Ceteris paribus, urban and small town residents seemed to have different styles of life but not different qualities of life. The small-town style was to rely heavily on family and neighbors, the big-city style to be more involved with nonkin and nonlocals and also, for those in demographically smaller groups, to be more involved with people like themselves (such as those seriously committed to a hobby or religious practice). Cities seem to encourage the availability of specialized subcultures. Also, the norms of public and private life were more distinct from one another the larger the place. These locational differences were in great measure, but not only, the result of residents selecting locations to fit their lifestyle preferences.

Network Analysis

While I deployed personal network analysis as a tool for examining community and social integration, many colleagues prefer to think of network analysis as "a perspective or a paradigm" (e.g., Marin & Wellman 2011, p. 22). Indeed, for some analysts, individualized networks form the essence of modernity (e.g., Castells 1996, Rainie & Wellman 2012). The tool we developed in the NCCS and the NCCS data themselves have helped other scholars address a variety of questions (e.g., Feld 1984, Blum 1985, Otani 1999, Grossetti 2007). Importantly, the method has been used, adapted, and improved by others (e.g., van der Poel 1993, Grossetti 2007).[2]

In the decades since we conducted the NCCS, "networks" has exploded as a topic of scholarly—and popular—interest. In the 1970s, I had to explain to the curious that networks did not refer to television broadcasters. Now, I have to explain that the term does not mean Facebook and Instagram. In between, the term was verbified into networking.

I moved on from network analysis after the NCCS but, almost by accident, embarked on another networks study in the early 2010s, the University of California, Berkeley Social Networks Study (UCNets), which is wrapping up as I write this article (see, e.g., Offer & Fischer 2018, Fischer & Bayham 2019).[3] The project seeks to understand the causal linkages among personal networks, life events and transitions, and health outcomes. I am especially interested in the dynamics of network change. We administered an adaptation of the NCCS instrument to a panel of about 1,000 San Francisco Bay Area respondents three times over a few years. Several articles have appeared and several more from the UCNets project should be in press by the time this article appears.

History of Technology: The Telephone

Addressing the historical problem of modernity by looking at the effects of urbanism today is, even at best, limited. Why not address an historical question historically? I started to do that in the 1980s by taking just one thread of the modernization story, the amplification of communication through new technologies. Having faster, cheaper, more pervasive communications is one dimension of structural modernity, and being able to communicate more easily with more people makes modern people's daily lives very different than those of people in the world we have lost. I focused in particular on the adoption of the telephone by ordinary Americans. Two features of the telephone made it an attractive subject: One, more than other modern technologies, telephony precisely captures interpersonal communication—dynamic density again. (Automobiles, in contrast, move people and goods; railroads are largely a production industry.) Two, there was little empirical work on the topic.

Combining a National Science Foundation grant with one from the National Endowment for the Humanities (the first to support statistical work, the second to scour narrative data, naturally), I spent a few years diving into the archives of the American telephone industry from its beginnings up to 1940. I looked in particular at how the industry marketed the technology; graduate students gathered data for case studies of three towns' experiences with the introduction of telephone service. This was an arduous project—I now appreciate why history books take so long to produce—but fun. That *America Calling: A Social History of the Telephone to 1940* (Fischer 1992)

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[2]I have reservations about the heavy reliance in the 1985 General Social Survey (GSS) and in countless surveys since on the "important matters" name-eliciting question, which Burt and Marsden derived from our instrument (Burt 1983, 1984). See Small's (2017, pp. 13–16) discussion of that history.

[3]The website is **http://ucnets.berkeley.edu/**; the data are available at **http://www.icpsr.umich.edu/icpsrweb/ICPSR/studies/36975**.

won the 1995 best book award from the Society for the History of Technology meant that I had passed my history exams.

Although the early telephone was described by many nineteenth-century commentators as what might later be called an alienating innovation (and greeted by others as a boon to mankind, but many of the latter were in the industry's pay), Americans were remarkably quick to absorb it into their mundane routines. They employed it as a way to handle practical tasks such as shopping, but also as a way of facilitating their personal ties—not as a way of replacing nor thinning those ties. The telephone quickly became a pervasive and unremarkable part of middle-class life, unlike, say, the automobile, about which controversy and conflict persisted for generations after its arrival.

At a theoretical level, I was struck by how deterministic much of the popular and of the scholarly writing on technology was (it still is). In this perspective, a technology impacts people, often by directly transferring its properties, such as its speed or mechanical impersonality, to its users. This made little sense. I argued instead for a user heuristic: "We should ask, Who adopted the device? With what intention? How did they use it? What role did it play in their lives? How did using it alter their lives?" (Fischer 1992, p. 17). The historical evidence showed that overall, people used the telephone to pursue the same sorts of personal activities and social relationships they had pursued without it, only more so—women calling to check on their relatives,[4] friends gossiping, young people arranging dates, and so on. It was more of a technology of sociability than of alienation. At the community level, the telephone, like the interurban trolleys and then the car, made it easier for Americans to spread out geographically if they wished to. Thus, telephone was not a social force but a social affordance (Greeno 1994, Wellman et al. 2003) that expanded users' options.

I left the history of technology after *America Calling* but get called back to it once in a while, particularly to comment on the newest communications technologies, for example, in the debate about whether smartphones were making people lonely. Answer: No.[5] Indeed, to the extent that the Internet repeats telephone history, it is also a technology of sociability rather than a substitute for so-called real social ties (Fischer 1988a).

American Social History

Enough warm up; enough modest goals. In the early 1990s, I undertook to marshal work in American social history to address the modernity question in America. This was a project of many years, and I am fortunate that when Berkeley asks its faculty, "What have you done for us lately?," "lately" is measured in years. For a sociologist to write a history book posed a professional risk; the product might interest neither historians nor sociologists. But taking intellectual risks is what tenure is for. One of my first steps was to review the 2,600-page *Encyclopedia of American Social History* (Fischer 1994).

As I was toiling away in the field of social history... Wait! Wrong metaphor. Historians tell interesting stories and tell them well. As I was gathering bouquets in the garden of social history, I twice detoured into side projects. One, discussed below, was a collective book on inequality that responded to *The Bell Curve* (Fischer et al. 1996). The other detour was more on topic. My then-Berkeley colleague Michael Hout invited me to join him in proposing a book project on the 2000 census to the Russell Sage Foundation. We suggested framing the 2000 findings in the context of social change across the entire twentieth century. With the help of Berkeley Demography students,

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[4]One finding was that a predictor of early adoption of the telephone is the number of adult women in the household (Fischer 1988b).

[5]These interventions largely appear in my blogging, for example, here: **http://bostonreview.net/blog/ claude-fischer-sherry-turkle-smart-phone-social-communication**.

we compiled the census and supplementary data needed to tell the story of *A Century of Difference: How America Changed in the Last One Hundred Years* (Fischer & Hout 2006). We laid out developments over the twentieth century in a variety of realms—fertility, urbanization, jobs, religion, family structure, and racial attitudes, among others. One overarching trend we described was the transition away from an America much riven by regional, racial, and gender differences—though remaining, these differences became less critical—to one more critically divided by education and income.

From *Century of Difference*, Mike and I spun off a paper on the rise of the religious "Nones" that would go viral. From about 1990 on, American survey respondents increasingly chose the answer "none" when asked which of several options described their religious preference, roughly doubling from about 7% to about 14% circa 2000 (and roughly doubling again to the present; Hout & Fischer 2002, 2014).[6] Our key explanation then and now is that liberal-leaning respondents increasingly refused to claim a religious identity as a symbolic protest against right-wing politicization of religion in America's culture wars. ("If being antigay, antifeminist, and antisex is what religion means," they seem to be tacitly saying, "then count me out.") Other researchers, such as Putnam & Campbell (2010) and those at the Pew Research Center (2012), have augmented the evidence and the explanation. The Nones phenomenon has gotten considerable attention in the media and in religious communities. Strikingly, nowadays political affiliation may determine Americans' religious affiliations more than their religious affiliations determine their politics (Fischer 2018b).

My survey of US history, *Made in America: A Social History of American Culture and Character*, finally appeared in 2010. In about 250 pages of text (and about the same number of dense notes and reference pages—to University of Chicago Press editor Doug Mitchell's bemusement, I'm sure), it recounts the experiences of Americans over three centuries thematically, with chapters on security, goods, groups, public spaces, and mentality. On the modernity question, the book argues that the consolidation over the centuries of physical and economic security provided more Americans with more of the personal independence embodied in the American ideal of the sovereign individual. The Americans whom the Founding Fathers could imagine as bearers of democratic "competency" (Vickers 1990) were, in effect, only propertied Protestant white male heads of households. All other Americans, from wives and children to hired hands and slaves, were these men's dependents and lacked competency. By the end of the twentieth century, however, almost all adult Americans exercised some autonomous competency—although certainly not equally so. Thus, more Americans participated more in the voluntarism that characterizes American society. Voluntarism I explained this way:

> The first key element of voluntarism [as a cultural system] is believing and behaving *as if* each person is a sovereign individual: unique, independent, self-reliant, self-governing, and ultimately self-responsible. . . . The second key element of voluntarism is believing and behaving *as if* individuals succeed through fellowship—not in egoistic isolation but in sustaining, voluntary communities. (Fischer 2010, p. 10)

Choice is the prime cultural directive here. People are supposed to join groups freely, and they are equally empowered to leave freely and leave guilt-free. Institutions such as markets, competitive status systems, and choice-protecting laws support the cultural system, minimizing forced membership and expecting individual responsibility. All this contrasts with so-called traditional society, with the world we have lost, in which individuals are organs of the group and bound to it. American culture also expects autonomous individuals to define themselves. Self-fashioning, an

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[6]Glenn (1987) reported a similar though much smaller trend over the 1960s and 1970s but detected an end to it in the 1980s.

eighteenth-century preoccupation of elites, became democratized and is now pursued in advice columns and self-improvement apps.

Regarding the classic concerns about lost community and alienation, this history claims that more Americans became more able to form personal ties and communities of choice rather than remaining in communities of ascription.[7] The chooser's freedom to sunder a tie is, of course, matched by the other's or the group's freedom to do the same. Thus, ties may be more chosen, more tailored to personal preference, but as well more tentative.

Made in America also advances claims about American exceptionalism. By this often abused term, I simply mean that American culture, society, and people are distinct from those of other Western nations—distinct in being much more voluntaristic as I used the term above. America is in this sense extreme or exceptional. A reader might well object that, if America is exceptional, how can I use its social history as a case study of modernity, a concern founded in the European experience? Good point. My reply is that despite American culture being on the voluntaristic fringe of Western culture, its historical trajectory largely parallels, with some exceptions (such as the absence of feudalism), that of other Western societies. America just went furthest.

The published reviews of *Made in America*, notably those by established historians, have been complimentary even as they observe that contemporary historians themselves avoid writing such sweeping thematic overviews.[8] Perhaps it is a case of a fool rushing in. . . .

While finishing *Made in America*, I engaged on a topic that joined social history and social networks: the claim that Americans' personal ties had weakened in the past generation or two. The main interlocutors were Robert Putnam (2001) with his blockbuster *Bowling Alone* and McPherson et al. (2006) with their *American Sociological Review* article claiming to find in GSS data that the proportion of Americans with no personal confidants had jumped sharply since the mid-1980s. I was skeptical of both claims, in part because they repeated long-familiar, periodic alarms going back at least to Reisman et al.'s (1950) *The Lonely Crowd* and Packard's (1972) *A Nation of Strangers*, but mostly because there was essentially no evidence in other surveys or in behavioral data to support them.

In *Still Connected: Family and Friends in America since 1970* (Fischer 2011), I reviewed and in several cases reanalyzed many polls, including the commercial survey that Putnam had used for *Bowling Alone*. I found remarkable consistency over 40 years in Americans' level of connection to others. There have been changes, to be sure: Because of declining birth rates, twenty-first-century Americans simply had fewer relatives to draw upon than did twentieth-century Americans, but they probably had more friends; they reported holding family meals and dinner parties at home less often but reported eating out with other people more often than did respondents a generation earlier; and contemporary Americans reported a greater volume of personal interactions by virtue of having electronic media in their communication toolboxes. There was no real evidence of increasing isolation or loneliness. I engaged in the public debate to clarify the scientific record and also "because [such false alarms] distract us from pressing, growing social problems, such as widening inequality, left-behind men of lower education, housing shortages, and children living in pockets of violence."[9]

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[7]Barry Wellman has described the rise of networked individualism somewhat similarly (Rainie & Wellman 2012), but this change developed generations before electronic networks.

[8]Excerpts from reviews can be found at **https://madeinamericathebook.wordpress.com/about/**. The book also won the American Publishers Award for Professional and Scholarly Excellence in United States History, 2010.

[9]This passage is from my letter to the editor of *The New York Times*, December 8, 2018 (**https://www.nytimes.com/2018/12/08/opinion/letters/loneliness-epidemic.html**), and another example is a *Boston Review* piece

PUBLIC SOCIOLOGY

Practicing public sociology, to me, means cogently explaining to the public what we have learned from robust research. It also entails conveying the broad sociological perspective: that the structural, institutional, and cultural contexts within which people act strongly shape how they act—a message especially important for an American public habituated to reductionist and essentialist ways of seeing and for a world public increasingly influenced by these American perspectives (see, e.g., Fourcade 2010). Thus, I chose the name *Contexts* for sociology's first public-facing magazine.

Doing public sociology is imperative at the very least because the public pays, directly or indirectly, for most of our work. As Herbert Gans said in his American Sociological Association (ASA) presidential address more than 30 years ago, we have a moral obligation to communicate honestly and clearly what we have discovered (Gans 1989). Today, this practice also entails "doing sociology in public" (Healy 2017, p. 771), that is, showing our homework on Internet media.

Second to teaching undergraduates, sociologists share what we have learned by responding to journalists. The interests of journalists and sociologists generally align—we both have stories to tell—but awkwardly. Journalists prefer shocking news—for example, that we are undergoing an epidemic of loneliness—when reality is usually much more prosaic (e.g., Fischer 2018a). Aside from writing a few essays for general media in the 1970s and 1980s, my first engagement with public sociology was being on an ASA committee that urged the association to beef up its media office and its efforts to serve journalists—which it subsequently did. The committee's work spun off a book (Hunter 1990); my chapter was "Entering Sociology into Public Discourse" (Fischer 1990). I fully entered a few years later.

Inequality by Design

In fall 1994, psychologist Richard Herrnstein and political commentator Charles Murray published *The Bell Curve*, which became public sociology of immense consequence. They argued that genetics explained social inequality and also argued, although not as baldly, that genetics explained why blacks and whites differed in their life outcomes. Propelled by a lavish PR budget, cover stories in two major magazines, and credulous journalists impressed by its many graphs and tables, *The Bell Curve* grabbed so much attention that it even prompted a question during a presidential news conference. On campuses, press reports about the book seemed to take black students aback; what the media called the latest science had reportedly confirmed age-old suspicions of their native competence. Among sociologists, the common reaction appeared to be shrugs of indifference— *The Bell Curve* is just foolish pop sociology and not even by sociologists—or simple dismissal. Those who dismissed the book largely asserted that "everyone knows" that the book's claims are wrong, with some critics suggesting that *The Bell Curve* demonstrated that a scientific sociology was impossible. What was missing in this tempest, at least to some of us, was a serious, reasoned, and research-based rebuttal to poor social science.

Several faculty members in the Berkeley Sociology department came together to write such a rebuttal: Michael Hout, Samuel Lucas, Martín Sanchez Jankowski, Ann Swidler, and Kim Voss, with myself as chair of the group. Several months of intensive work produced *Inequality by Design: Cracking the Bell Curve Myth* (*IBD*) (Fischer et al. 1996). We directly challenged *The Bell Curve*'s data analysis, showing in our reanalysis of the same survey data how much more important class background was to explaining who got ahead and who fell behind than Herrnstein and Murray had allowed. We challenged IQ-like tests as valid measures of a supposedly inborn

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(Fischer 2012b). Similarly, I have argued that the moral panic around smartphones privileges a bourgeois worry over the more critical problems of working people.

cognitive capacity. And we explained how social scientists understand racial differences in attainment to be socially constructed. At a deeper level, we sought to recast the question at issue from "What explains who wins and who loses in the race for success?" to more sociological ones, such as "How do the rules of the race affect outcomes?", "Where do the rules come from?", and "Why are the results for winning and losing more unequal in some times and places than in others?" In some societies and eras, losing the race means ill health, poor housing, psychological stress, stigma, early death, and so on, while in others the consequences are much milder. In some societies and eras winners are rewarded with multiple mansions, luxury cars, and tax-haven bank accounts, while in other societies they are rewarded with a modestly more comfortable life. Asking these structural questions leads one to examine the institutions that stage the race, that set the payoffs, and most especially, that determine the political decisions that in turn structure opportunity and inequality.

IBD gained modest media attention—some reviews in the general press, a few television appearances, a handful of public lectures, a human rights award. By the book's 1996 publication, however, many producers and editors seemed to feel that they had already covered the story two years earlier. And we were never able to get a debate with Murray. *IBD* has, nonetheless, become an important undergraduate textbook.

Contexts

In that 1988 presidential address on the "Discipline and the Public," Gans (1989, p. 8) suggested "an ASA-run or supervised magazine of high-quality popularized and pop sociology." Over the 1990s, members of the ASA Publications Committee, notably Paul Burstein, pushed to establish a general interest journal. Finally, around 1999, the executive committee of the ASA called for proposals to design such a general publication (P. Burstein, personal communication). Encouraged to apply, I proposed a magazine of sociologists writing for lay readers. The ASA accepted my proposal, although some ambiguity about the purpose and readership of the magazine remained.[10] "This . . . magazine," read the editor's note in the first issue, "initiates an exciting effort to bring compelling social research to the attention of a wide, new audience."

I considered *Contexts* to be an experiment testing the premises that sociologists know things that are of public interest, that clearly written sociology without an axe to grind can find a general readership, and that sociologists can write reasonably well. With the help of a professional journalist and several Berkeley graduate students, we ran a magazine-style operation, soliciting most of the articles, heavily editing or rewriting them, printing many images,[11] and trying for eye-catching layouts.

The experiment started off well in 2002. The articles were substantive and engaging; a few received general press coverage. A wholesaler picked up *Contexts* for distribution to newsstands to sit alongside other popular science magazines. *Contexts* even won two general publishing awards.[12]

Yet, as my term as editor closed in 2004, it became clear that *Contexts* would not be the sociological *Scientific American*. We lost the wholesaler and we did not break into the general media. Why? Perhaps the initial premises were simply wrong, for example, the premise of a general

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[10]The ASA announcement (Edwards 2001) called *Contexts* a new magazine but also stated that the "principal audience will be sociologists." My proposal clearly stated that sociologists were not the principal audience, that the general reading public was.

[11]Thanks to Jon Wagner's heroic labors in finding images gratis; we had no budget for them.

[12]The *Library Journal* named it one of the ten best new magazines of 2002, and the American Association of Publishers named it as the best social science journal of 2002.

audience. Gans himself ended his magazine suggestion by saying, "but that . . . is unrealistic since the current lay constituency for sociology is too small to support such a magazine" (Gans 1989, p. 8). Yet, today's proliferation of social science blogs and data journalism suggests that there is an audience. Perhaps the problem instead was launching a print magazine just as print was becoming obsolete (Healy 2017, p. 779). A completely different explanation is price: The ASA charged as much for *Contexts* as it did for its professional journals, about triple the cost of popular science magazines, thereby dooming the experiment. A few in the association argued that *Contexts* should be priced as a loss-leader for the profession, but others rejected the idea that one ASA publication should be favored over the others.

Contexts continues, to the credit of the editors who followed me. The NYU editors, for example, arranged to have a collection of articles published as an undergraduate text, now in its third edition (Cohen & Ali 2018) and the Minnesota editors created an online presence, which has flourished. Increasingly, however, the magazine has addressed sociologists and sociology students rather than the general reader; it has also increasingly preached to the choir on both perspectives and politics. These are legitimate changes, albeit not ones I would have made.[13]

Fortunately, in recent years, new developments have helped serve the purpose imagined for *Contexts*. Some sociologists have started using social media to inform readers of our research and our perspectives on topical issues (e.g., Philip Cohen, David Weakliem, Jay Livingston) and some media people have become talented data journalists (e.g., Kevin Drum, Nate Cohn).

More Directly

In the 2010s, I engaged in more personal public sociology. I started a blog, *Made in America: Notes on American Life from American History* (**https://madeinamericathebook.wordpress.com/**) that continued the themes of that book, bringing the findings of American social history to a wider public. The blog continues, albeit with a small readership, and over time the range of topics covered has expanded. For several years, I wrote reviews and then, from 2012 to 2016, a regular column—also titled "Made in America"—for the *Boston Review* with the same goal of entering social science into public discourse [**http://bostonreview.net/author/claude-s-fischer**; some of the early *Boston Review* essays are collected in a small format as Fischer (2014)]. This work has been rewarding because the general media occasionally pick up items from the essays and because it justifies my browsing widely in the social sciences. Occasionally I have used these opportunities to discuss my own research, for example, on the rise of the religious Nones, social isolation, and the implications of new communications technology. For the most part, however, I have been happy to be a reporter, perhaps a translator, of others' important, or at least intriguing, social science research. How successfully I can do that depends in part on how well I can write, which brings me to the next section.

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[13]Two illustrations are as follows: (*a*) A speaker at a session on public sociology at the 2019 ASA meetings proudly pointed out that *Contexts*'s original subtitle, "Understanding people in their social worlds" (which he mocked as meaningless), had been replaced by "Sociology for the public," because the public was now open to sociology. Perhaps it is (Hirschman 2018), but sociologists still trail other disciplines in having their views presented to key audiences (Wolfers 2015; an nGram analysis of book references shows much the same). I would still prefer a broader pitch. (*b*) Another speaker argued that it was better to preach to the choir because research shows that persuading the other side never works; only politically defeating it does. Besides my reluctance as a sociologist to take sides, I question the premise. Yes, Americans circa 2020 are heavily polarized on politics, but they are not much polarized on issues. [For instance, even on abortion, most Americans want to allow some but not all abortions to be legal (Pew Res. Cent. 2019).] I think many Americans would be open to learning more about important topics (whether politically contested or not), but they hear little from sociologists.

Writing

I have long been interested in writing itself, perhaps initially as a reaction to the sleep-inducing, confused sociology I had to read as a student. Robert Merton turbo-charged that interest when, as its series editor, he heavily corrected the manuscript for *The Urban Experience* (Fischer 1976b). Getting back draft pages drenched in the red ink of Merton's pen left me gasping, and revising the book took many extra months. But the rewriting experience was a great lesson from a master craftsman.

Sociologists can and should write clearly, and perhaps even entertainingly, for the general public. They can and should do the same for one another in professional venues. There is no justification for our typical obscurantism, jargon, and turgidness, even in the journals.[14] This concern led to my running a writing workshop with Berkeley graduate students for several years. One of the tasks students undertook was to reorganize their papers in ways that would persuade readers— "rhetoric" in the best sense of the term. Another was to edit each other's work and thus notice issues that they easily missed in their own writing. A task for me was to convince students that aping pompously nebulous continental *philosophes* was not the best route to professional success.

My concern with writing also led me to campaign against bad usage generally—largely to no avail. I hate discussions of causal influences as "impacts"; I banned "social capital" (except in ironic quotes) from *Contexts*; and I repeatedly warned about the grave risks in using metaphors (see Fischer 2012a). Though no exemplar, I think I write reasonably well *for a sociologist*—and therein lies the problem.

On Commitment

I am committed to this kind of public sociology. Others call for a public sociology that is more politically guided, arguing that we should do "research that matters in the first place" (Cohen 2018, p. 121) or that research should be the handmaid of activism. There is much to appreciate in issue-guided sociology, from investigations of tenement life a century ago to investigations of mobile trailer life today. I have done a bit of it myself (see above). Still, casting aspersions on research conducted just to pursue theory or to satisfy plain curiosity is unhelpful. Studying, say, migration routes of nomads in early China or the origins of bossa nova is as legitimate as studying topics in today's headlines. Moreover, basic research often leads to deeper understandings of society that are eventually helpful to activists and practitioners—for example, the 1960s explorations in social network analysis. What is incumbent on all sociologists is to share the discoveries of rigorous research.[15]

Too often, sociologists or sociologists-in-training address a topic confident that they already know what is happening and why, and that their work is just to document and publicize, when, in fact, they do not know. About ten years ago, I gave a lecture at a few sociology departments, including my own, on what American sociologists don't know about America. Focusing on historical trends, I pointed out, for one example, that the common view of modern Americans as increasingly rootless is 180° wrong because Americans are increasingly rooted (Fischer 2017) and,

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[14]I once had an exchange with Arthur Stinchcombe over a positive recommendation he gave a book which he admitted in the review to being so obscurely written as to need multiple readings. The failure to communicate, I argued, effectively negated any positive review.

[15]Contra Burawoy (2005, p. 5), our research findings are not the result of "translating common sense into science." As even brief reflection reveals, common sense is not only commonly superficial, simplistic, and self-serving; it is usually also contradictory. Duncan Watts has aptly pointed out that *Everything Is Obvious**: *Once You Know the Answer* (Watts 2011). Skepticism about the facticity of research unfortunately encourages some students to believe that anyone's opinion is as good as anyone else's in-depth scholarship.

for another example, that the notion of growing rationalism and secularism over the past several decades is also belied by the data.[16]

In the end, policy-makers and the reading public will disregard our claims and our prescriptions if they seem driven more by value preferences than by robust research. Any public sociology must be founded on sociology as a science (more on science below).

LESSONS

The *ARS* editors' invitation to write this article assumed that, with a half-century in harness, I have learned lessons that would be of value. Here are a few that might be.

I took what might be called a high-risk/high-payoff career path. I invested my time on big questions for extended periods each. Similarly, I shifted fields over the years—urban to networks to technology to history—in order to pursue an intellectual agenda. Each turn entailed a start-up period of learning a subdiscipline. The costs of this path include abandoning accumulated knowledge (what do I do now with my urban sociology collections?), forgoing the reputational rewards from earlier work (what did historians reviewing my grant applications care about my social networks research?), and dropping out of professional view within subfields (Fischer? You mean that networks book in the early 1980s? He's still alive?). The gamble paid off personally—I enjoyed moving to new fields—and professionally—I have been sufficiently rewarded, thank you. But a more strategic plan for pursuing professional advancement would probably be to go deeper and stay longer in one field.

Interdisciplinarity is a buzzword these days; actually, it has been for decades, especially for administrators. I took an interdisciplinary path, from experimental social psychology (Fischer 1969) to American history, but am skeptical of institutional arrangements intended to brew interdisciplinarity. For one, disciplines differ in ways that matter, from the motivating questions at their cores to the styles of their articles. For another, interdisciplinary units are usually the result of boundary-crossing developments rather than the origins of them. Such arrangements can be constraining, especially for the young scholars who would be the innovators. "When productive interdisciplinary work happens, it is usually between the ears of the scholar who pursued a research question into other disciplines or in the accidental collision of a couple of professors whose own pursuits lead them to cross paths. The breakthroughs are question-driven … [and] cannot be hot-housed in new, inevitably transient, departments of the Latest Next Thing" (Fischer 2013).

Working in a supportive environment makes it far more feasible to take interdisciplinary risks. Berkeley Sociology is such an environment. To be sure, no academic department is without divisions, stresses, and irritants. Berkeley Sociology had perhaps more than its share in the 1970s and early 1980s, but since then it has been congenial. Throughout, it has supported scholars willing to reach far. The faculty, it hardly needs noting, are superb, but probably more important over the years have been the graduate students. It is not only that we gain much by their collaboration, but we also learn much from them. As the work of Berkeley faculty and students over the decades demonstrates, the culture of the department and, to some extent, the structure of the university work toward boldness in scholarship over routine science.

Putting many years into sociology, as into other intellectual endeavors, means witnessing many fads and fashions. Topics surge, captivating a cohort or two of graduate students, and then fade. New methods, especially statistical techniques, appear as the long-sought answers to establishing validity and causality, and then are found to have their own difficulties. The years of experience

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[16]For instance, the percentage of Americans saying that they believe in life after death averaged about 77% in the 1970s but 81% in the 2010s (according to analysis of "postlife" variable in the 1972–2018 GSS).

make one appreciate the importance of the perennial topics such as inequality, race, power, and the modernity question I pursued. The years also make one appreciate the humble crosstab. Cutting-edge techniques boosted by the incredible speed of modern computing enable today's researchers to race immediately to final models, often missing the anomalies, complexities, and even coding errors in the data. I have frequently asked a research assistant to please run a simple two-by-two table first.

Finally, my predecessor in this space, James House, worried about "'the culminating crisis of sociology'—the essence of which has been and remains a lack of intellectual unity in the discipline, which increasingly paralyzes its ability to contribute [to] . . . improvement of human society" (House 2019, p. 3). I am more sanguine. That unity decades ago was the hegemonic control of the functionalist, Parsonian framework. Its castle was breached and sacked in the late 1960s. The resulting decentralization has mostly been to the good. And certainly the freedom to wander from any disciplinary center is one I have enjoyed.

I do have concerns, perhaps connected to his. Sociology understandably attracts people with strong moral commitments to social justice. For most, these commitments motivate high-quality scientific research. For some, however, they motivate intellectual shortcuts, presuming facts that are yet to be established or repeating familiar tropes. Take, as example, understanding crime. I can recall that about 40 years ago many sociologists dismissed rising crime rates as a media- and politically-induced mirage tinged with racial animus. The crime wave was real and was immensely damaging, especially to black residents of American cities. Denial did the profession no good. More recently, some have been quick to endorse the claim that Clinton's 1994 crime bill spurred our extreme rates of black incarceration; easily available data show otherwise (see Drum 2019). Any hopeful prescriptions for social change should be rooted in evidenced descriptions of reality.

Similarly, some sociologists seem satisfied to rediscover for the nth time familiar forms of class, or racial, or gender inequity. While usually true, these descriptions alone do little to advance the science (or, probably, the politics) that might help repair matters. Our studies need to go further. Further includes research that exposes previously unappreciated dynamics of inequality—for example, Alexes Harris's descriptions of the way fees and fines trap the poor in the criminal justice system and Matt Desmond's descriptions of the baleful ramifications of eviction. Further includes developing and supporting deep explanations of inequalities—for example, Doug Massey's accounts of the complex interactions among Mexican migratory patterns, American farm employment, and border enforcement, and also Doug McAdams's analysis of how sharecroppers' moves north, with the empowerment of black clergy and the rise of black colleges, presaged the development of the Civil Rights Movement.

Presuming facts without evidence and repeating the same simple tropes, whatever their moral impetus, encourages the public and policy-makers to dismiss sociology and sociologists' advice as partisan. They also invite others, notably economists, to colonize "our" topics, such as family structure, social mobility, and racial discrimination, with their own perspectives and prescriptions.

Sometimes correlated with such moral commitments, although for no logical reason, is resistance to the science in social science, a bit of which we saw around *The Bell Curve*.[17] My first peeve in this regard is the disparaging use of the terms "qualitative" and "quantitative." The distinction serves no legitimate purpose, obscures what matters, and should be banned. Anytime so-called

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[17]And what, some might ask, do I mean by "science"? Science, I maintained to several cohorts of graduate students in Berkeley's required logic of inquiry class, is simply the systematic collection of observations and the application of logic to explain them. More broadly, the collective scientific enterprise entails repeated sets of observations (for confirmation or disconfirmation) and the accumulation of such work toward more general understanding. I also argued that sociology was an historical life science, like paleontology or biology, certainly not a science like physics.

qualitative researchers write something like "most of the people I observed did *X*, a few did *Y*, and only one did *Z*," they are committing acts of quantitative sociology. The real distinction that matters is the kind of observations of human action that we use. Are the observations events that we directly see and hear? Or are they what people tell us about action? Or are they traces of action, such as documents or artifacts? Whether we then precisely tabulate the observations or summarize them verbally is of secondary interest. Another and related peeve is the over-application of the philosophy of science to doing empirical work. Whatever one might debate about the nature of reality, the importance of subjectivity, power dynamics in scholarship, and so on, in the end the empirical work, if it is to persuade any neutral reader, has to meet the standard criteria of evidence—reliability and validity—and of logic. (Students who proclaim critical approaches to sociology usually end up doing actual research not much different than confessed positivists do.)

House (2019) sees sociology falling behind other social sciences, notably economics, in influencing policy, which should be particularly galling to those who became sociologists in order to shape policy. I have no certain solutions, but my instinct is that, to compete in the marketplace of policy ideas, sociologists need to ask big questions about topics that concern the public, use all available tools, be open to surprising even if disappointing answers to those questions, and promote the findings in an accessible public sociology. I did not become a sociologist in order to shape policy, though some of my work may be relevant, but to sate my curiosity. That's OK, too.

ADDENDUM: PERSONAL HISTORY

Born in Paris in 1948, the first child of Nazi death-camp inmates, I arrived in the United States in 1952. (When we landed at Ellis Island, my mother concealed my fevered measles from the inspectors, making me an illegal alien, I suppose.) We lived in Paterson, New Jersey, where my parents labored in nearby garment factories. Our social circle was composed of other survivor families, an experience that bred in me a deep appreciation for the small triumphs of ordinary life: feeding guests, vacationing in modest motels, saving up for a first dining set, expecting a better future for the children. (All Americans should be so lucky.)

In the summer of 1963, as my family moved to Los Angeles, I participated in a National Science Foundation program at Arizona State University intended to seduce high school students into the sciences. My sociology course with John Kunkel worked. I went on to major in sociology at UCLA while being employed for three years in the Psychology Department as an undergraduate research assistant to social psychologist Harold H. Kelley, my mentor (Kelley et al. 1967). I spent most of my time, however, on the UCLA debate team.

In 1968 I began the PhD program in Harvard's interdisciplinary Social Relations Department, which, by the time I finished, had dissolved into its component parts. Although faculty superstars were plentiful in "Soc Rel," I learned most from my fellow students such as Ron Abeles, Paul Burstein, and Anne Peplau. The program was loose and my advisors accommodating, so I was allowed to wander off into urban social psychology. (At my dissertation prospectus meeting, Nat Glazer leaned over to Dan Bell and said, "Let the boy do what he wants.") In 1972, I joined the Berkeley Sociology Department, where I have, excepting occasional sabbaticals, remained since.

I met Ann Swidler in late 1971 at my Berkeley job interview (wherein a story lies). We married in 1977 and I have treated her ideas as community property ever since—as, she claims, she has mine. Two children and one grandchild have followed, so far.

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