
Bob Emerson’s approach to “troubles” has its origins in “the Sixties,” that protean and misleadingly labeled era which erupted from stirrings in the 1950s and continued to launch new lines of thought long into the 1970s. “Everyday Troubles” is not an unusual focus for a sociological study today but it would have seemed an odd book title earlier in sociology’s history. Like Howard S. Becker and his insufficiently appreciated mentor, Everett Hughes, Emerson’s honors simplicity in writing style. His texts are eminently accessible and modest to a fault. It takes a bit of explanation to convey just how true to the Sixties, how radical this book is.

A few notes on the author’s intellectual biography are essential. When Hughes left Chicago and resumed his teaching career at Brandeis between 1962 and 1967, he became central to Emerson’s development as a sociologist. Another powerful influence was Egon Bittner. Although he came to Brandeis after Emerson left, Bittner read Emerson’s dissertation and through their interactions became a window onto Harold Garfinkel’s work, which was then a cultish, for most readers inpenetrable, to many established sociologists infuriating, and for young sociologists a truly dangerous rebeginning of sociological theory that would alienate many from established and viable academic career paths. Bittner had trained at UCLA, as had Aaron Cicourel and John Kitsuse, all of whom were powerfully influenced by Garfinkel, who began teaching at UCLA in 1954. (In that larger project of intellectual history, the supposedly conservative Eisenhower years would be appreciated as the gestation period of what only became universally visible in the “anti-Establishment” 1960s.)

After Brandeis, Bob took a post-doc at the Law and Society Center, an exciting new center for qualitative sociologists at the University of California–Berkeley. Erving Goffman was then on the Berkeley faculty. Perhaps because of their common if generation-separated past as students of Hughes, and their shared ongoing research interests in the official treatment of juvenile delinquents and the mentally ill, and via the intermediation of Sheldon Messinger, among other intermediaries, Goffman became a mentor to Bob, reading and discussing parts of his dissertation in detail, then commenting on Bob’s work through correspondence that was sustained after Bob started his faculty career at UCLA and Goffman moved to the University of Pennsylvania.
Another window onto the same stream of emerging social thought was through Bob’s sister, Joan, who was a Ph.D. student at Berkeley in the 1960s. Joan studied alongside Harvey Sacks and Emanuel Schegloff and was sponsored in part of her work by Garfinkel. The protean nature of the times is indicated by the many ways that Garfinkel’s influence flowed independent of classrooms, Ph.D. committee composition, and institutional boundaries. It is fitting that it would require a creative ethnographic research project to trace this intellectual movement.

Through his forty-year career at UCLA, Emerson lived within an ecology of Sixties-era social thought. Continuous influences traceable to Hughes, Garfinkel, and their students were kept vivid by Emerson’s departmental colleagues, Schegloff, Garfinkel, later myself (conveying influences from Becker and Kitsuse), John Heritage, and Steve Clayman. Most important was his frequent co-author, Mel Pollner, a joyful and witty intellect who often seemed to have floated into sociology out of a Chagall painting.

Emerson’s work became a blend of micro-sociological traditions. The terms often used in academic sociology for referencing these traditions, “Chicago school” fieldwork, “symbolic interactionism,” and “ethnomethodology,” will seem odd to non-specialists, and they should. The central ideas are rooted in American pragmatism and European phenomenology, two philosophical traditions whose similar honoring of both an active human subject and of the scientific method as an arbiter of competing ideas was recognized by and stimulating to early 20th-century contributors. That these traditions should be blended in academic sociology would, to an outsider, seem inevitable. But due to the machinations of academic power, young researchers were pressed to affiliate with exclusive research camps.

If the “Sixties” was a period of striking intellectual independence, it was also one in which the self-serving constraints of disciplinary boundaries were being institutionalized in new forms. Academic sociologists then and now emphasize, even rigidify, their differences, for strong if not good reasons. As time goes on and literatures mount, there is an ever greater concern to limit what one needs to read. Departmental faculties differentiate subtypes of sociology when they make hiring claims for open faculty positions; specialization limits competing candidates and intercollegial conflict. Among late 20th-century academics, creating social networks that distanced themselves from each other became...
widely useful for stimulating in-group loyalties and excluding unsympathetic peer reviewers of journal articles, book manuscripts, and research grant applications.

Emerson refused to choose, through some combination of factors—wisdom, a deeply independent mind (his father, a Yale law professor, was perhaps the leading First Amendment legal scholar in the U.S.; Bob grew up listening for telltale FBI wiretapping clicks on the home phone), and an aversion to conflict. His independence was rare. (For a while, David Sudnow also kept feet in both camps, only to abandon academia entirely at mid-career. Murray Davis’s aborted career at UC–San Diego also showed the difficulty of straddling “interactionist” and “ethnomethodological” camps.) One unfortunate upshot of the split between pragmatist and phenomenologically rooted sociology is the lack of a common rhetoric for addressing fundamental issues. As a product of Emerson’s stubborn open-mindedness, *Everyday Troubles* invites an attempt to explicate a shared methodology.

Readers of this book may wonder how to read statements which assert that at a given stage of a troubled relationship, the aggrieved party “may” take a particular course of action. Why mention a particular course of behavior if others unmentioned might have been taken? Did the actor face any constraints on alternatives? If so, why not describe them? If not, do the instances reported constitute an arbitrary collection from the relevant set? And if all of the analyzed actions might or might not have occurred, in what sense does the social life of troubles have stages? Whence the patterning of the social life of troubles?

Emerson’s theorizing builds on a particular Sixties-era departure from the conventional genres of sociological explanation. Academic sociology was then entering a specific moment in which the dialectics of “the discipline” took on unprecedented intensity. The dynamic tension was not between quantitative and qualitative work; that conflict had already worked itself to a dead end in the 1950s. By 1960 there was an uneasy peace in which survey and demographic methods increasingly characterized articles in leading journals while qualitative sociologists, even as they typically manifested a besieged mentality, created their own journals, thrived on the unprecedented expansion of the job market in undergraduate teaching, and produced sociology’s most successful books as well as many presidents of academic sociology’s professional organizations.

The central intensifying dialectic in the 1960s was between advances in methodology, especially the ability to describe change over time, and the content of sociological explanations.
On the one hand, sociologists who had matured in the post-war period now knew seemingly everything. Celebrated works answered grand questions, like why some societies would become dictatorships and others democracies and why some large nations would modernize their economies and others, like China and India, had religions and value systems that condemned them to languish in “third world” status. Eastern European nations living under the tank-backed oversight of the Soviet Union were said to have established a viable middle ground between state socialism and Western capitalist democracy. Great progress was proclaimed in answering middle-range questions, like the patterning of sons’ inheritance of fathers’ social status and why crime rates were high in some cities and low in others. And at the micro level, through “exchange” theory, “expectations states” theory, and “social geometry,” sociologists made successful academic careers by arguing conflicting claims that they could predict when norms will be enforced in the everyday situations of social life.

When I began graduate school in sociology in the fall 1969, a professor in the Northwestern department, Bernard Beck, had just returned from an American Sociological Association annual meeting. He was still reeling (well, chuckling) in amazement at the panoply of research findings he had witnessed. The dominant style was correlational analysis that charted the state of given variables when other variables were in specified states. The overall thrust was an implicit claim for a comprehensive, final knowledge that one day would be visible on the world’s biggest blackboard, which would display lines in all logically possible directions connecting all of sociology’s mainstay demographic and social ecological variables, each line annotated with a quantification of the extent of variance explained. The ambitions of academic sociology seemed at once limitless and precise, both cosmological and astronomical. To dissidents like Garfinkel, who enjoyed provoking “the worldwide social science movement,” the quest was astrological.

The background of sociological explanation in the Sixties was an intellectual confidence that, not coincidentally, paralleled the confidence of national leadership, which in the 1960s was turning enthusiastically to academic sociology for policy guidance. Well into the 1930s, academic sociologists were backward-looking, still trying to catch up on the great social transformations that had occurred during nineteenth-century industrialization, which had been established before the academic field was created. After World War II, however modest
they may have been personally, academic sociologists had arrived at a historic moment of
stunning collective hubris: they were future-oriented, geared to predict.

At the same time, major leaps were being made in descriptive power, in particular in the
ability to describe social process or change over time. This turn, picked up by some in
as a call to focus on sequence, by others as a commitment to create new forms of diachronic data, ran across
both quantitative and qualitative research. The audio taping of conversations opened
up an unprecedented analysis of turn-taking sequences: with a precision and in patterns that had
never been accessible since humans began to speak to each other, every speech act could be
shown to be tailored in appreciation of its immediate backward- and forward-looking interaction
significance. An outstanding 1965 article by Norman Ryder set cohort analysis at the center of
the study of social change. Historical sociology’s research programs were expanded by creating
data sets of event histories. The importance of path dependencies in organizational development
was recognized in a celebrated publication by Arthur Stinchcombe, and the utility of the concept
began a long process of expansion, which still continues. Quasi-experimental design, stimulated
by national policy initiatives that demanded evaluation, was pushed to unprecedented
sophistication, in part by introducing additional phases of data collection on experimental and
control groups, beyond snapshot “before” and “after” measures. Large data-gathering operations
were organized to create multiple panel cohort studies, which could overcome the limitations of
“sampling on the dependent variable” (e.g., studying father’s influence on son’s status by
looking through the younger generation back to the older) that had compromised research on
social mobility.

Given methodological specialization, few of the pioneers in these descriptive advances
talked with each other or in their writings seemed to care about each other’s work. Emerson,
while only explicitly following leads only in qualitative sociology, was moving in parallel with
many of sociology’s fields when he began to describe social processes that had been ignored by
the sociological theories that had reigned up through the mid-20th century. *Everyday
Troubles* is a direct continuation of his dissertation work, which was on how juvenile court
judges worked. The vast majority of sociological research on juvenile justice had been
correlational, asking what kinds of persons, as defined by the researcher, *did* commit what
kinds of offenses and got what kinds of sanctions. To explain norm enforcement, the prevailing
theORIZING grew out of positivistic, highly abstracted formulations in small group and social
psychological work, \textit{viz}, namely, in which when the participants \textit{are}, or the group or the interaction situation is in this or that state, actors will be more or less inclined to define others as deviant.

Influenced directly by “labeling” theory, which had been advanced by both interactionists (Becker, Erikson, Goffman) and ethnomethodologists (Kitsuse, Cicourel, Pollner, Garfinkel), Emerson abjured making his own independent definitions of the accused, or even of the situation, and created data that followed change over time. He asked how judges developed their understanding of the kind of boys and the status of the cases they were judging. In his Boston-area case study he found, in part, that judges’ definitions of juvenile defendants were developed in response to critical interactions with probation officers, who in turn defined the accused with an eye to what could be done practically with them by remedial or punitive others. Routinely, the tail of the case, or what could be anticipated to emerge when bureaucracies got hold of it, dogged the judicial heads.

Next, collaborating in the 1970s with Sheldon Messinger to examine conflict outside of official forums, Emerson described the “natural history” in which people came to perceive, test and reformulate the interpersonal troubles they experienced. The definitions of troubles made by “members” (a term that ethnomethodologists often used to emphasize the distinction between the analyst’s and subjects’ views) were not their psychological projections nor were they ecologically determined, as the vast majority of then-current research approaches assumed; they were “negotiated” in relation to how others in the immediate situation responded.

Emerson’s studies were part of a destabilizing thrust in the dialectical relationship between sociology’s substantive theories and its methods. Confidence in the potential of substantive sociological explanation in effect funded the development of methodological advances, which then unearthed limits in prevailing substantive explanations, which, however, were by then so well established in tenured positions as not easily to give way. There was funding from government and from private foundations to study deviance and conflict, based on the confident assumption in mid-20th-century social thought that these were things that the researchers could more or less simply code as social facts and then explain via biographical and social environmental background conditions. The insidious nature of Sixties research was earnestly to improve sociological description, with the result of attacking the premises that had led to confidence in sociology’s substantive potential.
Emerson’s work on “troubles” was part of the critique of the prevailing positivistic approach to explaining crime and deviance. The typical research approach was predictive: social problems were presumed to be caused by prior events. The analyst portrayed crime, deviance, or other forms of trouble as existing at Time 2 and sought causal forces operative at Time 1. That was a problematic approach when data were correlational. But even when researchers developed diachronic data, fundamental problems remained, because they had an overly simple understanding of the process and even the direction of causality. New research began uncovering more and more contingencies between Time 1 conditions and Time 2 behaviors. Indeed, the new process-attentive research often showed that part of Time 2 behavior was the invention, creation, or manipulation of effective social descriptions of what had happened at Time 1. Again and again, those working in the spirit of Goffman, Becker, Cicourel, and Garfinkel found that part of the process by which members, at Time 2, perceived others as homosexual, as delinquent, or as mentally ill at Time 1, was through developing their appreciation at Time 2 that the deviants had manifested their moral status long before Time 1. This retrospective reconstruction of deviance was visible in data showing how power-wielding officials as well as lay actors in everyday life reconstructed their view of people they treated as problem-bearing.

There was a strong social critical thrust to this process-focused work. Judges and people handling their own troubles in everyday life contexts seemed to invent, or at least invoke, definitions of deviance based on what remedies were available. Prevailing theories and research designs, which been supported with relatively little or no data describing empirical processes between Time 1 conditions and Time 2 behavior, were also cast as fundamentally problematic. The thrust of sociological work on crime and deviance was “if/then,” forward-looking: Merton’s Robert Merton theorized delinquency as the result of a contradiction between a materialistic culture and a social structure of limited opportunity, Peter Blau combined census and police data sets to explain that metropolitan inequalities promoted high crime rates, Donald Black predicted norm enforcement in everyday police work as the result of pre-existing situational factors. The radical implication of new critique was that a vast amount of such sociological work was aimed at explaining fantasy phenomena: researchers-mainstream sociologists had assumed that individual deviants and interpersonal problems existed to be explained even while they unwittingly made them up; conventional research was subtly circular because the problems to be explained only could be detected only when the factors explaining
them were in evidence; sociologists were unwittingly serving those in power by giving professional grounding to the arbitrary versions of social life that justified their officials’ exercise of power.

Any good historian of social thought could find that these Sixties studies were developing long-standing threads in Chicago school interactionism, Weberian *verstehende* sociology, and the “emic” perspective in anthropology. For philosophical readers, there was no news in the ethnomethodological operation of “bracketing” members’ claims that their current action was founded upon conditions that previously existed. But within sociology, ethnomethodology made news by attacking sociologists who ignored members’ perspectives and imposed their own via what Cicourel damned as “measurement by fiat” and by research procedures that, in Garfinkel’s scathing language, treated members as “dopes,” robotic transmitters of pre-existing situational norms, cultural themes, and societal values.

Members were elevated in status and regarded as superior authorities to whom the academics who studied them must in effect defer by studying their methods of interpretation, i.e., that is, ethno-methods. But beyond broadcasting the call that the researcher ground his or her concepts in members’ constructions of reality, there was a new insistence that the analyst appreciate that members were folk sociologists, invoking their own elaborate, sequence-sensitive theories as they perceived others’ conduct and in response formulated their own. Not only must the researcher study the sequence of social action to see where and how definitions of the situation and of others arose, the researcher should understand that members have their own understandings of the sequencing of social action, such that every action that a subject under study took was shaped with regard to how that subject understood how others would understand the action’s specific sequential meaning.

Officials would treat “the first case we have seen of this kind” differently from “just another routinely encountered case.” The causal significance of the sequential meaning of the current case within the historical flow of cases was understood by the others as well, such that characterizing the case as “unprecedented” or “typical” was at the very essence of the processes determining how an accused, an inmate, or a patient would be treated. But recognizing members’ sociological theorizing did not make the sociologist simply a scribe. The researcher would not find members using “typical,” “unprecedented,” or any other particular phrases to base his/her understanding that members were negotiating over the status of the case within their
understanding of the sequences manifested by cases in general. Members would suggest that the
case is unprecedented or typical perhaps only with inflections, by the colorful character of their
expressions, via all sorts of mannerisms and tangential behavior that indirectly, subtly, but to
others effectively suggested they were dealing with a routine or an exceptional case. Part of the
impact of Sixties’ sociology was to expand the ethnographer’s descriptive work explosively.

The recentering of the research focus on members’ in situ experience and action meant a
new moral and epistemological relationship between researcher and subject. Doubt about
honoring the subject’s perspective was well-established by the 1960s. Freud’s writings on
symptomatic, neurotic, self-justifying, phobic attributions of one’s problems to others were still
resonant. In quantitative research, the methodological concern to limit “reactivity” sounded a
similar alert: what someone said about their social life could not be accepted as a transparent
truth. Data were artificial if produced in response to cues the researcher unwittingly was
emitting. Subjects’ neurotically distorted accounts of the past, and researcher-“contaminated”
versions of subjects’ biographies, had been something that the scientist should cure, avoid, or
discount when analyzing data.

Now that members were understood as folk sociologists, there was a new research agenda
to launch. The new work was to detect, document, and articulate how the folk sociological work
of the members studied was a natural, ubiquitous part of their behavior, part of the very being of
the stuff all sociologists study. Folk sociology was not a problem to be cleared away from the
vision of correct, professional sociological understanding of social life. Nor, as sociologists
reading anthropology might think, was it only a subject for ethnological curiosity. Folk sociology
was what all social life at its most fundamental level was made of.

As Emerson shows throughout his text, members do not simply invent the grounds of
their responses to the troubles others make for them. Accordingly, he does not seek to
delegitimize their accounts of the grounds of their conduct. He shows the nuanced sociological
understandings of any course of action a member takes.

Members’ trouble-relevant behavior always consists of responses made within an
elaborate semiotics of possible responses. When acting positively, people always know of the
alternatives they forego. Here the theme of negation in existential thought came into academic
sociology, albeit without being announced as such. To choose to do is also to choose not to do.
Indeed, any course of observed action is, as a social reality, even more a negative choice; any
observable action exists even more profoundly in what is not observable, because so many alternatives are wittingly, if implicitly, foregone.

Further, the semiotic context of a member’s action is historically differentiated. At each and every moment that a party to a troubled relationship is forming a response, he or she understands which of the possible responses he or she has already tried and which of the possible responses that might have been tried were purposefully deferred. Moreover, in capturing any party’s response in descriptions, the analyst must appreciate how that party is acting upon his or her understanding of how the other party’s behavior has been a product of the other’s folk sociology, which is also sensitive to sequence and aware of possible paths that were not taken.

In a further effort at clarification, let me be a particularly troublesome ego. If I respond today to my neighbor’s parking his car in front of my house by taking out my shotgun and waving him away, a researcher would be correct in understanding that I am very likely imputing to my neighbor intentional defiance and a view of me he does not have. But an accurate description of my action would also include that I am knowingly foregoing a host of less grave responses to the irritation (like complaining politely) as well as more grave responses (I am only waving the gun, not firing it), on my understanding that my neighbor’s action today is itself meaningful within a series of his prior irritating actions, and my responses and non-responses at prior times. My current response, that of introducing the new instrumentality of a shotgun into the troubled relationship I have with my neighbor, is based on my understanding of how my neighbor responded to my previous efforts to remedy our relationship. And I also understand that, while the current conflict is with a particular neighbor, others are watching or are likely to hear about the event, so that I am shaping their understanding of what I am likely to do in conflicts with them. And that too, I know, has a history.

To appreciate what is being accomplished in Emerson’s text, the reader should keep in mind that, in inspecting a given passage of his data, all the researcher-observer sees is a waving of the shotgun. But that moment is the superficial if potentially mortal upshot of an interaction which is three-dimensional both in an emotional and historical sense. Emerson’s path out of the Sixties ferment in sociological thinking was to opt not to base an explanation of trouble-relevant behavior on the visible tips of the iceberg and hope that the massive underlying realities can be ignored. He chose to jump into the risky business of documenting the micro-historical underpinnings of what becomes readily observable. The reader will immediately appreciate the
rewards, much as when reading Erving Goffman’s work. The author reveals biographical and
environmental meanings that are critical to the motivation and shaping of the situated action that
is observed—here my waving of the shotgun—which the reader, even if present on the scene
without any impediment to perception, could never appreciate without the researcher’s aid.

The three-dimensional, situational tip/sequential iceberg understanding that Emerson
opens up would be enough to make these studies invaluable for anyone doing sociological
research on troubled relationships as well as for anyone working in remedial, adjudicatory, or
therapeutic occupations. But there is also an ethnomethodologically inspired contribution.
Emerson’s text specifies the folk sociology that members invoke as they perceive and deal with
interpersonal troubles.

What sort of explanation does that provide? Clearly it is not a “sufficient condition”
explanation of the type sought by predictive theorizing. When writing “may” propositions,
Emerson does not mean to say that, once other conditions are specified in additional research, the
“may” conditions will be converted into causal propositions of either the necessary or the
sufficient kind. He is not using a qualitative sociologist’s rhetoric for the quantitative
sociologist’s claim of explaining some significant part of the variation in the explanandum. Nor
is he following an explanatory formulation once tried by David Matza: framing the causal impact
of an explanans in the nature of “soft determinism.” Nor is he following the contemporary
fashion in academic ethnography of offering a “mechanism” in a causal relationship. As used
today by those who would justify ethnography by claiming a contribution to deterministic
explanations, a “mechanism” is a “how,” a qualitative connection between causal variables
measured quantitatively. By specifying mechanisms, qualitative sociologists make the
explanatory value of quantitative research easier to digest.

Emerson’s book is a decades’ long product, a multicourse meal showing
infinite patience with the details of social interaction. It is the equivalent in sociology of slow
cooking. He does not take up any of the familiar recipes for framing the explanatory contribution
of ethnography. But then, put in the faddish way to state the question now, which seemingly
borrows a term from the social life of fast food restaurants, we may reasonably ask, what is the
“takeaway”?

One useful alternative to sufficient condition or predictive theorizing is common in
micro-interaction-focused social research. Emerson has used it elsewhere in his writings, but it
will not work with the current materials. Instead of specifying conditions at \( T_1 \) that will produce specified behavior at \( T_2 \), the analyst examines behavior realized at \( T_2 \) and, based on sequential descriptions of prior actions, develops an analysis which predicts that certain prior behavior or events will have occurred at \( T_1 \). Sometimes called retrodiction, this type of explanation is familiar in “natural history” and Darwinian theorizing. Stanley Lieberson has argued that sociologists have too narrow an understanding of “science.” With a more accurate appreciation of what natural scientists actually do, sociologists would better exploit this nondeterministic alternative. In fact retrodiction is often used in micro-sociology, although typically without the author writing self-consciously about the kinds of theoretical claims that are being made.

Emerson, without using the phrase or identifying the kind of explanation he is offering, has developed retrodictive theorizing in his people-processing writing. In his paper “On Last Resorts,” he argued that, across an organizationally independent variety of institutions, including juvenile courts, psychiatric emergency intervention agencies, and school discipline administrations, officials dealing with people in trouble all organize their responses within a folk-sociologically-appreciated, graded series of alternative responses, which series always has a “last resort.” Last resorts are meaningful contingencies of official behavior even, indeed most often, when they are not used. (Again the negation, the “what does not happen” looms as the most important social fact.) Emerson does not specify the contingencies that predict the turn to last resorts. Instead he shows how a collective folk sociology of last resorts is used to organize what the officials do. I.e., the official in effect communicates to the others involved (other officials and professionals working on the case, the troublesome person, his or her family and comrades) that “you know that I have a graded series of possible responses and could as a last resort order imprisonment (or physical restraint, or expulsion)” in order to get compliance that will resolve the case, at least momentarily. In order to get their work done, authorities arrange their alternative actions sequentially, such that an analyst can retrodict that if a last resort has been employed, other “lesser” responses will have been tried or attended to and passed over.

But in this volume propositions of a retrodictive sort are never asserted. For systematic reasons, Emerson cannot provide a retrodictive explanation of everyday troubles. Officials intervening in troubled relationships and responding to troubled people have constraints on them
that the subjects of the current chapters—us in our everyday lives—do not. In particular, organizational officials are aware that they are making records which other officials may use to review and assess their actions. Officials’ future-oriented action brings a discipline into their people processing which is absent from the everyday living of tensions with neighbors, residential mates, lovers, work colleagues—situations in which individuals, correctly or not, presume that they are living more or less private moments that are insulated from official review.

(After reading a draft of this forward, Emerson sent me a copy of a 1981 letter in which Goffman, after reading the published version of the “Last Resort” paper, made a similar point.)

Now, if for “everyday troubles,” predictive and retrodictive theorizing are both unworkable, what sort of explanation can enable the book’s descriptions of situated trouble-related actions to hang together? This study shows how people themselves systematize their behavior in troubles. They use the same actions to systematize how they respond to incessant irritations from people they know intimately and to sporadic irritations from those they know hardly at all. We may be surprised at who gets mad at us. We may be taken aback when they express their anger at us. We may whisper prayers that help us refrain from voicing protests and moving down the path toward calling in a third party to tranquilize a troubled situation. Our behavior in troubled relationships seems to us idiosyncratic, emotionally volatile, or unpredictable, but Emerson shows that it is not random. What the following chapters make clear is that, while predicting or even retrodicting particular responses is a problematic explanatory strategy, sociological theory can grasp the universal systematization of trouble-related behavior by those caught up in everyday troubles. We all go through the same operations in living our everyday troubles. At its most expansive, Emerson can justify the theoretical claim to have begun the outlining of the universal folk sociology through which interpersonal troubles arise and are transformed.

Emerson writes directly about folk sociology only in the conclusion. What he shows throughout are trouble-perceiving and responsive behaviors experienced in intimate emotional depth, differentiated by members with a fine interaction sensitivity, and encased in a vivid awareness of historical detail. (After reading these materials, one could conclude that no one is a better sociological observer than when inspecting the interaction contingencies of his or her own troubles.) But Emerson shows that the sociology of trouble-related social life is not a matter of collecting colorful facts. It is the sociologist’s discovery of a universal institution of folk
As Simmel would have appreciated, in everyday troubles even when the parties never put their conflicts to rest they share with each other the same folk sociology for registering irritations; they appreciate the proper contingencies for transforming them into complaints; they anticipate the same results when calling in third parties; etc.; and so forth. That means that each party to a conflict, at each moment of the conflict, is locating the moment within what he or she understands to be the general or typical natural history of conflicts of the sort at hand. The two parties in a troubled relationship may not see each other as normal or typical actors, but what they understand to be happening is a troubled relationship progressing as a variation on what they (rightly or wrongly) understand the natural history of troubled relationships to be.

How do they theorize? Not abstractly, not through explicit generalizing, but by forming their expressions in the current conflict as a version of what typically happens. One party sees that the other is responding now in a way that should be—and among right-thinking people is—reserved for a “late” stage of conflict. At the same time, the other party knows that she is gracious, that she is presenting herself as a forbearing soul, by foregoing a response that other right-thinking people would deem appropriate at this stage.

In a sense, troubles progress through interactions among theorists, each viewing the other as a deficient theorist, one who does not understand what is appropriate at this stage. Each takes for granted that what is appropriate and inappropriate as action in a troubled relationship depends on the stage it is in. Everyone knows the difference between living with irritations in the hope they will go away versus acting on them, between resolving irritations unilaterally or through trying to secure the cooperation of the troublesome party, between various ways of framing a complaint in the hopes the other will resolve the matter, between keeping it a matter between the two or involving others informally, between limiting the conflict to particular behavior and turning vindictive, and between keeping the dispute informal and bringing it to the attentions of the authorities. Roughly stated, this universal, sequence-sensitive folk sociology defines the stages that are represented in the chapters of this book.

This is not to say that all or most or even any conflict will go through these six stages. It is not to say that if a troubled relationship gets to the stage of authoritative involvement, it will have gone through the stages previously presented in prior chapters of the text. It is to say that at each moment of behavior in a troubled relationship, each party is acting on an understanding of
where he or she is and how he or she is advancing the progression that Emerson has captured and has made the backbone of his text.

The systematization of everyday troubles is done by the participants, just because there is no official to provide a systematic progression of stages of response, as occurs when a trouble enters a judicial, occupational, military, religious, or educational bureaucracy. In the bureaucratized trouble context, individuals in a troubled relationship will often need expert advice just because the stage progression is not of their devising. In everyday troubles, people must be their own advocate, judge, mediation counselor, psychotherapist, witness, and enforcer. In some of the material that follows, it will seem that a party to conflict, compelled to play all these roles simultaneously, is led to an Alice in Wonderland “off with their heads” justice that shows impatience with exploiting less consequential solutions. Nevertheless, in no case can a party conduct coherently, in no instance can a party in a troubled relationship form conduct that makes sense to as trouble-responsive behavior, without locating where the trouble is in a taken-for-granted, generalized understanding of a progressive treatment of troubles that is “natural.”

This volume is a testament to the abiding potential of sociological discovery. As such, it is a model for this book series, which is dedicated to showing how sustained ethnographic fieldwork of the first order can serve sociology’s collective project. Across all methodologies, quantitative and qualitative, sociology since the Sixties has been making consistent progress in improving the collective capacity to describe social life by more fully and precisely tracking its constantly flowing character. The discipline has been less successful in producing predictive substantive explanations that survive the test of time. Based on a career-long examination of the social life of troubled relationships, this volume not only advances knowledge of a universally compelling area of social life, it also makes an extraordinarily deliberated contribution to the still pressing challenge of developing a genre of sociological explanation that can live in an untroubled relationship with the discipline’s ever-increasing descriptive powers.

Jack Katz