Carl and Mike, circa 1987

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CARL J. COUCH AND THE IOWA SCHOOL: IN HIS OWN WORDS AND IN REFLECTION

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STUDIES IN SYMBOLIC INTERACTION VOLUME 49

CARL J. COUCH AND THE IOWA SCHOOL: IN HIS OWN WORDS AND IN REFLECTION

EDITED BY

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United Kingdom – North America – Japan
India – Malaysia – China

Emerald Publishing Limited
Howard House, Wagon Lane, Bingley BD16 1WA, UK

First edition 2017

British Library Cataloguing in Publication Data
A catalogue record for this book is available from the British Library

ISBN: 978-1-78743-166-9 (Print)
ISBN: 978-1-78743-165-2 (Online)
ISBN: 978-1-78743-188-1 (Epub)

ISSN: 0163-2396 (Series)

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SERIES EDITOR PREFACE

It is an honor to have this special volume that includes a complete version of Carl Couch's unpublished memoir, *The Romance of Discovery*, in Studies in Symbolic Interaction. This is a seminal volume. It significantly advances our understandings of the origins, forms and struggles involved with the emergence of the New Iowa School of Symbolic Interaction that emerged in the 1970s in the Sociology Department at the University of Iowa. Under the charismatic leadership of Carl Couch this new paradigm grounded symbolic interaction studies in fine grained, close-up audio-video studies of social interaction.[1] The studies were done in the Center for Research on Interpersonal Behavior (CRIB). Couch's students wrote several MA theses and PhD dissertations using materials generated in CRIB.

Michael Katovich's "Editors Notes" bring Couch alive – larger than life actually. As Mike and Shing-Ling Chen note, Couch was the father of qualitative laboratory research. He advocated a processual view of human life offered by George Herbert Mead. His students brilliantly implemented this approach in their CRIB studies. Couch's theoretical formulation and research designs were a breakthrough on several levels, showing that a second-by-second process view of social was valid, and of great importance.

Sadly Couch's work (and the work of his students) was not supported by his positivistic departmental colleagues. It was encased and suffocated in a hostile, monolithic intellectual environment. By the mid-1980s research in CRIB ebbed to almost nothing, Couch died in 1994, never to see the gradual acceptance of the processual view he had so brilliantly advocated. *The Romance of Discovery* tells this sad story. Reflections from nine of his former colleagues, students, and a family member shine a light on Couch, the man, the mentor, the father the best friend any person could ever have.

Mike's behind-the-scenes story of the writing of the *The Romance of Discovery* adds depth, nuance, and pain to the story. Mike and Shing-Ling's fine introduction and final assessment set the record straight. They challenge a new generation of interactionists to always be critical of those who fail to address the interactional conditions of everyday life. The vibrancy of symbolic interactionism resonates in that challenge.

In closing I thank Mike and Shing-Ling for their moral and editorial leadership and I thank them and their colleagues for this important contribution to studies.

Norman K. Denzin
Series Editor

VOLUME EDITOR PREFACE

From the first time I met Carl J. Couch, at drunk-o'clock in a defunct Iowa City bar called Mama's, until a few days before his death, when we talked for the last time (via phone), I saw and heard a man forever in love with his work and profession. I know he loved many people, especially his wife Dee-Dee and his children, but I always associated Carl with his love of work. He would work anywhere, but my most vivid memories take me back to his office in MacBride Hall – which we called "Bird Hall" due to the presence of several Audubon-like stuffed birds lining its hallway on the second floor – which was also Manford Kuhn's office while Carl studied at Iowa as a graduate student. Later, Carl and the Department of Sociology moved to Seashore Hall and I visited him there a few times, but even though the love of work continued, the place just did not seem the same.

Carl's decision to write what we have titled, *The Romance of Discovery*, began sometime in the late 1970s with a manuscript written in his McBride office. The manuscript, "Depictions of social types in 'Bird Hall'" (or so I remember), served as a portrayal of four types of academics who occupied the Sociology Department in Bird Hall, but that could also represent any Department in any University. One of the types, *the prima-donna*, pertained to the difficult academic/scholar who, although disliked by some and considered egomaniacal by others, managed to maintain at least some integrity when engaged in work. Carl put himself in that category. Another classification, *the charlatan*, came off as a crass careerist who had managed to work the system well enough, including forming important allies, to survive and thrive. It should be no surprise to those who knew Carl that he considered the majority of his colleagues as exhibiting the qualities of such a type.

When Carl made mimeographed copies of his categorizations, he disseminated them to trusted colleagues and students. I regarded his characterizations as pessimistic – not even the *prima-donna's* could be considered heroic in the Hollywood sense of the term – but I also considered them more sophisticated than Merton's categorizations of the deviant or Lofland's scheme of graduate students. I urged him to rewrite it, emphasizing how the elements and structure of openings, which had become our anchor, could apply. Maybe, I thought, he could even bring in a more tragic component, as when say, a *prima-donna* "sells out" to become a *charlatan*. As usual, Carl appreciated my feedback (he would often hand a manuscript to a student or colleague and say, "Here, kick the **** out of it!"), but I do not know if he ever incorporated any of my ideas.

I am pretty sure, though, that once he received enough feedback to suit him, he began, sometime in the early 1980s, to transform this manuscript into the first version of his book, which I believe he titled, *The Romance of a Profession*.

I never received *The Romance of a Profession*, but apparently, Carl credited me with encouraging him to write it. I found this out, one night, when a very prominent and prolific symbolic interactionist phoned me – late at night. The call awakened me and as I did not really have much contact with this person, I could not recognize his voice immediately. I did recognize his tone – one part anger, one part frustration, and one part parental concern – and noted his major reason for calling: to talk Carl out of trying to publish the now book-length manuscript. Since I, for one thing, had not read this particular manuscript, two, did not know the caller all that well, and three, suspected that said caller had read the vast majority of the manuscript under the influence of alcohol, I acted in a dismissive way. I felt a little guilty for my blasé attitude, especially considering the stature of the person on the line, but I figured we could rationalize my lack of urgency as correlated with recently awoken behavior, which fits somewhere in the elements of sociation.

The call did pique my curiosity, however, and the next day I called Stan Saxton to relay its content. Stan only kept muttering, "Yeah…yeah…yeah." When I asked Stan if the manuscript was as bad as the caller intimated, again Stan said, "Yeah," adding, "It's pure Carl, but it's also slanderous." Apparently, the four types of generic creatures that haunted Bird Hall had become flesh and blood characters with names; ones who did not hesitate to hurl "slings and arrows" at Carl or his students. Stan added, "By the way, you're mentioned in the book…we all are."

A year or so after disseminating *The Romance of a Profession*, Carl had responded to criticism from selected core students (including Stan) and interactionist colleagues, although I cannot say if my late-night caller shared any of his concerns with Carl directly. Rather than tone down his apparent vitriol, Carl upped the ante (he did love to play poker!), and sent his revised manuscript, *The Politics and Passion of Discovery*, to a broader net of people, including me. Stan, in particular, expressed consternation about the unapologetic revision. He felt that publication of this manuscript would not only create trouble but, more importantly, damage Carl's reputation. Stan could always talk frankly to Carl and Carl respected what Stan had to say. He listened, agreed for the most part, and explored publishing routes. To Stan's relief, Carl did not experience any success in finding a willing publisher.

Even though Carl remained convinced that the manuscript needed an outlet, his enthusiasm for publication had begun to wane. For one thing, he published an important book in 1984 (important to him, his students, and interactionists), *Constructing Civilization*. Then, in 1986, Carl, along with Stan Saxton and I, published (as co-editors) a Supplement to Norm Denzin's *Studies in Symbolic Interaction* series, titled, *The Iowa School*. In this supplement, Steve Buban, writing about revisiting the Chicago and Iowa School dichotomy, coined,

The New Iowa School, a term that seemed to energize Carl. One year later, in 1987, Carl published *Researching Social Processes in the Laboratory*, summarized, in part, in Appendix A of this book. In addition, Carl continued to publish several articles and book chapters throughout the 1980s and into the early 1990s, including a piece on using the past to situate action that I cowrote with him in 1992, published in *Symbolic Interaction*.

Further, Carl also began to get his share of deserved recognition, some of which involved bureaucratic responsibilities. The Department of Sociology at Iowa finally recognized him as worthy of promotion to Full Professor (thanks, in part, to a letter-writing campaign headed by Stan Saxton and Norm Denzin). After serving as President of *The Society for the Study of Symbolic Interaction* (SSSI), he received, in 1992, SSSI's most prestigious *Mead Award* for lifetime achievement as an interactionist. He did not take this award lightly and I believe he felt validated by it. As a longtime member of the *Midwest Sociological Society*, Carl finally became its President in 1993–1994, a position he relished and spent a great deal of time honoring. The manuscript, whatever its title, had become back-burner material.

Even so, Carl would return to it on and off, spending his summers in his and Dee-Dee's mountain cabin in Montana working on it into the 1990s. Carl trusted the people who leveled the most frank (and fair) criticism of his work and, consistent with this orientation, he relied a lot on Stan Saxton's feedback. Stan informed me that he tried to impress on Carl that the manuscript needed to focus more on the specifics of the passion, while noting, in the most general way possible, the politics that often undermined Carl's vigor. Stan also spent a good deal of time and energy convincing Carl to "sit on" the book until he could gain some emotional distance so as to rewrite it with a little more objectivity. By this time, however, Carl had fallen ill. Not long before his death, in a moment of slight dejection, he called the book, "an exercise in catharsis."

After Carl's death, Stan tried to edit the book with the objectivity that he had implored Carl to use. Stan told me that his first order of business would involve removing all specific references to Carl's antagonistic colleagues and particular details that in Stan's eyes seemed petty. I was more than happy to have Stan as the Editor, but after a year or so, he gave up on the task, convinced that the Carl he knew and loved could not emerge full bodied in this memoire. "He's too angry," said Stan. "He should have dictated it to someone who could have reined him in." I did not respond to Stan's assessment, but I did not forget it either.

Whatever specific changes Stan made or tried to make either vanished through the donut hole or got lost in his own chaotic paper shuffle. When Shing-Ling Chen proposed a revised edition that we would title, *The Romance of Discovery*, Bob Hintz seemed to be the only person who had retained a copy of, *The Passion and Politics of Discovery*. Neither I nor Shing-Ling could find any of Stan's proposed changes. With Bob's copy made available, Shing-Ling altercasted me in the role of Editor, agreeing to be my coauthor in an

Introduction which will follow these notes and giving Norm Denzin the task of writing a *Preface*. I took a harder look at the manuscript, wearing the Editor's cap, one that Carl wore frequently each time he pored over one of his student's pieces. I reimagined Stan's words; both Carl and Stan have passed away and all I can really do is reimagine each of their words. In light of those words, I put myself in the place of one "dictated to." I heard Carl's gravelly voice telling this story, true to his own heart. I began to interrupt, here and there, asking myself (or was it Carl?), "Do you really want to include this?" or, "Do you really want to be this explicit?" Or, "Do you want to put this sentence another way?" After a while, I began the editing job in the way Stan originally suggested – but with a little more social support, especially from Shing-Ling.

What follows, forming Part 1, after Shing-Ling's and my *Introduction* is Carl as Carl, but a little more costumed. I would imagine that if he magically reappeared and got his hands on this manuscript, he would have the same thing to say about it as Huck Finn said of Mark Twain's *Tom Sawyer*, "It's the truth, mainly." Part 2, *Social Scientific Foundations of Discovery* includes Carl's unpublished essay "Forms of Social Processes." It provides a systematic statement by Carl in regard to the primary foci of the New Iowa School, including methodological procedures that can record social processes as they occur, an emphasis on conceptualization, and an overall theoretical argument that two and three person groups are as real as individuals. In Part 3, former students (Ron Neff, Bob Hintz, Richard Patik, Shing-Ling Chen), colleagues (Jeff Ulmer, Mike Flaherty, and Steve Wieting), and one of Carl's children, Mike Couch, provide glimpses into Carl's self – as a teacher, mentor, friend, intellect, and father. Finally, a selected bibliography list works that Carl deemed significant to the emergence and sustainability of the New Iowa School.

I think if those who knew Carl well read it, they will hear that gravelly voice, imagine the shock of white hair, and see the black horned rimmed glasses. My hope is that people will see Carl as I and the aforementioned others saw him – as one capable of inspiring others and then being inspired by those same others. In particular, I ask the reader to imagine the arc of a mythological hero, as one who returned to the place of his intellectual birth and occupied the same office as his intellectual father so as to begin singing when ready to present others with his song.

Michael A. Katovich
Editor

INTRODUCTION: FROM GALILEO GALILEI TO CARL COUCH: PARADIGM SHIFTS AS POLITICAL STRUGGLES

In his lasting and influential thesis, Thomas Kuhn (1962) emphasized the concept of a paradigm shift, noting that all fields and practices of scientific inquiry undergo new approaches to understanding what scientists would never have considered valid before. Kuhn notes that such shifts rarely, if ever, begin as celebrated events. At any given movement of inquiry, the dominant view of science, or what Kuhn termed normal science, would have enough allies and practitioners to impair or even abort any radical discussion of anomalies and incongruities, making the one or ones affiliated with a shift pariahs at best, and sacrificial heretics at worst. From Kuhn's perspective, scientists maintain their disciplines via monopolies of perspectives, making any consideration of competing paradigms incommensurable with the acceptable (and uniform) missions. Any attempt to validate an alternative paradigm would, almost by initial definition of this alternative's premises, become hostile to normal science and in effect, hostile to the ethos of a scientific methodology. Any alternative seen as, potentially, a competing paradigm of thought would represent an irreconcilable account of a reality that many depended on for careers, livelihood, and prestige, among other things.

In Kuhn's formulation, scientists do not choose to ally themselves with a paradigm by and through consideration of rational premises or by honoring the importance of evidence that might contradict an accepted way of viewing the world. By implication, what constitutes scientific truth is not established by ongoing objective evidence, but rather by a consensus of what constitutes implied objective evidence by a scientific community (Mead, 1929). Accepting a paradigm requires a faith in the politics of any scientific enterprise rather than a commitment to alter any premise on the basis of emergent data. In effect, science is not a smooth and steady accumulation of knowledge. A more realistic characterization of the history of science would incorporate a metaphor of a battle field of political struggles among various intellectual options.

From Kuhn's perspective, the prototypical anti-normal scientist and paradigm buster, Galileo Galilei (1564–1642), not only challenged scientific convention, but became a public enemy of a very strong institution (Catholicism), creating a dual-scandal of heretic thought. His assertions, of course proven true

in the long run, nevertheless breeched what conventionalists held sacred in regard to their obdurate and crystallized perceptions. In maintaining faith in the scientific ethos itself, Galileo Galilei became an anti-heroic advocate of what Dewey (1941) would later call warranted assertibility. While Kuhn would place others in the category of heretic in regard to science, using Galileo Galilei as the "bar" (or perhaps, paradigm), he had no idea of another social scientist, Carl Couch (1925–1994), who practiced and taught his craft at the University of Iowa. In effect, while Kuhn and others might find the comparison between Galileo Galilei and Carl Couch somewhat specious, even preposterous, we see two scientists who advocated what appeared to be revolutionary procedures in the name of science in their lifetime.

While Galileo Galilei provided us with the most famous case of ideology strangling scientific inquiry, Carl Couch provided his students with a fine grained view of the construction of a particular paradigm of thought in the social sciences; facing consequent suppression by his colleagues and confronting, on a day-to-day basis, the harsh circumstances of response amid the grand possibilities associated with his findings. Carl Couch will never be known in grand terms, as was Galileo Galilei, who became the "father of observational astronomy" and promoted a motion theory of planets (in opposition to the dominant static theory of planets). Nevertheless, from our perspective, and from our experiences as those who took Couch seriously as the "father of qualitative laboratory research," his students who wrote several MA theses and PhD dissertations contributed to promote a processual theory of human interaction, against the dominant static view of interaction.

THE GALILEO AFFAIR

Galileo Galilei improved the newly invented, but primitive, telescopes in Europe in 1608, and began astronomic observations in 1609. On January 7, 1610, with improved telescopes of his own designs, he observed Jupiter, and found three small, bright moons near the planet. One of the moons was off to the west of Jupiter, while the other two were to the east, and all three moons were in a straight line. The next evening, Galileo found that the three moons were now west of Jupiter, but still in a straight line. He discovered that these moons orbited Jupiter. With this discovery, he concluded with a theory of motion in the Solar system. Based on this discovery and others, Galileo published (in 1610) *Sidereus Nuncisu* (*Starry Messenger*), the first published scientific work based on observations made through a telescope. In this work, he described the discovery of Jupiter's orbiting moons, supporting a heliocentric view of the universe, describing the Earth and the planets as revolving around the Sun at the center of the Solar system.

Galileo's heliocentric view was, to say the least, controversial in his lifetime, challenging the dominant geocentric view, or a description of the universe holding the Earth at the center of all the celestial bodies. Confronted with oppositions, Galileo did not remain quiet and instead intensified his heliocentric campaign. In meeting with fellow scientists, Galileo found that some of the scientists refused to even look through the telescope (Sharratt, 1994). Despite rejection, he remained vindicated as he felt that scientific inquiry supported his evidence. Instead of apologizing for his "observational error," Galileo became blunt and sarcastic, engaging in bitter feuds with fellow scientists and making enemies.

Galileo's advocacy of the heliocentric view was also met with hostility from the Roman Catholic Church. Church authorities subscribed to the geocentric view, as it concurred with their Holy Scripture. Facing the criticism of heresy, Galileo went to Rome in 1615 with an attempt to clear his name and make his case. However, his effort was in vain. In 1616, the Roman Catholic Inquisition declared any heliocentric view to be formally heretical. In addition, Pope Paul V. ordered Galileo to abandon his view, stating that if Galileo resisted the decree, stronger action would be taken. Based on the order, Cardinal Robert Bellarmine ordered Galileo to refrain from holding, teaching, or discussing anything to do with a heliocentric view, banning all books subscribing or sympathetic to such a view as well.

Despite facing prospects that would not only discredit him but also end his life, Galileo continued to champion his perspective. In 1632 he published *Dialogue Concerning the Two Chief World Systems*, a passionate defense of the heliocentric view, complete with evidence that he obtained via modern technology. In 1633, Galileo was ordered to stand trial on the suspicion of heresy. Galileo was interrogated with a threat of physical torture (Finochiario, 2007). Galileo was found guilty of advocating heresy with vehemence and sentenced to indefinite imprisonment (Blackwell, 1991). Galileo was kept under house arrest till his death in 1642. He did not live to see the gradual acceptance of the heliocentric view in the late 1600s.

THE COUCH AFFAIR

In the social sciences, following post World War II melodrama and atrocities, and amid a burgeoning revolution in media technology, some observers began experimenting with audio–visual technology. The development of video recording technology for purposes of reexamination and retrieval of data began in earnest around the mid-1950s. By the mid-1960s, video recording technology made its way to businesses and educational institutions for varieties of purposes, including role playing, precise reexamination of nonverbal responses and cues, and conversation analyses. During this time, The Center for Research

on Interpersonal Behavior (CRIB), a research laboratory with video recording capacities, was established in the University of Iowa. While Carl Couch did not establish CRIB, he became the instrumental audio–visual researcher, convinced that scientific inquiry = y into interaction processes could become more precise with the capacity to research that which observers usually saw for one time only. Along with his associates, colleagues and students, Couch formulated a new approach in studying social interaction that highlighted systematic inquiry into social processes-associated interaction and repeated viewing of such processes to detect changes in interactional dynamics that the naked eye, viewing one time through, could not detect. CRIB became the birthplace of New Iowa School of Laboratory Research, a breakthrough in laboratory research that emphasized agency and social interaction.

As with many symbolic interactionists of the day, Couch advocated a processual view of human life, offered by George Herbert Mead. Unlike many symbolic interactionists, however, who took for granted that interaction proceeds toward broader changes in face-to-face encounters. Couch's processual view maintained that significant changes occurred in the second-by-second nature of interactional processes. Such changes could be detected via audio–visual recordings. From his perspective, the dynamic milliseconds of change that altered interactional episodes became supported by the processual video data generated in CRIB. Couch led a group of intelligent and dedicated graduate students. The research conducted in CRIB in the 1970s were ground breaking, as CRIB researchers were able to capture processual data that escaped one-and-only-one-time viewers. In effect, Couch claimed that he and his researchers, using audio–visual technology, could identify heretofore "invisible" sequences of social interaction, unattainable in the past using naked eye observation. For the first time, social interaction was captured in its entirety using video recording. Not only that, such capturing would allow for playing back sequences for re-analysis and to identify, possibly, other sequences of interaction with a great precision.

Couch's theoretical formulation and research designs were a breakthrough on several levels. For one, he showed that a second-by-second processual view of social interaction was not only valid, but also fruitful. In addition, he provided clear evidence that rapid sequences of social interaction could be identified with a great precision and could correlate with significant alterations that could change naked-eye analyses. However, such a processual view of social interaction was ahead of his time. For one thing, conventional symbolic interactionists, sympathetic to Couch's processual view, nevertheless regarded the audio–visual capturing of such data as part of a broader technological-positivistic endeavor. On the other hand, the dominant view in sociology (that subscribed to technological-positivistic approaches, but not audio–visual technology as much as computer technology) advocated a static positivist view of stimulus and response. Further, and as Couch's remembrances that follow

show, he and his associates were met with opposition from the faculty in his own university.

Couch did not abandon the contemporary scientific institution entirely. Unlike Galileo, he did not see himself as above the institution. Instead, be attempted to impress upon institutional others the worthiness of his project. In effect, he believed in obtaining outside support to help validate his breakthrough. To seek external support, Couch met with the administration in National Science Foundation (NSF). However, as the NSF administration had little-to-no idea what Couch saw as significant, and as it had, traditionally, supported more quantitative studies, the NSF administrators were unable to understand Couch's processual qualitative approach.

Adding to the sting of rejection, and as mentioned, oppositions against Couch's works by his own departmental colleagues turned into hostilities. Antagonism escalated even though Couch and his associates became increasingly productive and successful. As mentioned, several students, creating a "new-normal" science of their own, produced MA and PhD documents, making a "new Iowa School" a going concern in the Department. Even so, and as Couch points out in later chapters, numerous institutional measures emerged to suppress the works by Couch and his associates, and harass the personnel associated with Couch. Through modifying curriculum, Couch's colleagues created an intellectual environment friendly to the learning of the traditional static view of sociology and hostile to the learning of alternative processual view. Through selective admissions, positivistic researchers ensured students admitted would contribute to the growth of the static view of sociology. Selective granting of scholarships also fostered the growth of the mainstream scholarship, and hammered alternative lines of inquiries. Through selective hiring, processes of granting tenure and promotion, not to mention other harassing measures such as rumors, unreasonable demands, or unfounded accusations, a monolithic institutional culture was created favoring the dominant static view, hostile to alternative approaches.

In the end, Couch and associate's ingenious works were encased, and suffocated in a hostile and monolithic intellectual environment. Although, as mentioned, Couch and his students engaged in active and productive research in the 1970s, research in CRIB was ebbed to almost nothing in the mid-1980s. Couch passed away in 1994, did not live to see the gradual acceptance of the processual view and qualitative research, nor did he see the implementation of the New Iowa School laboratory research in the University of Northern Iowa, in the late 1990s.

CRISIS MANAGEMENT AND POLITICAL STRUGGLES

Despite obvious differences in scope, historical context, and fame, we see some striking similarities between the Galileo Affair and the Couch Affair. First of

all, both illustrate that technological advancement is a precursor of scientific advancement. The development and improvement of the telescope paved the way for Galileo's discoveries. The availability of the video recording technology allowed Couch and his associates to obtain their breakthrough. In addition, both Galileo and Couch faced oppositions from the mainstream researchers of their time, who subscribed to normal science, a geocentric normality in Galileo's era, and a positivistic normality in Couch's time. Both engaged in campaigns to advocate their respective views, both were confident in their endeavors, and both insisted on backing their claims with clear evidence. The evidence, however, were seen as anomalies by the mainstream scientists. Feuding with mainstream scientists of their time, Galileo and Couch were both blunt and critical, which aggravated antagonism. Their gruff and unembellished attitudes were a true testimony of the confidence they experienced regarding the clear evidences they had at hand. As Couch indicated, not believing in the obvious evidence would be insane, a remark that in all certainty, did not endear him to his critics.

Acting on frustrations by not being acknowledged by their fellow scientists, both Galileo and Couch sought outside support. Galileo went to Rome, and Couch visited NSF in Washington D.C. As each of the institutions had no real idea of what the new research was all about, both found the meetings pointless, and their efforts in securing outside support were fruitless. The evidences that Galileo had, and the data that Couch obtained might be indisputable. However, what Galileo and Couch were not aware was that science did not rely on objective evidences alone, and scientists did not choose a paradigm rationally, as Kuhn pointed out. Paradigm shifts are political conversions. Subscribing to a paradigm requires a faith in all of its fundamentals. When anomalies occur, scientists ignore, disregard or relegate them. Kuhn explained this kind of dogmatism in the science community as a natural occurrence. He noted that no matter how great or numerous anomalies were present, mainstream scientists would never lose faith in the established paradigm. To lose faith in the paradigm, Kuhn continued, would mean ceasing to be a scientist in the traditional sense that such a scientist understood the term.

However, when anomalies accumulate, they push normal science into a crisis. Galileo's works and influences created a crisis for the normal scientists of his time, as well as the Roman Catholic Church authorities. In Galileo affair, we observed the efforts of crisis management by geocentric scientists and church leaders. CRIB and all the research conducted in it by Couch and his associates definitely fomented a crisis in the eye of his colleagues, although Couch's impact did not achieve the immense admiration of Galileo's impact. In the Couch Affair, as Couch explains it in the following pages, we witness various measures in which his colleagues engaged to manage the crisis.

One other difference between the Galileo Affair and the Couch Affair, which highlights the importance of this volume, is that with Galileo, he alone became the target of oppression. In regard to Couch, the target of harassment included

an entourage, a group of researchers working with Couch. Orders and judgments issued by the Church mainly served to limit the activities of Galileo, evidenced by his house arrest. Although Galileo had students, allies or patrons who supported his efforts, no literature available indicated that these aforementioned individuals suffered significantly due to their association with Galileo.

In this volume, readers can find detailed accounts of various crisis management tactics, concocted to suppress individuals associated with the enterprise initiated by Couch. Not only was Couch a direct recipient of various harassing efforts, but also his colleagues, graduate students, and potential applicants were affected as well. Even those who supported Couch, while sometimes vocal and ready to come to his defense, often remained as bystanders, unable to ward off the hostility that some others directed at Couch and his students. In effect, readers interested in the construction of crisis management in normal science would find this volume a fascinating read. It is interesting to note that sociologists who examine social life are in turn, a subject of examination. Most importantly, while Kuhn's book introduced a realistic humanism into the study of science, Couch's writing has introduced a realistic humanism to the understanding of academia.

Michael A. Katovich
Shing-Ling S. Chen

NOTE

1. Disclosure: Couch was on my dissertation committee, although we did not work together. CRIB had not yet been created. (I called it green-carpet sociology and he called me a "dumb s*on of a b****h*." We were close friends. We were there when The Society for the Study of Symbolic Interaction was formed. We fought many battles, fished for trout in Montana rivers, shouted at one another in countless sessions at professional meetings. Mike Katovich's Editors Notes captures Carl's passion; I miss him to this day.

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PART 1
THE ROMANCE OF DISCOVERY BY CARL J. COUCH

Editor's note: This memoir is a recollection of events that took place some 40 years ago. The opinions expressed are those of the author, Carl J. Couch. Where appropriate, to protect the anonymity of certain individuals, names have been omitted.

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PREFACE

I drafted an original version of this statement in 1984 and made a half-hearted attempt to have it published the following year. Several of my friends, whose judgment I respect, recommended that I delay publication. In the words of one, "A breakthrough is something recognized by others, not something one claims to have accomplished." Even if that is so, other factors, in my judgment, justify publication at this time.

Some of those who advised delay in 1984–1985 also stated that publication of this statement would embarrass me and my friends. If any of my friends feel embarrassed by this statement, I apologize. I do not anticipate personal embarrassment.

Only those directly involved have given any indication that an intellectual leap occurred significant enough to merit the term breakthrough. Despite overall skepticism from those I respect, and also from those who did not experience what I and a core group of graduate students experienced, I remain as convinced today as I was in 1974 and in 1984 that we achieved a breakthrough in the study of human social life. Further, I still hold to my claim (some might say my arrogance) that the breakthrough will have an impact on the social sciences equal to that of Mendel's impact on the biological sciences. At the very least, the breakthrough will have an impact on any discipline willing to study interpersonal communication and to deem such study as central to its core.

My desire to publish my claims outweighs my respect for my friends' counsel and my concern for their own possible embarrassment. It also outweighs what would probably constitute smart counsel to wait for greater acceptance. I suppose my decision to publish stems, at least in part, from an interest in heaping praise on myself. I cannot deny my pride in the part I played in the enterprise. However, I hope readers will view this statement as something more than egotistical bombast. At the minimum, it contains one person's description of a journey that involved a number of committed students and scholars that created a new way of examining social life. We emphasized the combined and coordinated activities of two or more people, rather than the activities of individuals.

At least two purposes beyond mere self-promotion informed the original drafting of this narrative. One includes my desire to bestow a modicum of recognition on those I judge responsible for the breakthrough. Second, I wish to emphasize the sacrifices associated with breaking new ground in the social sciences made by those who committed their callings and their careers to an approach to the sociology of human behavior. Many others took a more conventional path and either ignored this innovative approach or decided that it was too far outside of the mainstream. For them, the commitment to an alternative route constituted too much risk. As a corollary to these two broad purposes, this narrative also provides some data for those interested in the sociology of science, although I did not consider the provision of adding to the literature on such scrutiny as motivating me to draft the original statement.

The substantive content of this altered draft contains many similarities to the 1984 draft on which my friends and colleagues provided commentary. I have rewritten much of the draft, perhaps more than anything, to temper my more vainglorious assertions. Omissions of personal names and in some cases, private events that involved people who did not consent to be included in this book also distinguish this revision from the original draft. Most importantly, to impress upon the reader that our breakthrough had sociological substance and durability, I have added an Appendix (A) that attends to the methodology that defined the breakthrough and that also describes some of the theoretical content of the breakthrough. In any event, this narrative constitutes an insider's description and interpretation of events. If it suffers from anything, it suffers from the shortcomings common to such descriptions.

I have based my descriptions on unaided recall. I did not keep a diary and have no field notes, as a rigorous ethnographer would define them. I did make a few notes to myself and hung onto a few documents that verified and jogged my memory of the sequences of events leading up to the breakthrough.

Whether or not the accomplishment is as grandiose as I claim, the affair is still worthy of attention. A few months after the breakthrough, in a moment of doubt, I asked Marion Weiland, who will later appear as one of the key researchers involved in the breakthrough, if we had really achieved a paradigm change. Her response was, "Either we have or we are collectively insane, but either way we are worthy of our fellow sociologists' attention." Her assertion strengthened my own resolve for writing the following pages, providing me with the moral support I needed. Such bravado may only document how a small group of social scientists went collectively insane, but it may also demonstrate how a small collective altered the landscape of the study of interpersonal processes.

Perhaps the breakthrough will never be recognized; it may be that what those of us regard as a breakthrough was nothing more than a collective illusion. On occasion, human beings have so fervently wished for a state of affairs that they have deluded themselves into believing their wishes were reality. Many have so wished to speak with God with unabashed passion that they

have convinced themselves and, in some cases, others, that they did speak with God. As yet the scientific community has not validated such encounters; although lack of scientific validation has not precluded faith in other communities. Perhaps what we consider to be a breakthrough will never be validated by any communities. If the discovery is to have the consequences I foresee, others in addition to the small group involved in the discovery will have to validate the discovery.

This statement will not go through the painstaking delineation of the precise content of the breakthrough, nor will it elaborate on the specific conceptualization that makes up what I call a paradigm. Instead, I offer a description of events and conversations that I judge as relevant to building a foundation for a breakthrough. I stress the ethos associated with our accomplishments and my own interpretations of the transformation of my orientation to social psychology, which will include my take on the elementary forms of social processes. Hopefully, readers will gain appreciation of my intentions and efforts once they immerse themselves into my overall growth and development as a teacher and researcher. To familiarize readers with more comprehensive accounts of our research, I am attaching another Appendix (B) which demonstrates some of the implications of the breakthrough and attendant applications.

My position as the academic advisor to the graduate students who participated in our work makes me as qualified as anyone to offer descriptions of what occurred and judgments on who contributed what.I occupied a central position in the communication network of those responsible for the achievement.In my more pretentious moments I even regard myself as the catalyst for the accomplishment. Even so, I do not pretend to be responsible for the critical research, the reformulations of the questions, or how previous works became reconceptualized. The hard work of my students, their dedication, and the seriousness of their commitments made the breakthrough possible. Formally, I took on the role of teacher, but in day-to-day practice, I became their student, learning with them as we all went along.

I may have slighted the contributions of some. I apologize to those who feel slighted; perhaps they can offer their own account. Others may make different assessments and interpretations. Allowing everyone to have a say remains one of my most important values, but a time comes when something, even if it lacks unanimity, must be written. I feel strongly that the time has come for our breakthrough to be acknowledged in writing; bestowing credit orally and behind closed doors amounts to a shallow endeavor. I hope the reader considers this narrative an attempt to give a more powerful voice to our accomplishments. If the breakthrough as I see it turns out to resemble, in the eyes of others, an illusion, consider me one more academician who assigned great value to his or her work while resembling a lone academician who has lost his way.

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CHAPTER 1

BACKDROP

Carl J. Couch

The breakthrough achieved at the University of Iowa in 1974 rested on an intellectual foundation provided by scholars of preceding generations and concurrent efforts by other scholars at other locations. Many scientists and sociologists of science have noted that all research endeavors involve embarking on studious paths that either current colleagues or deceased and distinguished researchers forged. We all owe debts to fellow and fallen explorers. With apologies to Erving Goffman, no scholar worships at his own shrine.

The idea of a science of the social, what became known as sociology, gained articulation through the efforts of the positivist philosopher August Comte in the mid-nineteenth century. Comte provided the foundations for an idea that behaviors and thought processes experienced by individuals emanate from external forces and dynamics, an insight brought to fruition and given specificity by Emile Durkheim late in the nineteenth century. Durkheim's powerful voice and sustained commitment to seeking social forces that explained individual conduct served as a compelling directive that persuaded others to undertake sustained research for the purpose of formulating principles about social phenomena per se, rather than individualistic behavior.

Durkheim recognized that social life only exists in process but concluded that it was impossible for researchers to "mentally fix" such processes. It's important to remember that when Durkheim alluded to such mental fixation, he (and other scholars) neither had the technological nor theoretical means to treat the study of social processes as an end point. The ontological reality of process could not be "captured" via a corresponding epistemology. Therefore,

Carl J. Couch and the Iowa School: In His Own Words and In Reflection
Studies in Symbolic Interaction, Volume 49, 7–12

ISSN: 0163-2396/doi:10.1108/S0163-239620170000049003

according to Durkheim, sociological principles could only be formulated by researching what he called social facts – real things that required stasis, or an epistemology that would reveal the nature of such facts before they disappeared in their own processual movement. Later, students of Durkheim abbreviated his conception of social facts as static stimuli for examination, using static methodologies (such as quantitative analysis).

In my opinion, most contemporary researchers continue to accept this interpretation of Durkheim's epistemological standpoint to make the world static prior to examining it. They generate and analyze social facts such as questionnaire responses and bureaucratic records instead of observing and analyzing social processes. I do not contest that Durkheim advocated such studies; I do however believe that he advocated a static epistemology due to a lack of technological advancement rather than due to some ideological dictum that would resist change. If he had the means to figure out how to study, empirically, social processes in a systematic way, his *Rules for a Sociological Method* would, in my mind, have a considerable emphasis on merging epistemology with his stated ontology.

The conception of science offered by current sociologists who base their endeavors on their readings of Comte and Durkheim corresponded with Newton's paradigm. Such a world (and global view) conceptualized phenomena as existing in a steady and linear motion. According to Newton, the solar system, God's time piece, was running "on time uninterrupted." Such a straightforward and stable progression could yield data for selective natural explanations and predictions. However, commitment to this paradigm denied emergence, or the sudden and unpredictable outcomes that contradicted linear and symmetrical logic.

Despite recognition of the possibility and even significance of emergence, many social scientists attached themselves to a "game plan" based on the ostensible program of research that Durkheim developed early in his career. Such research procedures carried on by notable sociologists in the twentieth century provided the foundation for what has become known as positivist sociology in the United States. By the end of World War II, positivist sociology, with its emphasis on hypothesis testing and outcomes, dominated most of the sociology departments in the United States.

Quite independent of the developments that culminated in the establishment of positivist sociology, proponents of G. H. Mead, who had received a PhD in psychology, formulated a paradigm of thought for the study of human behavior that assigned priority to the social dimensions of human life. The proponents, including Herbert Blumer and Everett Hughes, became fascinated with Mead's nuanced view of paradigmatic views that changed over time. Rather than sticking to one paradigm of thought, Mead and his students recognized that various perspectives contributed to the dynamic dimensions of social life. Such perspectives, created by minded individuals, formed interlocking conceptions of social selves as they related to others. This multi-faceted and dynamic

view of life called for the systematic study of social processes. Mead's students became taken with the study of ongoing human behavior as adjustable, given particular situations, and as even altering such situations through adjustments.

Mead's call for a science of the ever-changing social, in contrast to Durkheim's call for stasis, rested on a foundation provided by Darwin instead of Newton. Mead's notion of the possibility of multiple paradigms not only acknowledged transformation brought on by humanly created emergence but also called for research of the consequences of such emergence (which could go through iterative transformations). As with the Darwinian vision of renewal and conversion, Mead's perspective called for researchers to simultaneously attend to continuity and change.

Herbert Blumer christened the ongoing processes of thought advanced by Mead as symbolic interaction theory. In the years following World War II, symbolic interaction theory competed with positivist sociology for dominance in a few of the PhD granting sociology departments of Midwestern Universities. During that time, Manford Kuhn joined the sociology department at the University of Iowa. Somewhat frustrated by what he considered as discourse overly-enamored with "what Mead really said" or "what Mead really meant," Kuhn set out to advocate a research program informed by symbolic interaction theory. At the University of Wisconsin, Kuhn had acquired familiarity with symbolic interaction theory while a graduate student advisee of Kimball Young, who had worked with G. H. Mead while a student at the University of Chicago.

Kuhn advocated using symbolic interaction theory to provide specific and systematic procedures for research endeavors. His best known data-gathering tool, "The Twenty Statements Test" (TST) relied on key tenets of Mead's view of the self. By using outcomes generated from TST research, Kuhn believed that he was advancing symbolic interactionism by providing greater validity, precision, and universality to the theory.

Among his other accomplishments, Kuhn devised the TST to observe self-conceptions and the Significant Others Test (SOT) to observe orientation to and affiliation with others. Kuhn concluded that if interactionists took sociological principles seriously so as to formulate universal statements and propositions, interactionists needed to generate data that could be shared with others and subjected to sustained analyses. While he regarded the research approach advocated by Blumer and Hughes as well as others affiliated with symbolic interaction theory at the University of Chicago as important and worthwhile, it also had to withstand more scientific, and perhaps more positivistic testing so as to differentiate symbolic interactionism from "high class journalism."

By the early 1950s, Kuhn had attracted a following among the graduate students in sociology at the University of Iowa that included Tom McPartland, Fred Waisanen, Bob Stewart, Hal Mulford, and me. We sought to formulate generic principles of human conduct by generating data via the TST and SOT and subjecting such data to sustained analysis. We thought we could transform

symbolic interactionism into a true science. Following Kuhn's lead, we had little tolerance for disembodied theorizing or theoretically uninformed research. We were not impressed by research based on naturalistic observations. We made up a small but robust collection that undertook efforts to redefine symbolic interactionism as a distinct scientific program of action that would extend Mead's vision of modern science as based distinctly on research science.

McPartland received his PhD in 1953; Waisanen received his in 1954; Stewart, Mulford, and I received ours in 1955. Kuhn continued to attract students after we left but the research of the late 1950s and early 1960s did not have the viability of the early 1950s. In the immediately following years, McPartland published several articles based on data generated by the TST. Kuhn and others also published articles that attempted to extend symbolic interaction theory by systematically analyzing data generated by questionnaires. The publications received noticeable attention to the point that Barney Glaser commented famously that we had created an "Iowa School" of symbolic interaction that rivaled the "Chicago School" associated with Blumer and later, Howard Becker and Anselm Strauss.

After giving a well-known Presidential speech to the Midwest Sociological Society that claimed to have wrestled symbolic interaction from the oral tradition to the scientific world of print, Kuhn died in 1963. Iowa's Department of Sociology hired Howard Erhlich to replace him. Erhlich had no interest or training in symbolic interaction theory. Despite putting the University of Iowa on the map, the department made no effort to maintain the symbolic interaction tradition. Instead, the department decided to become one of the major players in the general field of social psychology, hoping that it would emerge as a major intellectual center for any social psychological endeavor.

However, symbolic interaction did retain some footing in the department, largely through the efforts of Harold Mulford, who had a research appointment in the department of psychiatry. At Mulford's behest, I joined the faculty of the University of Iowa in 1965 with a joint appointment in psychiatry and sociology. Mulford and others did not expect me to maintain either the Kuhnian or symbolic interaction tradition, but I harbored the hope of re-activating it in the Department of Sociology. However, my efforts were handicapped by my joint appointment and the subsequent lack of having a full say in the Department of Sociology's direction.

My personal journey toward the breakthrough began serendipitously in 1969 when I acquired a full-time appointment in the Department of Sociology. As fully situated in the department and relatively autonomous, I could dedicate greater effort to promoting symbolic interaction theory and research. My goals at this time remained vague, but I would soon recognize some significant opportunities that emerged, creating significant changes in the academic world. I did not know for certain what these changes would bring about, but I felt, with a great deal of certainty, that the changes would alter perspectives, world views, and most important to me, the foundations of sociology.

Some of the ongoing changes occurring in the Department of Sociology in the late 1960s proved conducive to a new undertaking dedicated to the formulation of generic principles of social phenomena. Despite deemphasizing the role of The Iowa School within the department, Kuhn's legacy had remained viable throughout the 1960s. Further, although none of the faculty except Mulford and me advocated symbolic interaction theory, knowledge of the theory still infused the department. Most importantly, a new generation of graduate students had become acquainted with symbolic interaction theory and these students displayed much more interest in the theory than the extant faculty.

In a short period of time, I had gone from a person without a full-time position to a sought after academic advisor. During this time, the department faculty had evolved into a heterogeneous lot, including theoretical functionalists, statistical empiricists, positivistic attitude theorists, specialists in the family, criminologists, and various gadflies who neither ascribed to a perspective nor advanced much in the way of original research. My admiration varied for all of my colleagues, but I appreciated that diversity was at least tolerated if not welcomed.

On the national level, sociology had become a growth discipline. Many students entered sociology graduate programs in the 1960s with the belief that the discipline offered knowledge or could be the source of knowledge that would enhance the human condition. The increase in graduate student enrollment in sociology during the 1960s at the University of Iowa probably mirrored most other major universities. But by the end of the decade, I got the feeling that many who had looked to sociology with hope became disenchanted with the discipline.

Although I cannot articulate the linkages systematically and with full confidence, I am convinced that the breakthrough indirectly reflected and drew upon the surrounding social conditions. The intellectual curiosity that became established within the department, the external political climate of anti-Vietnam protests, the horrible assassinations and unrest that emerged in their wakes and various demonstrations for civil rights across the land seemed to coincide and peak in the 1960s and early 1970s. Sociology graduate students at Iowa put themselves in the forefront of the protests and demonstrations as did graduate students at many universities. However, while the students at Iowa had contributed a fair share of time to protests and demonstrated alongside other protestors on campus, their activities paled in comparison to other student protestors at other Universities. The graduate students at Iowa showed a great deal of sympathy for protestors nationwide, but they seemed to show more interest in sociology and especially, the budding symbolic interaction theory in our department.

As protests increased across the nation, interest in sociology as a discipline that could solve immense social problems seemed to decline in the early 1970s. What some have referred to variously as mainstream sociology had entered the doldrums by the 1980s. Robert Stewart suggests that a substantial number of people who normally would have entered graduate school to study sociology instead became involved in political activity (or countercultural pursuits) in the

late 1960s and early 1970s. Furthermore, he offered the observation that the development partially explains how the discipline of sociology lost its vitality.

As noted, the graduate students at the University of Iowa who dedicated themselves to formulating lawful principles of social life drew inspiration from the protests but did not commit themselves fully to politics. Their interest in sociological theory as relevant to political activity rather than their dedication to political activity per se, kept them involved in graduate school rather than devoted to protests and demonstrations. Those who formulated the breakthrough were as disenchanted with mainstream society as they were with mainstream sociology. However, the ones who remained committed to the breakthrough attempted to merge their political sympathies and sensibilities with the scientific study of social processes. We believed in changing the world. We also believed that we could change it incrementally, through some political action informed by scientific scrutiny.

The profound disenchantment at other universities that occupied many newsworthy stories did not seem to "infect" the graduate students at Iowa. Many young men and women continued to show up, willing to do whatever it took to study sociology, and especially symbolic interaction. It almost seemed that we had built a Mecca of sorts, making accommodations for them to study what interested them, even though we could not afford to support them financially. Until the late 1970s the number of graduate students far exceeded the financial support that the department could offer these students. Some faculty, such as Jay Weinstein, a demographer, had soft money to support those who did not receive assistantships, but intense competition among graduate students for the assistantships became the norm.

The competition among graduate students for scarce resources, combined with idealism linked to the discipline created a fertile seed bed for intense intellectual efforts. The creative juices of graduate students in Iowa's Department of Sociology existed across the board, not merely restricted to those who worked with me to forge the breakthrough. Those who formulated the breakthrough created relationships with others and each created a pattern of mutual stimulation, even though not all of them had direct involvement with each other.

By the early 1970s Frank Kohout, a proponent of symbolic interaction theory who had come to Iowa in the late 1960s, and I found ourselves surrounded by a collectivity of graduate students who had at least some interest in symbolic interaction theory and formulating generic sociological principles. Membership in the collectivity was somewhat fluid, but several hard core members formed a critical mass that impressed me, Frank, and a few other faculty members within the department. For the first time since my graduate school days, I thought that I had a group of bright and dedicated thinkers that could do what I, along with my cohort, did under the tutelage of Manford Kuhn. This time, however, we would change sociology without relying on a positivistic world view that informed Kuhn's enterprise. We would create a "New Iowa School."