



Carl and Mike, circa 1987

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

STUDIES IN SYMBOLIC INTERACTION

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STUDIES IN SYMBOLIC INTERACTION VOLUME 49

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

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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It is an honor to have this special volume that includes a complete version of Carl Couch's unpublished memoir, *The Romance of Discovery*, in *Studies in Symbolic Interaction*.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 intimidated, 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 memoir. "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 Nuncisus* (*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 (Finochiaro, 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 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, he 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 son 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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