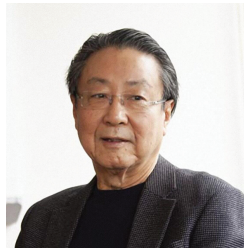


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Keywords

autobiography, fieldwork, institutional change, theory, confirmation

Abstract

This article examines the relationship between biography, chance, and persistence in accounting for the assembly of an American sociologist. It traces the accumulation of experiences involved in a research journey aimed at explanation of social behavior and institutional change. The process of discovery leading to a new theory may arise from serendipitous observations gained through fieldwork, while new combinations of ideas also emerge from social interactions with acquaintances, colleagues and friends. Cross-disciplinary intellectual trade offers rich opportunities for advances in the social and behavioral sciences.

1. INTRODUCTION

Setting the wellspring of my life's work as an American sociologist in an autobiographic frame takes me back in time to my arrival in the world, two weeks after the Japanese surrender in the Pacific theater of World War II. My father was in Nanjing, where he was a member of the delegation that accepted the surrender of the Japanese imperial army in China. There he received a telegram from my mother, who was residing in a mountainous retreat, far from the Japanese occupation. Learning of my birth, my father sent back a return message: "Call him Victor, for victory" in the war against Japan. My father had come to the United States during the Great Depression on scholarship to study at Asbury College, a Methodist seminary in Kentucky. He was adventurous, exploring the mountains of Kentucky and making friends with families there despite warnings from his college friends about hillbillies. Shortly after Japan's surprise attack on Pearl Harbor, having just finished his qualifying exam for a PhD at the University of Chicago, he was recruited by his Methodist friends to participate in a top-secret mission. With joint appointments in the Chinese and American militaries, he returned to China shortly after Pearl Harbor as an intelligence officer charged with coordinating the rescue of Doolittle Raiders, whose B-25 bombers were to land on the southeastern coast of China after taking off from an air craft carrier near Midway in a surprise raid over Japan in April 1942.

Growing up in Southern California in the 1950s made me wonder, early in my life, about my social origins. California was very different demographically in 1950, when the Nee family arrived in San Francisco. My father's American friends had lobbied Congress in support of a special act enabling us to enter as immigrants at a time when there was no legal immigration from Asia to the United States. We were met at the mooring of our ship, the SS Cleveland of the American President Lines, on November 30, 1950, by the minister of the Methodist church in nearby Burlingame. From the Bay Area, we traveled to Southern California, where my father gave lectures, after which my mother led her four children in singing Chinese children's songs. She had studied music and literature in college and had a gift for languages. As a native Taiwanese, she was happy to be an English-speaking immigrant in America. Our society was a national network of well-placed Methodist friends of my father, who were kind and generous in helping us to settle in Costa Mesa and then Altadena.

I started kindergarten not speaking a word of English. My K-12 education in public school was surprisingly uneventful, though I was the only nonwhite in my high school graduating class. There were no other immigrants from East Asia like me. During my second year of premedical study at the University of California, Los Angeles (UCLA), I attended a lecture by Martin Luther King, Jr., and joined the civil rights movement. I marched on New Year's Eve at the Helms Bakery in Los Angeles to protest racial discrimination against African Americans. I had never experienced discrimination myself, although I was a racial minority. By my second year at UCLA, I had become disenchanted with the idea of medical school and decided to transfer to the University of California (UC), Santa Cruz, a new liberal arts campus overlooking the Monterey Bay. At Santa Cruz, students were involved in the anti-Vietnam War movement. Not surprisingly, this led to my curiosity about the Chinese Revolution, as its success was prominent in the narrative behind the domino theory justifying America's war in Vietnam.

2. ASSEMBLING TOOLS OF SOCIOLOGICAL STUDY IN HARVARD, SAN FRANCISCO, AND BEIJING

I arrived in Cambridge in the fall of 1967 to start my first year of graduate study in modern Chinese history at Harvard. This provided me with a timely draft deferment. My antiwar male friends had decamped to Canada; other alternatives were unappealing. In the course of that year,

I met a French diplomat and scholar, Marianne Bastid, who had studied at Peking University and witnessed the birth of the Red Guard student movement and the idealism of the first year of Mao's Cultural Revolution. She was spending a year at Harvard to write a monograph on her experience as a French student in Peking. I interviewed Marianne and, with some documentary research, wrote a seminar paper on the origin of Mao's Cultural Revolution at Peking University. I subsequently gave a copy to William Hinton, whom I met at a small reception following his lecture on *Fanshen* (Hinton 1966), his participant observation study of social revolution in a North China village. Little did I know that he would pass my seminar paper on to Paul Sweezy, editor of *Monthly Review* (*MR*). I received a telephone call from *MR* in New York City inviting me to meet with Paul Sweezy and Harry Magdoff. Over lunch, Sweezy pulled out a beautiful dust jacket of a book entitled *The Cultural Revolution at Peking University*, and with my name as the author. Sweezy said he needed to have a revision of my seminar paper before May 15, for simultaneous publication in their July-August special issue. The managing editor needed time for translations for their Spanish and Italian editions. How could I refuse? But I said the problem with this deadline was that I wanted to take part in the Harvard student strike. On the night of the protest (April 8, 1969), I climbed over the wall across from Memorial Hall into Harvard Yard and stood vigil with other graduate students and young professors in front of the Widener Library. I am sure my observations of the student occupation of University Hall and the early morning arrests influenced the revision I subsequently submitted to *MR*, which captured the spirit of this antiwar protest. There was a Swedish translation. I later learned from Ross Terrill, an Australian graduate student of Chinese politics, that when he was in Paris that summer, he saw, on a Left Bank newsstand, a French translation of my paper published in *Les Temps modernes*, founded and edited by Simone de Beauvoir, Jean Paul Sartre, and Maurice Merleau-Ponty.

At the end of my second year at Harvard, I transferred to the PhD program in sociology, as my focus was shifting from history to the contemporary era of transformative change. During my first year in sociology, André Schiffrin, the executive editor of Pantheon Books, who was in Cambridge to meet with authors, invited me out of the blue to write a book on an American Chinatown. A follow-up meeting with him in New York City led to a contract with an offer of a generous advance. I went back to Cambridge with the contract in hand unsigned. I wasn't sure this would be a good idea given my interest in conducting field research in China. I was engaged to Brett de Bary, a fellow graduate student who arrived at Harvard the same year I did to study modern Japanese literature. She thought it would be a great project for us to work on, though our parents discouraged us from taking it on because it would distract from dissertation work. Brett drew inspiration from *Let Us Now Praise Famous Men*, the documentary study by James Agee and Walker Evans of rural poverty during the Great Depression. On our honeymoon in June 1970, we flew to San Francisco, where we rented an apartment on Powell Street on top of Nob Hill. I still remember the clanging of the trolley cars as they passed by our apartment, a block away from Chinatown. After a summer of field research where we observed daily life in Chinatown and conducted life-course interviews, we returned to Cambridge. Ezra Vogel, who was trained by the social anthropologist Clyde Kluckhohn, generously organized a special seminar with Herbert Gans and Richard Sennett. At the seminar we reported our field observations and shared texts from our interviews. This seminar provided us training in the science and craft of sociological ethnography.

Our fieldwork and documentary research on the social history of San Francisco's Chinatown brought to light a dark history of racism, anti-Chinese collective action, and racial violence against Chinese immigrants across the western states. The call for a white California by early settlers was nearly unanimous. A split labor market competition had sparked a national anti-Chinese xenophobia that culminated in the passage by Congress of the Chinese Exclusion Acts in the late nineteenth

century. The legacy of racial violence and driving out of Chinese settlers from small Chinatowns and scattered settlements across the western states cast an enduring shadow on the lives of the respondents we interviewed. Our ethnography of Chinatown documented a grim social history of the multi-generational effects of hypersegregation in a densely populated racial ghetto with a high suicide rate. The legacy of racism shaped the experience of every social group, including the young and angry assimilated fourth and fifth generations. That summer I realized how different my family's experience in this country had been from that of the Chinatown descendants of the nineteenth-century immigration from the Pearl River Delta, and how fortunate I was to have grown up as I did in Southern California. In 1971, we carried out another round of field research in Chinatown. We completed our coauthored book while still in San Francisco. *Longtime Californ': A Documentary Study of an American Chinatown* (Nee & de Bary 1973) was published early in 1973.

In 1972, Brett and I flew to Tokyo, which was for me the staging ground for field research to study the ongoing social revolution in China. The Japanese students and intellectuals we met in Tokyo were protesting sporadically on the streets against the Vietnam War. From the perspective of Tokyo's protest marches, China's uninterrupted revolution was a scene of tumultuous and violent political upheaval where rebels seized power from the Communist Party and set up Paris Commune-inspired revolutionary governance in universities, factories, and municipal governments. I was invited to travel with Brett from Tokyo to China in the spring of 1973, after the ping-pong diplomacy that brought Nixon and Kissinger to China in February 1973 for the negotiation that preceded America's recognition of the People's Republic of China (PRC). Brett and I were part of an American delegation of graduate students and young professors opposed to the Vietnam War, affiliated with the Committee of Concerned Asian Scholars, who gathered in Peking and were invited to meet with Premier Zhou Enlai in the Great Hall of the People. We were traveling with my mother, who was active in the US-China Peoples Friendship Association.

On this first trip, I had received prior approval from the Foreign Ministry to conduct field research in a Chinese factory. I arrived in China via Hong Kong with a contract and an advance from Pantheon Books to write a sequel to Maurice Zeitlin's (1967) *Revolutionary Politics and the Cuban Working Class*. On the train from Shenzhen, then a small fishing village on China's border with Hong Kong, was Winston Churchill's son. I wondered what was on his mind as he entered Communist China. In my hands was a copy of the questionnaire Maurice Zeitlin had used for his study of factory life in Cuba. I was working on adapting the questionnaire for use in the Chinese setting, at a state-owned factory in Shanghai. When I arrived at the factory, I was met by its director, and as we walked past the entryway, I saw a large placard, "Welcome American sociologist Victor Nee." My survey questionnaire had been distributed to the factory workers before my arrival, and the factory had stopped production to accommodate my research. "But I'm a graduate student," I thought to myself as I walked into the factory. After I had spent the afternoon interviewing factory workers for their life histories, a small delegation of tall factory workers walked robustly into the room with their compendium report. They told me that the workers, in their respective work groups, had arrived at a consensus view after discussing each item of the questionnaire. The factory's revolutionary committee had then met with the leaders of the four workshops to distill this compendium report. I looked at the data sheet and found zero variance for every item. Crestfallen, I thanked the workers for taking the time and trouble to respond to my questionnaire and for the interviews. Knowing no book would be published, André Schiffrin generously let me keep Pantheon's advance.

In 1974, I was invited to travel to Beijing from Tokyo as part of the first group of international students from East Asia, Europe, and Canada to study for a year at Peking University. I was the only American student. At the time, *Beida* admitted "worker, peasant, soldier" (*gongnongbing*)

students without entrance examination based on political correctness and perceived social origin. The education reform nationwide emphasized an “open school” education (*kaimenbanxue*) involving frequent excursions to state-owned factories and suburban people’s communes to learn from ordinary working people. The University’s library, one of the best in China, was closed, even for the faculty. In classes, we were provided with ideology infused selective readings, distributed in mimeograph form. It was clear to me that the educational reform led by ultra-leftist Jiang Qing, Mao’s wife, was a colossal failure. Zhou Peiyuan, the president of the university, whom my parents had met in Altadena, where he was a physicist at Caltech before returning to China in the 1950s, confided to me that this was his view as well. A “worker, peasant, soldier” student told me in confidence that he didn’t see the difference between being a student at *Beida* and his previous life as an educated youth sent to a remote army base in Heilongjiang province.

This was the worst time possible to conduct sociological research in China, in the throes of the final years of Mao’s Cultural Revolution. From its early revolutionary idealism, the Red Guard student movement had disintegrated into violent factional battles reminiscent of the Reign of Terror in the French Revolution. Civil war involving positional battles between warring factions led to high casualty numbers and much destruction in various regions. Beijing had just experienced a failed coup by Mao’s chosen successor, Lin Biao, who died when his plane crashed in an attempted escape to Moscow. Xenophobia and unspoken fear were widespread. Try as I might, I found sociological field research to be impossible, despite my persistent notetaking and efforts at being a participant observer at Peking University.

I returned to Cambridge in 1975 knowing that I had failed in my effort to conduct field research that I had hoped to. I confronted a genuine dilemma. I wanted to do a thesis on the Chinese Revolution. After all, Theda Skocpol, my friend in the same sociology cohort, had just completed her dissertation on the state and social revolution with George Homans as her advisor. I met with Homans, and to my surprise, he encouraged me to do what I had set out to do. Ezra Vogel was on sabbatical leave in Hong Kong, but luckily George Homans agreed to serve as my thesis advisor.

I had first met Homans after my initial semester in the Department of Sociology when, as the director of graduate study, he called me to his office regarding my incomplete in a sociology graduate seminar. He greeted me with an avuncular, booming voice, a bit gruff initially, but the encounter soon evolved into a jocular conversation, with Homans retelling childhood stories about Yankee clipper ships that members of his extended Boston Brahmin family prospered from in the nineteenth-century New England trade with China. After this meeting, I enjoyed going to his weekly afternoon sherry hour in William James Hall, as well as occasional conversations in his office, continuing our lighthearted interaction.

I was drawn to Homans’s exchange theory and perspective on social science after reading his *The Human Group* (Homans 1950), *Social Behavior* (Homans 1974), and *The Nature of Social Science* (Homans 1967). I was particularly impressed by his use of logically related general propositions specifying social mechanisms enabling and motivating cooperation, trust, and social order in close-knit networks. His theory of social exchange was both parsimonious and general in its explanation of the endogenous social processes guiding human behavior. He assessed economics as the most successful social science, while at the same time asserting that economic action was just social exchange in the context of markets. Like Talcott Parsons, Homans was a theorist who looked to sociology as the social science within which economics and political science were important sub-fields. Homans’s rational action proposition assumed Herbert Simon’s (1972, 1991) specification of bounded rationality constrained by information asymmetry and uncertainty. Unlike the utility maximizing of *homo economicus* in neoclassical economics, in his view, bounded rationality was coupled with satisficing, “the behavior of human beings who *satisfice* because they have not the *wits* to *maximize*” (Simon 1957, p. xxiv, emphasis in original). Bounded rationality and satisficing

provided an integrative behavioral glue that connected economics, sociology, political science, and psychology in post–World War II cross-disciplinary social and behavioral science.

In one of our longer conversations, Homans smiled approvingly when I said that his social exchange theory was impressive for its parsimony and generalizability across cultures and time. But then, to provoke a reaction, I asked if his theory’s explanatory power was not limited to the microlevel of small groups? What about the macrosocietal level? At this point, Homans concluded our conversation, ruffled, but with his characteristic gruff Bostonian charm, “That’s for you to work on, Victor.”

I devised a new thesis proposal that extended exchange theory to explain the workings of the mass line, an institutional innovation used to marshal peasant nationalism in support of the Communist-led war of resistance against Japan’s militarist “burn all, kill all” campaign in North China (Johnson 1962). The mass line further enabled the Chinese Communist Party (CCP) and its Eighth Route Army to mobilize peasant nationalism in support of the Chinese Revolution. A team of grassroots cadres entered and lived in villages to listen to peasant demands and then devised and implemented policies for collective action to address their needs. The Party thus managed to earn the trust and commitment of ordinary villagers; this was evident in the tidal flow of young peasant men as fresh recruits in the People’s Liberation Army (PLA) and as grassroots volunteers in village militias and para-military organizations in the war of resistance in North China. My dissertation, which I completed at Cornell, was praised by members of my thesis committee and quickly approved. I sensed Homans was in his own way pleased that social exchange theory was general enough to explain a key institutional innovation that won bottom-up support for the Chinese Revolution from the peasantry. However, I did not think my thesis was publishable. To me, its importance was in providing a launching pad for an ongoing research program focused on transformative institutional change.¹

3. INDUCTIVE AND DEDUCTIVE THEORIZING ON MARKET TRANSITION

I accepted an assistant professorship offer from the Department of Sociology at UC Santa Barbara (UCSB) in 1977 but taught my first year in Cornell’s sociology department. I applied for and was awarded two alternate-year fellowships from the Social Science Research Council (SSRC) and American Council of Learned Societies, enabling me to take time off every other year from teaching at UCSB to pursue research and writing at Cornell, where Brett was an assistant professor. This bicoastal arrangement enabled a dual-career marriage and cemented collegial ties and friendships stretching from Santa Barbara to Ithaca.

In the spring of 1980, funded by an SSRC fellowship, I arrived at the Yangbei production brigade by army jeep after a two-day drive from the island city of Xiamen. En route, the female PLA officer from Shandong province who accompanied me told me about her sacrifices and the disappointment she now felt over the fact that the revolution she and her husband had fought for was stalled and likely over. The Yangbei production brigade was a single-surname Hakka village located in the Fujian–Jiangxi border region near the Wuyi Mountains, historically an area of peasant discontent and rebellion that had inspired Mao’s observation, “a single spark can start a prairie fire.”

As I walked with the production brigade leader through the village, my thoughts turned to the social anthropologists Clyde Kluckhohn and John Pelzel. They both were in the multi-disciplinary

¹To my surprise, long after I was a tenured professor, Aage Sørensen, then the chair of the Department of Sociology, published my thesis in a monograph series he edited with Liah Greenfeld (Nee 1991).

Department of Social Relations founded by Talcott Parsons, and the latter was on my dissertation committee. At Yangbei I felt I was representing what had been the social anthropology wing of that Harvard department, although the Department of Sociology had just pulled out of Social Relations to establish itself as an independent department, in the hope of gaining more faculty lines.

The puzzle I wanted to explore in the village had to do with the nature of the relationship between the peasant household economy and state-managed collectivist agriculture, given the long tradition of peasant household individualism in China. I lived in a peasant's home, where I conducted interviews with ordinary villagers, and I observed the workaday life of the village community. My participant observation research revealed behavioral patterns consistent with bounded rationality and satisficing when peasants worked in collectivist agriculture, but optimizing behavior when they worked on the household's private plot and in animal husbandry. They obviously earned their grain allotment and some welfare provisions in collectivized agriculture. However, they continued to rely on their household economy for their basic security and well-being. The priority of the household economy for peasants had been reinforced by the failure of Mao's Great Leap Forward, a mass mobilization that had ended in famine. My household survey revealed "holes" in the composition of peasant households consistent with the villagers' narrative of men leaving the village and dying on the road in futile searches for food.

I learned that peasant calculation of utility was subtle, often revealing a sophisticated investment logic centered on supply and demand for their own products in weekly rural markets in the nearby township. Peasants gamed the distribution system of collective agriculture by spacing births and extending numbers of children over time to maximize on the household's share of grain. Having an adult member employed in a nonfarm job in the nearby township or county town was a strategy for optimizing the household's grain allotment. In collective agriculture, women conserved their energy and effort for childcare and work on the household's private plot. Men focused their attention on marketing the household's pig in the nearby rural market town. To maximize the price fetched on the family pig, the men timed the slaughter according to the cycle of supply and demand for pork in local markets. Peasant utility maximizing in the household economy was motivated in part by a pervasive hunger. The tension between the peasant household economy and collectivized agriculture revealed the central problem of state-managed agriculture. The perfunctory pace of work in collectivized agriculture underscored not just a classic free-rider dilemma but a conflict of interest between the peasant household and the institutional design of the people's commune system. The state's interest in collectivized agriculture was motivated by the need to appropriate agricultural surpluses to finance its ambitious five-year plan for heavy industrialization and to supply grain for cities through central planning. This left peasants with barely enough food for subsistence. Hunger was a routine conundrum of daily life. Peasant household members expended less effort when they worked for the production team and brigade, contributing to the low productivity of collectivized agriculture through "everyday forms of resistance," the apt subtitle for James Scott's (1987) book, *Weapons of the Weak*.

China's transition to a market economy began in poor agrarian communities. Bottom-up collective action by peasants in Anhui province sparked a social movement in the 1980s that spread across rural China calling for the disassembly of the economic institutions of collectivized agriculture. The outcome was a nationwide collapse of the people's commune system. Peasant entrepreneurs, drawing on their achieved market prowess, started small cottage industries. Concomitantly, the state adopted new laws establishing the household responsibility system to replace the people's commune system and to restrict small businesses in light industry and commerce to no more than seven employees. The ensuing increase in productivity in agriculture by means of the new household responsibility system released an abundance of surplus labor seeking nonfarm

employment, first in township and village enterprises, and then in the cities. This led to the largest internal migration in history, supplying a tidal wave of rural migrants seeking new industrial jobs in China's booming capitalist economy in its southeastern coastal provinces.

My brief venture into economic anthropology in the mountainous Hakka village proved productive in that it opened my mind to imagining a multi-level conceptual framework that would link microlevel purposive action of individual actors with meso- and macrolevel structures of opportunity embedded in a bottom-up emergence of economic institutions of a market economy. At UCSB, my colleague Otis Dudley Duncan had coached me on how to design a good survey questionnaire. He advised using, verbatim, reliable standard measures of social behavior tested in earlier surveys by social demographers and avoiding new wording and fanciful ideas that cannot be reliably measured. (He emphasized that this was the only occasion in academic life where plagiarism was a good idea.)

In 1985, as a visiting professor at Xiamen University, I was able to spend three months with a team of economics students, moving from village to village in the nearby Longhai and Tong'an districts, conducting survey research and observing institutional change at the start of China's transition to a market economy. To my knowledge, this was the first large-scale household and village survey conducted by an American sociologist in China. In the evening, I would share my thoughts in conversations with some of the Chinese students on my research team. I was searching for elements of a theory and conceptual framework that could answer questions and address the puzzles I encountered during my field research in the villages and market towns. As modern social and behavioral science has emerged, field research has often provided the opportunity for discovery of a grounded theory. In his fieldwork in industrial districts, Alfred Marshall, a founder of neoclassical economics, sought to discern the sources of cumulative advantage in England's competition with the emerging industrial power of Germany. He came to focus on three factors in his theory of agglomeration: a surplus concentration of skilled workers and technicians; economy of scale in the provision of nontradeable inputs; and knowledge spillover, a social process wherein "mysteries of the trade" are in the air and good ideas are passed from one worker to another, germinating the seeds of new ideas or innovation [Marshall 2009 (1890)].

My subsequent endeavor to devise a deductive theory with logically related propositions and derived predictions was influenced by George Homans. I argued that in an ongoing transition to a market economy, institutional change in the basis of power, defined as control over resources, would cumulatively augment the power of entrepreneurs and direct producers, while concomitantly resulting in a relative decline of power held by the redistributive political elite. This prediction was counterintuitive, given Communist Party power. Publication of my theory of market transition (Nee 1989, 1996) provoked a lively debate in sociology and numerous competitive tests. Initially, it seemed as if the entire sociological field of researchers disagreed with my deductive theory and its prediction. After helping to design questionnaires aimed at collecting behavioral data in the nationwide China-Cornell-Oxford nutrition and health survey, I assembled a new 29-province data set for a multi-level analysis of market transition, which informed debate in the *American Journal of Sociology*, as well as collaborative empirical papers with Yang Cao, Rebecca Matthews, and Lisa Keister, graduate students working with me at Cornell. Yang Cao and I explored path-dependent institutional change, regional variability of institutional contours in the uneven penetration of markets, and a resilient redistributive state socialism in inland and poor hinterland provinces of China. A grant from the National Science Foundation (NSF) provided funding for a new round of survey research in urban China, and our analysis reported results that were consistent with market transition theory.

The market transition debate was productive. Xueguang Zhou, Nan Lin, and Yanjie Bian told me that, though they thought my theory was wrongheaded, they were happy that it opened the

way for a slew of China-focused articles to be published in mainstream sociology journals. I concluded that a theory's capacity to serve as a whipping post for a lively debate should be seen as an affirmative signal of its value. There was no competing theory, and the criticism seemed ad hoc, coming from different angles and reframing the question with a variety of statistical models and data sets (Cao & Nee 2000). The eclecticism of specialists in China studies aside, the sociological critics of my market transition theory were engaged positivists whose aims were to advance sociological science by putting my theory to the test of evidence using different statistical models and data sets drawn from a variety of countries and regions: China, Central Europe, Russia, and Vietnam. I was grateful for their attention and have tried to incorporate their findings in my work on the emergence of dynamic capitalism in China.

4. A NEW INSTITUTIONALIST THEORY OF ASSIMILATION

I had met Alejandro Portes at an SSRC conference convened at the East West Center of the University of Hawai'i and invited him to give a seminar on his work at UCSB. I felt that his study of ethnic solidarity and enforceable trust in ethnic enclave economies provided a compelling analytic narrative of endogenous socioeconomic mobility for immigrant minorities. I told Alex that I would try to confirm his enclave economy hypotheses with PUMS data on the Miami Cuban ethnic economy, a project on which I worked in collaboration with Jimmy Sanders, a sociology graduate student at UCSB. To my surprise, we did not find evidence consistent with his prediction of ethnic advantage in the immigrant enclave and disadvantage outside of it. Instead, our analysis showed that Cuban immigrant entrepreneurs living and operating small businesses both inside and outside of the enclave economy fared equally well.

I accepted an equivalent tenured offer from Cornell's Department of Sociology in 1984, arriving a year later in the fall of 1985. Several years later, with sequential grants from the NSF in 1988 and 1990, I traveled with Jimmy Sanders and a team of Cornell sociology graduate students to Los Angeles to conduct life-course interviews focusing on job transitions of East Asian immigrants. The use of event history was inspired by my field research on the collaborative CLEO physics lab at Cornell. I regularly played singles tennis with physicists late at night. Afterwards I would go home to sleep, and the physicists would return to the lab to work. I was curious and asked if I could study the CLEO lab. Not far from the indoor tennis court, a large multi-university team of physicists was studying the bottom and charm quarks, invisible subatomic particles important to the Standard Model in high-energy particle physics. I learned that CLEO physicists smashed atoms in the Wilson Synchrotron Laboratory accelerator to study the invisible charm quark through the visible trace pattern left by its trajectory. It occurred to me that, analogously, it was also possible to identify the nature of the boundaries of segmented labor markets and immigrant enclave economy by examining, with event history analysis, the similarly invisible trajectory of job changes among post-1965 immigrants from East Asia.

Our event history analysis of job transitions of a random sample of immigrants from their first job to their current job revealed that the boundaries of immigrant enclave economies were neither closed nor segmented, as had been argued by segmented labor market theorists (Reich et al. 1973, Cain 1976). Rather, they were porous, enabling recent Asian immigrants to pursue job mobility in the mainstream metropolitan labor market, often moving in and out of the immigrant enclave economy. Our findings suggested that in the post-civil rights era, mainstream American labor markets were open to immigrant minorities in their pursuit of opportunities in line with their educational attainment and work experience (Nee et al. 1994). Our analysis showed an initial trajectory of frequent job changes, often with moves in and out of the immigrant ethnic economy, until an equilibrium wage in line with the job seeker's stock of human capital was attained. In the case of technical and professional immigrant minorities, the equilibrium job frequently was

secured in the mainstream labor market. This finding was inconsistent with the claim of Portes and his associates that racial discrimination against immigrant minorities in open labor markets was unyielding and incorrigible (Portes & Sensenbrenner 1993, Portes & Zhou 1993).

Notwithstanding our debate over the enclave economy hypothesis, both Alex Portes and I looked to Robert Merton as a mutual friend exemplary in his call for theories of the middle range and predictions that can be tested and confirmed with evidence. My first exchange with him occurred when he sent me an email out of the blue in 1989 detailing why he liked my *American Sociological Review* article on market transition theory (Nee 1989). Later in 1994, as a visiting scholar at the Russell Sage Foundation (RSF), where Merton was a senior scholar, I realized in reading his *Social Theory and Social Structure* that we shared a common view of sociological theory as a set of logically related propositions that specify the “social mechanisms—that is, the social processes having consequences for designated parts of the social structure” (Merton 1968, p. 43). At the RSF, Merton read and commented on my papers, including unpublished work written in the previous year. He invited me to dinner at the University Club with his wife, Harriet Zuckerman. After he regaled us with vivid recollections of his years at Columbia, the graduate students he trained, and those he did not, the conversation drifted to me. He told me he had a seasoned eye and was confident that I would make significant contributions to advancing sociology as a social science; but, he said, I published too much, and I needed to slow down and stay focused on the important questions.

During my year at the RSF, Richard Alba dropped by on several occasions and persuaded me to collaborate with him on a paper for an SSRC conference on contemporary immigration and assimilation. I had met Richard at Cornell when both of us were junior faculty. After my year at the RSF, we met occasionally in Ithaca or in Albany to work on our conference paper on the assimilation of post-1965 immigrants, knowing that assimilation as a sociological concept had fallen into disrepute. Assimilation theory, we agreed, was used mainly as a whipping post to criticize the Chicago School’s definition of assimilation for its WASP view of the American identity. Later, I suggested to Richard that we coauthor a book on the subject.

At the turn of the new millennium, during a sabbatical leave as a visiting professor in the Department of Sociology at Harvard, I worked on a revision of an early draft of our book. I felt that the old Chicago School paradigm of assimilation we had used as our conceptual framework for the SSRC conference paper was outdated and needed to be replaced. The problem with the Milton Gordon extension of the Chicago School was its teleological narrative of straight-line assimilation of immigrants and their descendants. I sensed an opportunity to outline a new institutionalist theory specifying proximate and distal mechanisms enabling and motivating assimilation of immigrant minorities. I was influenced by the “new realism” in the philosophy of social science, which emphasized realist explanations specifying causal mechanisms (Miller 1987).² Discovery of causal depth was a priority of the new realism in its criticism of logical positivism.

In contrast to the teleological assumption of straight-line assimilation, a neo-assimilation theory asserts that assimilation is a dependent variable involving a contingent social dynamics shaped by conflicting segregating and blending mechanisms: for example, episodic clashes between nativist xenophobia and the purposive action of immigrants and their children in pursuit of the American dream. In this view, assimilation arises from the unintended consequences of bounded rationality and the satisficing behavior of new immigrants and their children in their utilitarian pursuit of a better life.

²New realist explanations can be reframed in a positive theory of logically related propositions from which predictions can be derived and confirmed with evidence (Nee & Alba 2013, DellaPosta et al. 2017, Solow 2017, van de Rijt 2017).

Civil rights–era legislation outlawed discrimination by race, thereby increasing the cost of racism. The Immigration and Nationality Act of 1965 extended civil rights–era institutional reform to immigration law, abolishing the national origin quota and providing equal immigration standards for immigrants from all countries. These transformative institutional changes increased the cost of racial discrimination and cumulatively opened access for immigrant minorities and their children to mainstream American institutions. Moreover, cultural beliefs and values were evolving; the social legitimacy of informal norms and formal rules of equal opportunity for ethnic and racial minorities was increased, as could be seen in the growing social acceptance of interracial friendship and marriage in the American mainstream. Notwithstanding the persistence of racism, the assimilation of post-1965 immigrants and the second generation proceeded at a faster pace than it had for the late-nineteenth- and early-twentieth-century immigrants from Europe, as our synthesis of empirical research showed (Alba & Nee 2003, Waters & Jimenez 2005, Kasinitz et al. 2008, Drouhot & Nee 2019).

5. QUEST FOR A THEORY OF INSTITUTIONAL CHANGE AND SOCIETAL TRANSFORMATION

The mid-1980s to the opening decade of the new millennium was an intellectually exciting time at Cornell's Department of Sociology. Michael Hannan led a redesign of the graduate program, and the arrival of new senior faculty transformed the old empiricist department toward a focus on theory as explanation and theory-driven empirical research. It became customary for the active faculty to present and discuss research in progress and circulate their articles, both unpublished and published. Regrettably, Hannan and Susan Olzak returned to Stanford in the mid-1990s.

During my year (1996–1997) at the Center for Advanced Study in the Behavioral Sciences at Stanford (CASBS), Bob Merton visited, and, on a walk, he told the story of how he and Paul Lazarsfeld contributed to its design. When I returned to Cornell in the fall of 1997, Philip Lewis, the dean, asked me to serve as chair of the Department of Sociology. I was surprised to find myself in the mood for institution building. I persuaded Phil Lewis and his associate dean, Biddy Martin, soon to be the Cornell provost, that it was within Cornell's reach to build a first-tier sociology department. The arrival of new senior and junior faculty soon followed from my meetings in the dean's office. The year at the CASBS had given me time to reflect on what I had learned from my cross-disciplinary research. *The New Institutionalism in Sociology* (Brinton & Nee 1998) came out of that year; it was coedited with Mary Brinton, who left the University of Chicago to accept a Cornell offer. With the support of the provost's office, I managed to secure funding from the Atlantic Philanthropies to establish two research centers: the Center for the Study of Economy and Society, and the Center for the Study of Inequality. I used the latter to recruit David Grusky and Szonja Szelényi to Cornell. Richard Swedberg joined us from Stockholm University after a year at the CASBS. His office was next to mine, and we met often in our offices, in the corridor we shared, and over lunch. We were committed to advancing economic sociology at Cornell, but we kept the big picture of advancing sociology in mind in our conversations and scholarship. He and I organized conferences on the new institutionalism in sociology, from which we produced *The Economic Sociology of Capitalism* (Nee & Swedberg 2005).

Although I received multiple outside offers, I stayed at Cornell, where the top priority on my agenda was to launch an ongoing study program on the new institutionalism. The challenge was to build a community without walls of like-minded social scientists to work on a cross-disciplinary theory of institutions and institutional change. I envisioned my quest as necessarily involving traversing allied disciplines in the social and behavioral sciences in search of the holy grail of a compelling theory. I organized an ongoing series of lectures, workshops, and conferences on the new institutionalism in sociology, economics, and political science. The economist Douglass

North, on one of his first trips to Ithaca, stayed at my home. After dinner one night, he talked about his youthful years as a graduate student in economics at UC Berkeley. Mainstream neoclassical economics was not then of interest to him; instead, it was Sociology that was the most exciting of the social sciences. He had traveled east to study with Talcott Parsons, Karl Polanyi, and Robert Merton. I made a mental note that sociology had flourished in the postwar atmosphere of open cross-disciplinary intellectual trade between economics and sociology. Sociology was not isolationist as it is today, but fully engaged in intellectual trade with its allied social sciences. North invited me to present my work on the new institutionalist economic sociology at the inaugural meeting of the International Society of New Institutional Economics, which he cofounded with Ronald Coase and Oliver Williamson. After he was awarded the Nobel Prize in economics, he included me on the keynote panel he chaired at an annual meeting of the American Economic Association in Chicago, together with Ronald Coase, Oliver Williamson, and Avner Greif.³ Cumulatively, my ongoing organization of cross-disciplinary exchanges on the new institutionalism enabled a lively intellectual trade between economics and sociology.⁴

Émile Durkheim had founded sociology as a new discipline, distinct from psychology and economics—one dedicated to the study of institutions as social facts. His methodological holism assumed that individuals are like the leaves on the branches of a tree that is an institution's structure. Why focus on the dynamics of individual leaves fluttering randomly in the breeze? This methodological holism shaped the post-World War II flourishing of mainstream American sociology. Talcott Parsons's articles in the *Quarterly Journal of Economics* and *Journal of Political Economy* argued that whenever empiricists in economics extended neoclassical economics to the macro-economic arena, they relied on structural-functional sociology for their analytical narrative. The problem with this in a world undergoing transformative institutional change was its assumption of an equilibrium enabling the stability of a status quo, as in a Nash equilibrium.

Given this postwar structural-functional consensus, George Homans's methodological individualism made him the odd man out. In Harvard's sociology department, it set him at odds with Talcott Parsons and his many graduate students. Critiquing Durkheim's theory of suicide, Homans pointed out that despite the higher suicide rates of northern European countries, suicide is an individual act, not simply a social fact of Protestant countries. Homans and Simon shared a behavioral assumption of bounded rationality involving uncertainty and satisficing. In this framework, purposive action works well as the microfoundation for a new institutionalist theory of institutions and institutional change.

6. THE ROLE OF OPPOSITION NORMS IN THE EMERGENCE OF DYNAMIC CAPITALISM IN CHINA

In an article criticizing aspects of the new institutional economics, Mark Granovetter (1985) sidestepped the question of institutions. He focused instead on ongoing social relationships that enabled cooperation and trust in both markets and hierarchies. Nevertheless, he acknowledged

³Our papers were subsequently published in the *American Economic Review*.

⁴Participating were social and behavioral scientists in economics, sociology, and political science, including Oliver Williamson, Douglass North, Roger Gould, Harrison White, John Meyer, Duncan Watts, Paul DiMaggio, John Padgett, Richard Swedberg, Walter Powell, Francis Fukuyama, James March, Peter Katzenstein, Alejandro Portes, Mark Granovetter, Justin Yifu Lin, Oliver Hart, James Robinson, Delia Baldassarri, Loïc Wacquant, Marion Fourcade, Neil Fligstein, Paul Ingram, Bruce Carruthers, Martin Ruef, Barnaby Marsh, Patricia Fernandez Kelly, Siegwart Lindenbergh, Håkan Holm, David Strang, Kaushik Basu, and Ravi Kanbur. The participants became members of a cross-disciplinary center without walls, as fellows of the Center for the Study of Economy and Society at Cornell.

the limitations of trust based on close-knit network ties. Those whom you trust are in a structural position to betray the trust, as in Ponzi schemes and embezzlement. Hence, he argued for a case-by-case study of concrete network ties, as in his case study of Thomas Edison and Western Electric.

At a small conference at the Rockefeller Foundation Bellagio Institute in the Italian Alps in 2002, I sat across from Harrison White with the unenviable assignment of serving as the commentator for his new book, *Markets from Networks* (White 2001). I had been surprised to see that his theory of production markets was built on a neoclassical microfoundation extending Michael Spence's signaling theory. I commented that this enabled him to proffer a deductive theory that was both predictive and general, specifying a rational choice microfoundation to explain the emergence of a production market organized in a pecking order of competing firms. I also noted that his work included a critique of Granovetter's network embeddedness approach, insofar as it limited economic sociology to a case-by-case incrementalist approach as in anthropology. This elicited a nod of agreement from White from across the narrow conference table.

Serendipitously, Barnaby Marsh at the John Templeton Foundation invited me to submit a research proposal to conduct a large-scale study of the emergence of a market economy in China. The timing was perfect. I had just completed a chapter for the *Handbook of Economic Sociology* (Nee 2005), edited by Neil Smelser and Richard Swedberg, which synthesized a decade of my work on a cross-disciplinary theory of institutions and institutional change. In a nutshell, in the new institutionalism, an institution is a dominant system of interrelated informal and formal elements: customs, conventions, social norms, cultural beliefs, and formal rules. Institutions are self-reinforcing social structures that provide conduits and channels for purposive action in pursuit of legitimate interests (North 1990, Powell & DiMaggio 1991, Evans 1995, Brinton & Nee 1998, Fligstein 2001, Ostrom 2005, Padgett & Powell 2012, Nee & Opper 2012). Hence, transformative institutional change necessitates not only remaking the formal rules, but also a corresponding realignment of interests, informal norms, and the balance of power in any economy and society.

In a lecture I attended, Cornell astrophysicist Steve Squyres detailed the thoroughness of preparation, design, and testing of the solar-powered rovers Spirit and Opportunity that were exploring the planet Mars. This was inspirational. An equivalent mission for the new institutionalism in economic sociology, I thought, would be to conduct an independent study of the birth of a new capitalist economic order in the People's Republic of China. There would not be another opportunity to study the social dynamics of emergence of this new economic order.

Why and how did a rational legal form of dynamic capitalism emerge in a state-owned economy under the leadership of the Communist Party of China? This was the question I posed in my research proposal. The Templeton Foundation awarded me two generous multi-year grants, which, together with supplementary contributions from Swedish foundations, funded a longitudinal research project extending my quest for a sociological theory of transformative institutional change. Sonja Opper, an economist at Lund University, had spent a semester at Cornell's Center for the Study of Economy and Society to work on China's economic reform. We seemed to share common assumptions about the nature of the social science, so I invited her to participate in this research project.

After my work on the 29-province China–Cornell–Oxford nutrition and health survey, I was aware of the limitations of such a massive study and had decided that a regional focus would provide a viable setting for a longitudinal analysis with causal depth. This involved traveling in Zhejiang, the southeastern province of the Yangtze Delta region, where I visited local market towns and emerging industrial clusters in the first year of the grant. Thick smog, dusty roads, trucks, and busy factories were telltale markers of early capitalist economic development. In that province, Protestant missionaries had established a strong and lasting Christian legacy in the port

cities of Wenzhou and Ningbo under unequal treaties imposed by Western powers. But in the Song and Qing dynasties, the Yangtze River delta had been the commercial center of imperial China and of neo-Confucianism. Prerevolutionary cultural beliefs notwithstanding, there was no pickup on my questions probing for patterns of behavior linked to the Protestant ethic, Buddhist religious beliefs, or neo-Confucian ethics in the founding of capitalist business ventures. In other words, a Weberian approach focusing on religious and cultural belief did not gain traction in the exploratory fieldwork.

What did attract my attention in my interviews were long narratives that were consistent with household individualism. Peasant entrepreneurs in poorer county towns and townships in Zhejiang detailed how starting a household business venture had been, for them, the only way out of agrarian poverty. Underlying poverty in southeastern Zhejiang was a long history of subsistence agriculture, along with the absence of state-owned enterprises. The Chinese state had not invested in industrial development there because this area was across the straits from Taiwan and considered a potential war zone. In our exploratory research we found a robust correlation between per capita poverty of subsistence farmers in poorer rural counties and early entry into private business by peasant entrepreneurs.

After exploratory research, it became clear that our research design needed to employ mixed methods. I decided to combine field research that entailed visiting a select group of private enterprises for on-site interviews with distribution of a lengthy two-part survey questionnaire, to be administered in successive waves (2006, 2009, 2012, and 2016/2017) to a random sample of 700 CEOs in the Yangtze Delta region.⁵ The 12-year study of the emergence of economic institutions of capitalism involved me in frequent travel to the region. I was lucky to have the support and trust of Lu Hanlong, director of the Institute of Sociology at the Shanghai Academy of Social Sciences (SASS), who introduced me to Sun Bocheng, the seasoned director of the SASS survey research unit. By then, I was myself seasoned in field research and experienced in the use of survey research and semistructured questionnaires. What I liked about working with Sun Bocheng was that he agreed to join the research effort because his friends and colleagues told him it was impossible to do. Why would any sensible CEO founder of a private manufacturing firm in the PRC led by the Communist Party participate in an American-led study of the emergence of capitalist economic institutions in China? After all, the stated objective of Deng Xiaoping's economic reforms had been to enable China's state-owned enterprises to recover from the chaos of the Cultural Revolution and resume their economic growth, fueled by a greater reliance on market mechanisms. The emergence of a dynamic form of private enterprise capitalism was not part of the plan.

Altogether, we did in-person interviews with 111 founders of private manufacturing firms, including some from among the CEOs randomly sampled by survey research and also others whom I interviewed while traveling with Brett de Bary and the documentary filmmaker Safaa Fathy in an industrial cluster near the Zhejiang–Jiangxi province border region, and in Shanghai and Jiangxi province. In 2008, Håkan Holm, a behavioral economist who was Sonja Opper's colleague at Lund University, joined our research team, bringing a laboratory-in-the-field component to our study. Experiments in behavioral economics (usually conducted in university laboratories) were implemented on location, as an integral part of the survey and interviews.

What explained the bottom-up endogenous emergence of the economic institutions of capitalism in China? *The New Institutionalism in Sociology* (Brinton & Nee 1998, pp. 19–45) had contributed to theory a focus on the social dynamics characterizing the interaction between formal

⁵The survey instrument can be found in the Appendix of *Capitalism from Below* (Nee & Opper 2012, pp. 265–349). We interviewed 111 CEOs and founders of private manufacturing firms in seven cities where a random sample of 700 CEOs were drawn.

rules of the state and social norms and networks at the micro- and mesolevels. China's economic reform led by the CCP and crafted and implemented by the State Council had legally restricted household enterprises to the level of a no-more-than-seven-employees cottage industry, with the intention of making private business ventures a mere appendage of a dominant state-owned economy. However, peasant entrepreneurs discovered that demand for consumer products in free markets far exceeded that small-scale household production capacity. To meet market demand, CEOs quickly grew their firms beyond the legal limit, risking closure and expropriation by the state for violating the law.

Opposition norms in violation of the restrictive formal rules and regulation of the state enabled a bottom-up construction of economic institutions beyond the shadow of the state-owned economy. Peasant entrepreneurs designed and constructed upstream and downstream economic institutions that enabled them to employ migrant laborers from inland provinces, to build private supply chains to break the state's monopoly control over raw material and nontradeable inputs, and to obtain from private sources the capital needed for the rapidly emerging private enterprise-led free market economy. New domestic and international marketing arrangements were forged to distribute consumer products manufactured by the new entrepreneurs. Welfare-maximizing oppositional norms enforced in overlapping networks of peasant entrepreneurs motivated solidarity, cooperation, and trust in the construction of economic institutions supportive of private enterprise. In a number of rural townships we visited, even the party secretary and his extended family were deeply involved in the private enterprise economy as entrepreneurs and workers. It was not uncommon to find villages and townships where nearly all adults were in one way or another participants benefiting from a flourishing private enterprise economy.

The immense profitability of private manufacturing led to its diffusion through increasing numbers of industrial sectors. The causal mechanisms we identified were market competition; entrepreneurial action; monitoring and enforcement of welfare-maximizing norms in close-knit industrial clusters; and mimicking by relatives, friends, and acquaintances of the pioneering first wave of private enterprise entrepreneurs. With diffusion and market penetration, a virtual social movement emerged in villages and townships, defined and led by entrepreneurially talented peasants from poor and modest socioeconomic backgrounds.

A Schelling diagram in *Capitalism from Below* (Nee & Oppen 2012) captured the tipping point where the sheer numbers of private enterprise entrepreneurs and the relative ease of joining a swiftly growing social movement spawned by the emerging private business ventures made it impossible for the state to enforce the legal rules limiting the size of private enterprise to seven employees. Sustained rapid economic growth, broadly based regional diffusion, and the profitability of private enterprise as a source of local wealth had tipped the balance of power decisively in favor of the booming private enterprise manufacturing economy and away from failing, loss-making, state-owned enterprises that were unable to compete. The hollowing out of the state-owned economy was evident in the hulks of bankrupt and abandoned state-owned enterprises where private ventures now rented space. This economic boom led by private enterprise generated the enormous accumulation of private wealth, nonfarm jobs, and regional economic development that greatly expanded the tax base of local and municipal governments in the Yangtze River delta.

Not surprisingly, the central government moved to accommodate transformative change in the real economy ex post facto by making sequential legal reforms that legitimized private enterprise and endowed it with legal status equivalent to that of state-owned enterprises. The constitutional rights that the state enacted and implemented as law provided the foundation for a rational-legal capitalism led by both the private enterprises and public corporations listed on China's hybrid stock exchange. Thus emerged a new engine of economic growth not only in China but also in the global economy.

7. CONGRUENT NORMS, COOPERATION, AND TRUST IN THE EMERGENCE OF A KNOWLEDGE ECONOMY

Capitalism from Below (Nee & Opper 2012) provided confirmation of market transition theory within a cross-disciplinary, multi-level new institutionalist framework that, I surmise, will stand the test of time. After so many trips from Ithaca to the Yangtze River delta two and sometimes three times a year, I was exhausted from long-distance air travel. I turned almost immediately to a follow-up study closer to home, in New York City. I was in pursuit of a parallel theory of institutional change in the bottom-up emergence of a twenty-first-century knowledge economy, where social norms of close-knit overlapping networks are generally congruent with the formal rules of advanced liberal capitalism of the United States. I saw this as a logical follow-up to the theory of oppositional norms explaining the bottom-up emergence of economic institutions of capitalism in China.

Social norms congruent with the formal rules of organizations are commonplace in advanced economies. Examples abound: the social norm prohibiting smoking in the workplace and public places such as airports, for instance. The new institutionalism in sociology argues that norms congruent with formal rules of institutions contribute to higher organizational performance by lowering the cost of monitoring and enforcing formal rules (Nee & Ingram 1998). Congruent norms and networks at the micro- and mesolevels complement the formal rules of organizations and institutions, enabling and motivating high economic performance.

Again, I found a useful example in Cornell's high-energy physics CLEO collaboration. For nearly 30 years, from 1979 to 2008, it had been the highest-performing research lab funded by the NSF's physics program. But with 22 physics departments participating in its study of the charm quark and more than 250 names listed in alphabetical order on all publications, the absence of individual recognition of contributions at first seemed to me incongruent with CLEO's high level of organizational performance. The research for a paper submitted by CLEO to the *Physical Review* could be based on one or two doctoral dissertations, with research reports written up by a faculty committee. When the reports were published as an article, the names of the physicists who did the research, as well as of those who wrote the reports, were unidentifiable among the long rows of physicists listed in alphabetical order according to their university affiliations. Given the anonymity of those who did the work, I was curious as to what explained CLEO's success. I came to understand the Wilson Synchrotron Laboratory as a complex organization in which all members who joined the lab—whether involved in the design, building, and maintenance of equipment; the operation of its accelerator; the collection and organization of data; administration of the lab; or preparation of working papers and research reports for publication—were seen as contributing to its success.⁶ The division of intellectual labor was essential for its operation. If the publications were authored only by a select few, a free-rider dilemma could arise, and then the social norms of cooperation and communal ownership of intellectual property would not be able to sustain such a close-knit social fabric. In a close-knit international community, cross-cutting and overlapping networks served as channels of information flow for not only knowledge but also the reputation of members of the CLEO multi-university collaboration. Robert Merton was the first to notice what he labeled as a “communist norm” that mandated sharing of “substantive findings of science. . . assigned to the community. . . The scientist's claim to ‘his’ intellectual ‘property’ is limited to that of recognition and esteem” (Merton 1942, p. 121).

⁶Cornell's Department of Physics was very strong on machine building, which is inherently a cooperative effort. The experimental group was led by Nobel laureate Robert Wilson, who later founded Fermilab.

I launched my research in Manhattan in the fall of 2012 after returning from a visiting professorship at the new New York University (NYU) Abu Dhabi campus. The dot-com stock market crash of 2000 had led to the collapse of a fledgling tech economy in midtown Manhattan, and only a few tech firms survived. Following the terrorist destruction of the World Trade Center the next year, the municipal government led by Mayor Bloomberg went into full crisis mode. Then the exogenous shock of the Great Recession of 2008 greatly exacerbated the situation. Manhattan's venerable investment banks went belly-up, leaving tens of thousands of financial services professionals unemployed. With the collapse of investment banking, which had been a crucial pillar of New York City's economy, this core financial service industry was lost. Mayor Bloomberg surmised it was not likely to recover from the multiple bankruptcies. But while the American main street economy was in deep recession, the New York tech economy was experiencing a bottom-up endogenous emergence and rapid acceleration, spewing out new startup tech firms and private sector employment of knowledge workers. By the time I started my field research, there were over a thousand new startup technology firms located in midtown to lower Manhattan and Brooklyn.

I recruited two Cornell seniors to go through this entire population of startup tech firms looking for the names of the CEOs who were tech startup founders and the addresses, websites, email addresses, and telephone numbers of their startups. Todd Bridges, a postdoctoral fellow funded by the NSF, joined me in the initial round of interviewing. Eventually there were interviews at more than 110 startup tech firms—some occupying small, coworking office spaces and others, more substantially developed, with spacious offices.

Interestingly, my exploratory field research visiting and interviewing CEOs of startup technology firms in Manhattan revealed a level of trust and cooperation similar to that of Merton's "communism in science." The tech entrepreneurs recounted numerous times when they reached out to help other tech entrepreneurs without anticipation of benefit to themselves. Surprisingly, the norms of cooperation and trust motivating them in helping others were based not on direct reciprocity, as in exchange theory, but on a cultural belief that a rising tide lifts all boats. With respect to open access, equal opportunity, and intellectual property rights, these norms were congruent with formal rules of American liberal capitalism. As in China, the bottom-up emergence of a twenty-first-century technology-enabled knowledge economy in Manhattan and Brooklyn had depended on neither federal and municipal government support of industrial policy nor change in existing formal rules. What mattered was an endogenous social process of self-help within a close-knit community of technologists and entrepreneurs in startup tech firms.

An economic institution playing a significant role in promoting this sense of community has been the monthly NY Tech Meetup, co-organized by Scott Heiferman and associates in 2004, using his new online platform Meetup (<https://www.meetup.com>). From the initial small group of technologists gathering in Heiferman's office, the monthly assemblies grew rapidly, coming to occupy the largest auditorium in Manhattan, on the NYU campus at Washington Square. At these town hall-style meetings, six to eight entrepreneurial teams present their innovations and business models to an audience of 800 fellow technologists, entrepreneurs, angel investors, software programmers, and job seekers, as well as interested faculty and students. The audience's designated role is to ask questions and provide helpful feedback to the startup teams. A reception following the assembly provides opportunity for participants to interact face-to-face, exchanging ideas, knowhow, practical advice and often business cards.

My fieldwork in New York City underscores two key social processes: (a) knowledge spillover and (b) network rewiring, defined as social exchange creating a new social relationship to facilitate ongoing exchange of ideas and knowhow. Knowledge spillover and network rewiring are social mechanisms, I conjectured, that provide a parsimonious explanation for the high level of

innovation activity I observed in my fieldwork and, more broadly, for the swift bottom-up emergence of the New York City technology-enabled knowledge economy (Nee et al. 2023). To date, six research reports have been completed and a deductive theory with explanation has been sketched and confirmed with evidence.

8. CONCLUSION

In retrospect, I have the sense that the itinerary of an American sociologist I have just recounted involved an adventure in which, learning from failure and benefiting from serendipity and just plain luck, I managed to find various ways to unlock the doors of discovery. Over the long haul, a discovery for me has meant finding explanations of the workings of institutions and institutional change. Explanation requires a theory that specifies mechanisms with clear scope conditions that enable prediction and confirmation with empirical evidence. My cross-disciplinary quest to address the puzzle of transformative institutional change has utilized tools of inductive inquiry. Fieldwork with observation and in-depth interviews has proven indispensable in revealing sometimes counterintuitive drivers and patterns of social behavior that elude the grasp of quantitative measurement. This revelatory capacity characterizes inductive inquiry in fieldwork as an investigative method that can aid and abet the gestation of a grounded theory. The crucial principle here is to rely on theory, not just a review of the literature, as the basis for explanation, prediction and confirmation with evidence.⁷

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⁷About this, I am in broad agreement with Duncan Watts (2011) and Richard Swedberg (2014).

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