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From Physics to Russian Studies and on into China Research: My Meandering Journey Toward Sociology

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Abstract

I was initially reluctant to become a sociologist because my father, William Foote Whyte, was a prominent sociologist. Growing up during the Cold War led me in sequence through studying physics to Russian studies and then into Chinese studies, and I finally chose sociology as the best discipline for research on China, the primary focus of my career. The theoretical and methodological eclecticism of our discipline enabled me to do research on many intriguing research problems regarding contemporary Chinese society, first at a distance from Hong Kong and then through a series of collaborative surveys conducted within China. I found I could use sociological theories to more accurately and objectively interpret Chinese social patterns, while at the same time some paradoxical features of Chinese society enabled me to challenge existing social science theories. Over the course of my career I was gratified to see the sociological study of China move from being a somewhat marginal specialty into joining the mainstream of our discipline.

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INTRODUCTION

It might be assumed because I am the son of a famous sociologist—William Foote Whyte, author of the classic work *Street Corner Society* (Whyte 1943) and much else and a former president of the American Sociological Association (1981)—that I absorbed a love for sociology by familial osmosis and was destined to follow in my father's footsteps. The reality is much more complicated, involving a complex sequence of efforts to find my own way in life and finally ending up somewhat reluctantly in the same field as my father.

Pop did serve as a positive role model, both intellectually and in terms of personal character and values, and from an early age I wanted to become a college professor. I could see the appeal of being a respected professional, exploring ideas and training students, not having to be in an office from 9 to 5 and report to a boss, and with the added possibility of traveling to interesting places and having summers off. The appeal was influenced by a deeper professorial genealogy. My paternal grandfather, John Whyte, was a professor of German at Brooklyn College, and my maternal grandfather, Clarence King, although trained as a lawyer, spent much of his career working in charitable organizations and late in life taught community development courses in the School of Social Work of Columbia University.¹ (As a result of this background, I have told students that I experienced zero intergenerational mobility.)

Pop majored in economics at Swarthmore and then was offered a junior fellowship in the Harvard Society of Fellows, then a post-bachelor's rather than a postdoctoral program. Pop used his three years affiliated with Harvard to live and conduct fieldwork in the Boston North End, the Italian immigrant neighborhood where he became friends with the youths portrayed in *Street Corner Society*. The Society of Fellows was dedicated then to interdisciplinary study and discouraged pursuit of a PhD in a "narrow" discipline. Pop had other ideas, and when he finished the fellowship he enrolled in the sociology PhD program at the University of Chicago, with a fully drafted doctoral thesis traveling to Chicago with him in a suitcase.

When he completed his PhD, my father got his first teaching job at the University of Oklahoma in 1942, and I was born shortly after my parents and sister Joyce moved to Norman. Our stay in Oklahoma was cut short when Pop contracted polio and had to spend more than a year rehabilitating. After Pop recovered he was offered a job back at the University of Chicago. I don't remember much about life in Hyde Park, because in 1948 my father took a new job in the School of Industrial and Labor Relations at Cornell, where he happily spent the rest of his academic career and his retirement years, until his death in 2000. Following advice from new colleagues, Pop decided we should live not in the "big city" of Ithaca (with less than 30,000 inhabitants then), but 11 miles away in the town of Trumansburg (with fewer than 2,000 residents), where he joined a carpool that commuted to Cornell. So from first to twelfth grade, I attended Trumansburg Central School, where my classmates were more likely to be children of farmers, auto mechanics, pharmacists, and insurance salesmen than the offspring of professors.

While I was growing up, Pop specialized in industrial sociology, and the things he was doing didn't appeal to me very much. Once he took me along when he was researching the Corning Glass Works, in a project that required him to spend hours near hot furnaces, bending over to interview glassblowers as they plied their trade. Later on we spent an enjoyable year living in Maracaibo, Venezuela, where Pop spent his sabbatical around greasy oil rigs of the Creole Petroleum Corporation, interviewing workers and managers there. Since I didn't bother to read *Street Corner Society* until after I went away to college, I didn't realize there were many other types of sociology that didn't require fieldwork in hot and dirty environments.

¹My academic lineage goes further back. My great-grandfather, Franklin Hiram King, who died in 1911, was a professor of "soil physics" at the University of Wisconsin, where King Hall is named in his honor.

SPUTNIK AND THE KENNEDY ASSASSINATION AS TURNING POINTS

Pop did not make any attempt to interest me in sociology. My WASP parents were firm believers in independence training, and they wanted to raise their children to find their own paths in life, as they had been raised.² Since I had always done well in mathematics and science classes, I pondered some sort of future scientific career. I was influenced by growing up during the Cold War, and particularly by the Soviet launching of Sputnik in 1957, beating the United States into space. Sputnik shook me and most Americans out of pride in the United States as the most advanced country on earth. I became aware that a year earlier, Soviet leader Nikita Khrushchev had bragged to a group of Western diplomats, “We will bury you!” The idea that the Union of Soviet Socialist Republics (USSR) might one day supplant the United States as world leader by employing a Communist Party dictatorship and a centrally planned socialist economy was profoundly troubling to me. I decided to study physics in college, feeling that this would suit my academic abilities and prepare me to contribute to American scientific competition with the USSR. I knew that Cornell had an excellent physics department, so I applied and was accepted in 1960.

At Cornell I had a series of first-rate physics teachers, including Nobel Prize winner Hans Bethe. I took a wide range of other courses, and early on I decided that where possible, I would enroll in courses on Russia and the Soviet Union to fulfill my liberal arts requirements. I started Russian language study early, commuting to Cornell for introductory Russian classes in the summer before the start of my freshman year. I later took courses on Russian history, Russian literature, and Soviet politics, and eventually I accumulated enough credits for a Russian studies minor. My parents encouraged my developing interests in both physics and Russia, and they paid for me to enroll in a summer language tour of the Soviet Union in 1963, with a program that took me to the University of Michigan for further Russian language preparation and then a month traveling around the USSR. I only took one sociology course at Cornell, Introductory Sociology, in my final semester as I coasted to graduation.

In my senior year I was beginning to wonder whether I had enough dedication and talent to excel in a career in physics. When President Kennedy was assassinated, my plans changed abruptly. I was in a laboratory on campus, staring into an oscilloscope and recording readings from the screen as part of a physics experiment, when someone rushed in with the tragic news. It soon became known that his assassin, Lee Harvey Oswald, had lived in the USSR and had married a Russian woman. I recall thinking, do I really want to spend my life taking readings from laboratory instruments when I could be doing research to better understand the Soviet Union? Although it was too late to change my major, I decided that instead of doing graduate work in physics, I would apply to MA programs in Russian studies, and after graduation I entered the program at Harvard. I planned to take more courses on the Soviet Union while also sampling graduate courses in several disciplines to find a good fit for later doctoral study.

KHRUSHCHEV’S OUSTER AND THE CULTURAL REVOLUTION AS FURTHER TURNING POINTS

Not long after I arrived at Harvard in 1964, Nikita Khrushchev was ousted as leader of the Soviet Union, with Leonid Brezhnev replacing him. Even though he had threatened to bury the

²My mother, Kathleen King Whyte, decided to enroll in art school rather than attend college. She became a calligrapher and book-jacket designer before, like most middle class women in her era, she diverted her energies to raising four children and supporting her breadwinning husband. [She designed the book-jacket for the original edition of *Street Corner Society*, was a consummate editor and critic of Pop’s later writings, and finally earned co-authorship of his final research project book, *Making Mondragon* (Whyte & Whyte 1988).]

United States (as well as sneaked missiles into Cuba), I admired Khrushchev for his courage in denouncing Stalin in 1956 and for signing the nuclear test ban treaty with President Kennedy in 1963. Brezhnev made the USSR boring for the 18 years he was in power. Because I had already taken many Russian studies courses, I decided to sign up for some courses on China, the other communist giant. I enrolled in my second sociology course, Ezra Vogel's undergraduate survey on contemporary China, and also courses on the history of Sino-Russian relations.

I found the idea of such an ancient civilization as China now striving to pursue communism intriguing, and my interest increased when the Cultural Revolution erupted in 1966. In launching the Cultural Revolution, Mao Zedong proclaimed that the Soviet Union was not much different from capitalist America and that he would lead China toward a more pure socialist future. He was starting that long march by encouraging high school and college students to rise up and attack the party officials who had been oppressing them. Mobilizing a mass movement of young Red Guards to attack the party itself contradicted everything I had learned about Leninist political systems, and suddenly China seemed a more exciting society to study than the Soviet Union. But I hadn't yet started studying Chinese, and I scrambled to make up this deficit by enrolling in another summer language program at Cornell to start learning Mandarin.

In the final year of my MA program, I still faced the problem of what discipline to study for a PhD. As I thought about this dilemma, sociology began to have some appeal. It dawned on me that if I wanted to do research to understand the institutions and social change patterns in either the Soviet Union or China, studying economics or political science would be too limiting, while sociology would allow me to study virtually any topic that interested me. I also ruled out anthropology, given the obstacles to Americans doing fieldwork in either the USSR or China. The methodological eclecticism of sociology, in contrast, seemed an advantage. The very traits of sociology that bother many of its practitioners—being too broad and balkanized into diverse specialties, with no intellectual core or agreement on what should be studied and how—became the things about sociology that seemed particularly suitable for preparing me to study the societies that interested me. That same diversity made me feel that I could conduct sociological research that would be very different from my father's work.

Given my belated consideration of seeking a PhD in sociology, I enrolled in Alex Inkeles's course on personality and social structure as well as the doctoral proseminars of what was then still the Social Relations Department. One seminar was taught jointly by social psychologist David McClelland and anthropologist John Whiting, both of whom were interested in how societies and national populations differ from one another.³ Given the reassurance that these stimulating courses provided, I applied to the graduate program in sociology at Harvard and was accepted.

BELATEDLY DISCOVERING THE ATTRACTIONS OF SOCIOLOGY

Although my rationalization for my choice was not based upon the intellectual appeal of sociology, I soon realized that the field offered much more than suitable preparation for research on the Soviet Union or China. The late 1960s were glorious days to be a sociology student at Harvard. The faculty included not only famous names such as Talcott Parsons and George Homans, but also future leaders of the field such as S.M. Lipset and Robert Bellah, prominent social psychologists such as Thomas Pettigrew and Stanley Milgram, a graduate training course on comparative sociology and cross-national research taught by Donald Warwick and Paul Hollander, and the

³ I was not aware of it at the time, but the Social Relations Department was in its last days, with the sociologists in particular eager to split off and form a Department of Sociology. The disintegration was completed by 1972.

ambitious six nation modernization survey project being conducted by Alex Inkeles (which provided research assistant jobs to me and many others). Only one statistics course was required, but it was an excellent one taught by Fred Mosteller, and I went on to study survey research methods with David Armor. I also continued to receive guidance for studying the Soviet Union from Paul Hollander and for studying China from Ezra Vogel.

I found myself excited by the courses and research activities I was surrounded by at Harvard. Since I was particularly interested in how societies such as the Soviet Union and China were attempting to develop economically and the challenges their development represented for the United States, the intellectual atmosphere was very congenial. This was the heyday of modernization theory and cross-national research, and many faculty members were engaged in work related to the sociology of development. Whether it was Bellah's work on how Japan had developed without a Weberian Protestant ethic; Inkeles's project on how education, urban living, and industrial work modernized the attitudes of former farmers in six developing countries; McClelland's research on the distribution of "need achievement" personalities in the Middle East; or Lipset's (1963) recent book on the United States, *The First New Nation*, many faculty members had things to teach me that might help me understand the USSR and China. Even Talcott Parsons had gotten into the act, becoming interested in evolutionary theories and publishing *Societies: Evolutionary and Comparative Perspectives* (Parsons 1966).

I became interested in issues that applied to a variety of societies, and not just to the USSR and China. These experiences led me to focus on comparative and historical sociology and to be most drawn toward macrosociology questions and theories—how and why do the social patterns of different populations, or of entire societies, vary and how and why do those patterns change over time? As a professor of sociology I would need to teach general sociology courses, and in preparing for my oral exams I focused on social stratification and the sociology of the family, both in comparative perspective. I also prepared for the future seminars I would teach on the sociology of development, in part by taking economist Dwight Perkins's course on the economic development of China and Japan.

After I decided to pursue a PhD in sociology, Pop tried not to display too openly how pleased he was, given our joint commitment to me finding my own path. But sometimes he just couldn't help himself. When I told him I had written a paper for Milgram's seminar on attitude change and social influence, he asked if he could read it "to learn new things." On a visit home while I was preparing for my written preliminary exam, Pop mentioned that he had kept the notes he had used to study for his own generals at Chicago a generation earlier, and would I like to use them to help prepare? He dug out some old pads with his handwritten notes, and he mentioned that these included summaries of the ideas of Gumplowicz (Ludwig, 1838–1909) and Ratzenhoffer (Gustav, 1842–1904). I thanked him but said I didn't think I would be needing his notes, since we didn't even have to know the works of Pitirim Sorokin, by then retired from Harvard although recently (1965) elected ASA president.⁴ Still, in later years our conversations mostly involved mundane topics like family events and baseball, and we did not often engage in sociological shop-talk.

As I was completing coursework, I remained torn about whether to plan a dissertation focusing on the Soviet Union or China. My grounding in Russian studies was much better, but China by then seemed a more exciting society to study (plus the food would be better). My indecision resulted in me planning two possible thesis projects and defending two different prospectuses. One

⁴I don't recall any writings by Sorokin on my reading lists, and I only saw him once, at an anti-Vietnam War protest in a Cambridge church in 1967 (he died the following year). We did, of course, have to know the works of Marx, Durkheim, and Weber, and that is still true today.

would take me to the Soviet Union to work on the problems of Russian villages. [One obstacle was that in 1966 I married Veronica (Ronnie) Mueller, and by then we had a son. If I was selected, Ronnie and Adam could not accompany me to the USSR.] The other project would take me to Hong Kong to interview refugees from China to explore how the regime's political indoctrination sessions worked in different organizational settings. I applied for an International Research and Exchange (IREX) fellowship for the Soviet project, and for a Foreign Area Fellowship (FAF) for the Hong Kong project, and I have been grateful ever since that IREX named me only an alternate, while I received the FAF.

I had already taught my first college course, an undergraduate survey of Soviet society at Boston University, and I had drafted a paper on the problems of Russian villages that became one of my first publications (Whyte 1970). But in later years I did not pursue further research or teach courses on Russia, although my writings on China have emphasized comparisons with the Soviet Union.

As I prepared to leave for Hong Kong in 1968, an unexpected opportunity arose. The Ford Foundation had developed a grant program to encourage universities to hire contemporary China specialists in underrepresented fields, which included economics and sociology. The Center for Chinese Studies at the University of Michigan had received a Ford grant, enabling them to pay half the salary of starting assistant professors in those fields. The Center started looking for a sociologist of China, and I benefited from the *guanxi* (personal connections) between two Alexes. Alex Eckstein was an economist who had worked on the USSR with Alex Inkeles at Harvard during the 1950s, but now he was at Michigan specializing on China. Eckstein contacted Inkeles and asked if he knew any sociology doctoral students working on China, and Inkeles recommended me. I was flown to Ann Arbor for interviews based solely on my thesis prospectus, and things went well enough that before I even departed for Hong Kong, I received an offer to join the sociology faculty at Michigan. Even more unusual was that the offer included funding for us to fly to Ann Arbor in 1969 so I could spend a year writing up my thesis, only starting teaching in 1970. I was surprised and grateful that Michigan was willing to gamble on me even before I started my dissertation research.

ON STUDYING THE PEOPLE'S REPUBLIC OF CHINA AT A DISTANCE

I was taking a gamble myself in deciding to become a sociologist of contemporary China. Despite the allure of studying the tumultuous changes taking place in the world's most populous nation, I faced some obvious problems. I had never been to China, and Americans were not able to visit until after President Nixon's 1972 visit (I made my first trip in 1973). Conducting research inside China did not become possible until diplomatic relations with the United States were established in 1979. Furthermore, sociology had been condemned as a "bourgeois pseudoscience" and abolished as a discipline in 1952.⁵ So at the time I started my thesis project, I couldn't be sure that I would even be able to visit China. It seemed very unlikely that I would ever be able to conduct fieldwork within the People's Republic of China (PRC), train students from that country, or collaborate with Chinese sociologists.

Despite these problems, I knew that a well-developed infrastructure had been established in Hong Kong to enable Westerners to conduct research on the PRC at a distance, and my first three China research projects were all conducted in Hong Kong [my thesis project in 1968–1969

⁵ Sociology had been abolished in the Soviet Union under Stalin, but after Khrushchev's 1956 secret speech the discipline was rehabilitated there. An attempt by Chinese trained in sociology to request a similar revival in their country was squelched by Mao, and it was not until after his 1976 death that sociology was rehabilitated in China (Whyte 2019a).

(published as Whyte 1974); research on social change in Chinese villages in 1973–1974; and a parallel project on social changes in Chinese cities in 1977–1978, the latter two involving collaboration with William Parish (Parish & Whyte 1978, Whyte & Parish 1984)]. During those projects I was based at the Universities Service Centre alongside other graduate students and professors who were studying the PRC at a distance.

My Hong Kong projects involved in-depth qualitative interviews (in Chinese) with recent refugees from China that were conducted over several three-hour interviewing sessions using an extensive topic outline. The interviewees were treated as ethnographic informants, not respondents in a survey, and the goal was to piece together from multiple snapshots of the small corners of Chinese society of each informant a more general picture of contemporary social life in the PRC. I didn't record the interviews but took detailed notes that I typed up after each session. Ezra Vogel had lived in Hong Kong a few years earlier while similarly interviewing refugees for his planned book on how the city of Canton (Guangzhou) and the surrounding province had been transformed under Chinese Communist Party (CCP) rule (Vogel 1969). Ezra generously invited me to read through his interview transcripts, and the many hours I spent working my way through his filing cabinets were essential preparation for my own project.

LAUNCHING MY ACADEMIC CAREER AT THE UNIVERSITY OF MICHIGAN

In the months after we flew back from Hong Kong to Ann Arbor, I became worried that Michigan's Department of Sociology might not be a good fit. The Michigan department was larger than Harvard's, with over 30 faculty members, and it was organized differently. I did not realize that demography was part of sociology, while at Michigan it was one of the three wings into which both the faculty and the graduate program were organized: population, social psychology, and social organization (i.e., everyone else, including me). Furthermore, Michigan was much more quantitatively oriented than my training had been, and I became concerned because the only numbers in my thesis were at the bottom of each page. My worries came to a head during my first semester teaching when I was asked to give a departmental bag lunch talk on my thesis project. As colleagues and students filed in, I saw Dudley Duncan arrive and take a seat. I knew Dudley was a famously competent and rigorous advocate of quantitative methods, and I had fears of him listening for a bit and then standing up and storming out, declaring "this is not sociology!" However, I needn't have worried, as Dudley and everyone else was interested in political indoctrination rituals in Mao's China, asked substantive questions, and didn't critique my interviewing methods.

Michigan turned out to provide an extraordinarily supportive environment for me to develop my teaching and research in sociology and China studies. The area studies centers at Michigan were very strong, and the Center for Chinese Studies was arguably the leading such center in America then. I got to work with excellent sociology doctoral students specializing on China, including Chuck Cell, Dick Barrett, Andy Walder, Gail Henderson, Xiaohu Xu, Jieming Chen, Wang Feng, Hongsheng Hao, Bill Lavelly, Jinyun Liu, Shenyang Guo, Ellen Pimentel, and Max Lummis, and I also got to help train many first-rate doctoral students in other departments. [When I moved to George Washington University (GW), three of my new colleagues were former Michigan students, political scientists Bruce Dickson and David Shambaugh and historian Ed McCord.]

The Department of Sociology also turned out to be very supportive. Even though most of my colleagues focused on American society, my senior colleague Ron Freedman had traveled to Taiwan in the 1950s and launched an ongoing program of collaborative research on that society's population trends. Later Al Hermalin and Arland Thornton joined Ron in research focusing on Taiwan. Given my research on family change patterns in the PRC (on which more later), I shared

with them a mutual interest in understanding Chinese family trends. That was also the heyday of collective action research at Michigan, with Chuck Tilly, Bill Gamson, and Mayer Zald attracting droves of students with their research on protest movements. Since China had just been through perhaps the largest collective action in history, it might be assumed that I would find this focus congenial. But the Cultural Revolution was so peculiar, with a charismatic national leader mobilizing the masses to attack power holders, that I could never figure out how to apply the social protest frameworks of my colleagues to China, although I found talks on their research very interesting. (Later Andy Walder was able to solve this puzzle; see Walder 2009, 2019.)

After travel to China became possible and as the PRC started to send students abroad for graduate training, my location in the poorest wing of the department (in terms of resources), the Center for Research on Social Organization, turned out not to be such a disadvantage. The training grants of the richest wing, the Population Studies Center, supported many graduate trainees in demography from the United States and China, bringing to Michigan more future China sociologists than I could have found fellowship support for in my wing.

Once research within China became possible, I began a series of collaborative survey projects within the PRC, and being at Michigan facilitated my switch in research location and methodology. The first project was a survey in 1987 of ever-married women in Chengdu, the capital city of Sichuan Province. Sichuan University agreed to host my project because Sichuan and Michigan had recently established a sister-state relationship. To retool my skills for the Chengdu survey, I became the faculty investigator for the department's Detroit Area Study survey training course in 1984, designing a survey of ever-married women in Detroit to examine changes over time in American mate choice customs and marital relations (see Whyte 1990, 1992). In the Chengdu survey and subsequent surveys in Beijing in 1991 and in Baoding, Hebei Province, in 1991 and 1994, I turned to Ron Freedman and Al Hermalin for advice, as well as to another senior colleague, sampling specialist Leslie Kish. In both the Chengdu and Baoding surveys, I included questions from earlier surveys my colleagues had conducted in Taiwan, thus making it possible to compare the patterns of family change in the two rival Chinese societies (see Whyte 1995, 2003, 2005, 2020b).

Even after I left Michigan, I continued to draw on (exploit?) the talented people I had worked with there. The project that has been my primary focus since 2000, examining variations and changes over time in attitudes toward rising income inequality in the post-Mao era through a series of China national surveys (in 2004, 2009, and 2014), would not have been possible without the Michigan gang. The stimulus to launch the project came from Leslie Kish, and our PRC collaborator throughout has been Shen Mingming, director of the Research Center for Contemporary China at Peking University, who had received his political science PhD at Michigan (with me serving on his dissertation committee). The research team for the project included former wolverines Wang Feng and Jieming Chen, Pierre Landry (Michigan political science PhD), and Albert Park (then a member of the Michigan economics faculty).

I ended up spending twenty-five happy and productive years at Michigan, eventually receiving tenure and becoming a full professor.⁶ I only departed for GW in 1994 because of a change in my family situation. My marriage to Ronnie was ending by the late 1980s, and when I remarried in 1991 to Alice Hogan, Alice was a program officer at the National Science Foundation (NSF). Alice and I spent the initial years of our marriage as a commuting couple, but then a temporary post for me co-directing the Sociology Program at NSF in 1993 and then an invitation to join the Department of Sociology at GW enabled us to finally experience “postmarital cohabitation.”

⁶Ann Arbor was also a great place to raise a family, and my children Adam and Tracy both attended public schools there and then graduated from Michigan.

FRAMING MY RESEARCH AGENDA

At Michigan I established the research agenda that I have followed since. I have worked on other projects that did not involve China, and three of my books (Whyte 1978, 1990, 2000) are in family sociology. But since most of my work has focused on China, I will use my projects on that society to illustrate what I have tried to contribute.

Although I was originally drawn to the study of China by Mao's attempt to build a more pure socialist society, when after his death China made its unexpected reversal toward markets and capitalism, I didn't lose interest. The combination of rejecting socialist economics while retaining Leninist politics seemed paradoxical, and the economic growth boom that followed was unexpected and spectacular. I also found it paradoxical that almost simultaneously with its relaxation of state controls over economic behavior, China in 1980 launched its draconian one-child policy, which tightened state controls over that most intimate of family behaviors, how many babies to have. So I continued to find interesting and challenging questions to study.

When I began my career, popular images of Red China, particularly in the wake of the Cultural Revolution, often portrayed it as a society populated by totalitarian automatons. Even though my interest in China stemmed from growing up during the Cold War, I saw my research as devoted to providing a more objective view of that society, not contributing to anticommunist polemics. I also didn't share the enthusiasm for Mao and the Cultural Revolution that quite a few young China scholars displayed at the time. I told my students that I had been inoculated against such enthusiasm by Stalin, and that my interest in Mao's utopian campaigns stemmed from wanting to see how they would work out, not from sharing his vision of pure socialism.

From the outset I had to think about how to frame my research in order to appeal to two distinct audiences, fellow sociologists and China specialists. The strategy I developed involved creating an intellectual dialogue between my two fields. In some works I tried to use social science theories to better understand the realities of contemporary China, while in others I attempted to use puzzling features of Chinese social patterns to contest theoretical frameworks drawn from the social sciences. In what follows I briefly discuss selected examples of both approaches. Since the findings from my Hong Kong projects have stood up well and I regard what I learned from those qualitative interviews and my later survey projects within China as equally valid and valuable, I mix examples from both phases of my research together here.

Using Sociology to Speak to China Studies

While a student, I had read *Thought Reform and the Psychology of Totalism*, by psychiatrist Robert Lifton (1961), which suggested that the CCP had developed group social pressure and indoctrination rituals that made it possible to brainwash individuals and turn them into fervent believers in Maoist values. But I had also read Amitai Etzioni's (1961) impressive book, *A Comparative Analysis of Complex Organizations*. (Amitai was then at Columbia, but many years later we became colleagues at GW.) Etzioni's book was based upon research in Western societies, but it presented a forceful argument that there are fundamental differences between normative, remunerative, and coercive organizations, in terms of their compliance structures and the orientations of subordinates. By applying these ideas through contrasting case studies drawn from my thesis project interviews, I was able to show that political indoctrination rituals in five different types of Chinese organizations (schools, bureaucratic offices, factories, farms, and forced labor camps) were much more successful in producing behavioral conformity than in changing personal beliefs, and that not surprisingly they were the least successful in the latter regard among inmates of forced labor camps (see Whyte 1974). The individuals described in my interview transcripts were very far from being uniform automatons.

A second example stems from my long-running survey work on popular attitudes toward rising income inequality in post-Mao China. Many analysts assert that Chinese citizens are becoming increasingly angry about the rising income inequality unleashed by market reforms, a claim I refer to as the “social volcano scenario.” Furthermore, this scenario also leads to the generalization that rural Chinese, stuck at the bottom of the national income hierarchy, are especially angry about distributive injustice. However, the national surveys I directed revealed that Chinese were less angry about distributive injustice than citizens in other societies, that they had not grown steadily angrier over the years, and that if anything, villagers had more favorable attitudes regarding current inequalities than urbanites (Han 2009; Whyte 2010, 2016; Whyte & Im 2014). In explaining these results, I drew on social justice research to point out that the social volcano scenario failed to distinguish inequality from inequity, and I argued that the massive improvements in general living standards in recent decades disposed Chinese to be more likely than their counterparts in other societies to feel that getting rich depends upon merit rather than upon societal unfairness. I also pointed out how villagers had suffered under a form of “socialist serfdom” in the Mao era, so that improvements in their living standards seemed more dramatic to them than those experienced by favored urbanites. These findings led me to conclude that to explain Chinese popular attitudes on inequality issues, past personal history and relative expectations are more important predictors than current status characteristics.

A final example of understanding China better through drawing on social science frameworks involves my work on the post-1980 one-child policy (Whyte et al. 2015, Whyte 2018). Many accounts argue that China’s coercively enforced birth limits led to widespread abuse and suffering, but that nonetheless, the campaign was responsible for driving fertility down to subreplacement levels by the early 1990s. With my collaborators I was able to show that most of the decline in Chinese fertility occurred during the 1970s, prior to the one-child campaign, and that the further decline since 1980 was primarily due to China’s extraordinarily rapid economic development, rather than to coercively enforced birth limits. In other words, the familiar demographic dictum, “development is the best contraceptive,” applies as much to China as to the similar declines to subreplacement fertility in Japan, Taiwan, South Korea, and Hong Kong, none of which employed state coercion to reduce births.⁷

Using Chinese Social Realities to Speak to Sociology

In terms of using Chinese evidence to contest prevailing social science theories, my early years at Michigan were a period when dependency and world systems theories were popular (e.g., Frank 1967, Wallerstein 1974). These theories claim that if developing countries become economically dependent upon rich countries and multinational corporations, income inequality within those countries will inevitably increase. Another framework, economist Simon Kuznets’s inverted U-curve hypothesis, involves a similar claim, arguing that as any agrarian society industrializes, income inequality will initially increase before subsequently starting to decline (Kuznets 1955). Dick Barrett and I published a paper (Barrett & Whyte 1982) in which we showed that Taiwan was a deviant case in terms of both frameworks, since as that society developed, it was highly dependent upon investment and trade with more advanced countries and companies, but income inequality declined. We explained this trend in terms of distinctive features of Taiwan’s

⁷ My interest in the one-child policy is not purely academic. In 1994 Alice and I were the beneficiaries of China’s abusive campaign, which enabled us to adopt our daughter Julia, then a healthy and thoroughly delightful 9-month-old whose only “defect” was her gender.

experience, including the prevalence of small family-owned manufacturing firms and, paradoxically, the somewhat egalitarian consequences of Japanese colonial rule over the island prior to 1945.

My projects on changes in family patterns also challenged existing theories. Over my career I have continued to grapple with the modernization framework I studied at Harvard, as applied so impressively to explain family change by William Goode (1963) in his classic book, *World Revolution and Family Patterns*. Goode argued that as agrarian societies modernized, their traditional family patterns would inevitably change and become more conjugal, thus converging toward the family patterns that already existed in advanced Western countries. In his framework it was the level of development reached that was important, not whether it was reached through capitalist or socialist development.

However, in my first project with Bill Parish, we discovered that village families had become somewhat more conjugal by the 1970s, in terms of greater freedom of mate choice and later marriage ages, even though very little modern economic development had occurred. However, in other respects those same families remained decidedly not very conjugal, such as in the continuing virtually universal negotiation of bride prices between bride and groom families, even though the PRC had declared such payments illegal (Parish & Whyte 1978; Whyte 1977, 2020b). Later, in my 1987 Chengdu survey, I discovered that freedom of mate choice had increased sharply during the 1950s but then stalled, with young women in the 1970s and 1980s still having less freedom to date and decide whom to marry than their counterparts in urban Taiwan (Whyte 1995, 2020b). Then, in my 1994 Baoding survey, I found that married daughters in that city contributed as much to support their own parents as did their brothers, whereas their counterparts in urban Taiwan still tended to follow the traditional pattern of married women supporting their husbands' parents rather than their own. So in terms of mate choice, Chengdu families were less conjugal than those in urban Taiwan, whereas in regard to filial support for elders, Baoding families were more conjugal than their Taiwan counterparts (Whyte 2003, 2005, 2020b).

Two revisions of Goode's family modernization framework were called for (see Whyte 2005, 2020b): Socialist transformations make a difference by eliminating family property and inheritance more rapidly and thoroughly than under capitalism and by producing altered institutions that require families to adapt to new patterns of work, residence, and everything else. In other words, the microinstitutional features of Chinese socialism made families in the PRC different than their counterparts in Taiwan's capitalist setting, rather than leading to the family convergence Goode predicted. Also, Chinese socialist families could become more conjugal in some respects (e.g., high maternal employment, married daughters providing support to their parents) while not becoming all that conjugal in other respects (e.g., marriages contingent on bride prices, women not able to consider more than one marriage prospect). More generally I concluded that modernization theory was not really a theory of economic development, since it didn't provide clear predictions on where and why economic development would occur. It was only a theory predicting the social consequences if development did occur, and some of the predictions turned out to be wrong when applied to China, or at least oversimplified.

Another example involves my only overlap with my father's work. Pop had difficulty getting his thesis approved at Chicago because, as it was a study in urban sociology, he had to have Louis Wirth on his committee, even though his description of a poor but tightly knit immigrant neighborhood directly contradicted Wirth's claim that cities were inherently disorganized, with high levels of crime and social problems (Wirth 1938; Whyte 1994, chapter 14). In my article, "Urbanism as a Chinese Way of Life" (Whyte 1983), drawing on my second project with Bill Parish, I echoed my father's critique, describing how residence committees and other features of Mao-era cities made

them fairly orderly places, although also lacking the creativity and energy that characterize cities in other societies.

The final example of Chinese reality challenging social science theories concerns the post-Mao economic boom, which I have previously analyzed in the *Annual Review of Sociology* (Whyte 2009; see also Whyte 2020a). China's boom contradicts much accepted wisdom in the sociology of development, stretching from Max Weber's ideas about how Chinese culture and family patterns made that society lag in launching an industrial revolution (Weber 1930, 1951) up to economic advice to get the institutions right, particularly through secure private property rights (e.g., Acemoglu et al. 2005), as well as on the need to quickly make the transition from planned to market institutions (termed shock therapy; e.g., Kornai 1990). In explaining the paradox of rapid economic growth despite only gradually instituting market reforms and still not providing secure private property rights, I rely on a combination of factors, including a cultural legacy that was actually conducive to economic development, Cold War changes that opened the West up to Chinese goods, the availability of a Chinese diaspora in Hong Kong and beyond to provide investment capital as well as quality control and marketing advice, and having a rural majority population (about 80% when the reforms began) with basic education who were desperate to escape the socialist serfdom of people's communes, and who were thus willing as migrants to take on difficult urban jobs for very low wages.⁸

The Changing Relationship Between Sociology and China Research

Over the course of my career, the relationship between my two fields has undergone a sea change. Gone are the days when sociological research on China was conducted by a few area specialists who used unusual methods to study that country at a distance. China sociology has joined the mainstream. Now most major sociology departments include trained China specialists, the journals in our discipline often carry articles on the PRC, and it is not uncommon for prominent sociologists who are not trained area specialists to launch China research projects. The contrast with economics is stark, for few economics departments include China specialists researching the most rapidly growing economy on the planet. Unfortunately, the contrast is also sharp regarding the sociology of Russia, Japan, India, and other important countries, which remain relatively underdeveloped specialties.

Sociology at George Washington University and at Harvard

From 1994 to 2000 I taught at GW, and then from 2000 until my retirement in 2015, I was a professor at Harvard. I was pleased by the offer to teach at GW, but I had some difficulty adjusting after being spoiled at Michigan. The Department of Sociology at GW was small, and it had history of internal conflict, particularly between the criminologists (another specialty I hadn't encountered earlier) and everyone else. Furthermore, one of the conditions for offering me the job was that I had to agree to chair the department after the incumbent, Joe Tropea, finished his term, the sort of administrative responsibility I had studiously avoided at Michigan. In addition, the department only offered BA and MA degrees, so the only doctoral students I got to work with were in other

⁸Ironically, decades of discrimination against rural citizens, who as lowly paid migrant laborers in the millions powered the post-Mao boom, have now begun to imperil further economic growth. Despite China's fabled tradition of valuing education, its labor force today is more poorly educated overall than in many other middle-income countries. The CCP is scrambling to overcome the educational barriers faced by its rural citizens (e.g., migrant children being barred from attending urban public schools) in order to build up the human capital needed for China to escape the middle-income trap (see Whyte 2019b).

departments. I also discovered that, unlike at Harvard or Michigan, undergraduate teaching at GW relied heavily upon adjuncts rather than regular faculty. As chair I was uncomfortable about students not being made aware that many sociology courses were taught by adjuncts. However, there were also attractions to being at GW, including a strong Elliott School of International Affairs and the many talks, conferences, and events in the Washington area that brought together China specialists from academe, the government, and think tanks, plus the fact that Alice and I could both work in DC while settling into family life there and raising our new daughter.

My move to Harvard again involved serendipity. My former student, Andy Walder, had started teaching at Columbia but then moved to Harvard when jobs were offered to him and his wife, Jean Oi, a Michigan-trained political scientist. When Andy received tenure in sociology but Jean's tenure bid in the Department of Government was denied, they left and now teach at Stanford. Harvard started looking for a new China sociologist, and two failed searches were conducted. Then, perhaps in desperation, they contacted me and said that if I was willing, I would be appointed to the China sociology slot without even having to come give a job talk. That appeal was hard to resist, and I agreed. As a result, I became a professor of sociology back at Harvard, replacing my former student, Andy Walder.

Ezra Vogel had just retired as I arrived in 2000, but he remained incredibly active intellectually, and he was very supportive of my return. Soon after my arrival, Ezra and I began convening monthly dinner meetings at his home, to which we invited China sociology graduate students and postdoctoral scholars, with a group member or visiting China sociologist giving an after-dinner talk on their current research. At Harvard I again had good doctoral students specializing on China to work with, including Xiaojiang Hu, Min Zhou, Alison Jones, Qingxia Tong, Dongxiao Liu, Dian Yang, Joyce Liu, Weihua An, Maocan Guo, Patrick Hamm, Chunping Han, Hsin-Chao Wu, Vince Feng, Phoenix Wang, Letian Zhang, Dong-Kyun Im, Jundai Liu, Yun Zhou, and Amy Tsang. My impression is that the dinner meetings of the China sociology group played as much of a role in their training as the graduate seminars I taught on China.⁹

I had left Harvard more than 30 years earlier, and in the interim much had changed. Retiree Ezra was the only faculty link to my student days. I found myself back in William James Hall, although this time they gave me a much better office. I knew that the Department of Sociology had gone through some difficult times, with sharp internal conflicts, particularly over the familiar cleavage between quantitative versus qualitative sociology. By the time of my return, the department had healed, with key actors in those conflicts gone, and the quantitative-qualitative debate was no longer an issue. One problem the department faced was that no untenured faculty member had received tenure after Andy Walder and Mary Waters years earlier, so the junior faculty members were understandably demoralized. However, during my time we hired away prominent sociologists from other top departments (e.g., Mary Brinton from Cornell; Rob Sampson and Mario Small from Chicago, following Bill Wilson earlier; Frank Dobbin, Michèle Lamont, Bruce Western, and the late Devah Pager from Princeton) who turned out to be congenial colleagues with first-rate intellects. We were also able to hire talented new junior faculty members who went on to receive tenure (Jason Beckfield, Jocelyn Viterna, Filiz Garip, and Sasha Killewald), so morale and congeniality in the department improved considerably.

I was also fortunate that Harvard's Fairbank Center for Chinese Studies was and is incredibly strong and stimulating, with an impressive array of China specialists (including many from other Boston-area universities), talented undergraduate and graduate students, and multiple grant

⁹After I retired, my replacement as China sociologist was Ya-Wen Lei, a former Harvard Society of Fellows junior fellow with a PhD from Michigan, and Ya-Wen and Ezra continued to host these dinner meetings.

programs, as well as public lectures, conferences, and other activities. In many ways the China studies side of my life at Harvard was as supportive as what I had experienced at Michigan at the start of my career. When I retired from teaching in 2015, I was sorry to leave such a positive academic environment.

CONCLUDING THOUGHTS

Although when I was young I thought I would have a very different career and for a long time tried to avoid becoming a sociologist, I am glad that my avoidance tactics were ultimately unsuccessful. Having a famous sociologist as a father can't have hurt my career, but my assumption that the breadth of our discipline would enable me to carve out my own path as a sociologist, rather than following closely in Pop's footsteps, turned out to be accurate. I am similarly grateful that my gamble on eventually being able to conduct research in China and to have Chinese sociologists as students and collaborators worked out, and that the PRC rewarded me by undergoing such dynamic and unexpected changes that I have always had fascinating research problems to study. I can't imagine that I would have had such a rewarding career if I had stuck to my plan of becoming a physicist.

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