DIFFERENT STROKES

Qualitative Research in the *Administrative Science Quarterly* from 1956 to 1996

John Van Maanen

The title of an essay, article, chapter, or book is part of the text. It is the first part of the text we encounter and therefore has some power to attract and perhaps condition the reader's attention. For the writer, choosing a title is something of a creative act, helping to bring into focus the piece of writing that follows. I jotted down thirteen possible titles for this essay—including the too frivolous but autobiographical "How I Spent My Summer Vacation" and the accurate but previously used "Varieties of Qualitative Research"—before settling (uneasily) on the rather whimsical lead phrase, which comes from the vernacular bon mot "different strokes for different folks." I settled on this title for several reasons.

First, this is an introduction to a collection of organizational studies produced by authors using a range of qualitative research methods. The plural is important, for it signals my concern with the grab bag of techniques and strategies used in qualitative research, including, for example, historical analysis based on archival materials, conversational analysis of closely edited snatches of naturally occurring talk, intensive interviews designed to ferret out native points of view and account for them, and living with and living like members of an organization so that ethnographic impressions of organizational life can be constructed. Each has distinctive information gathering and

harvesting conventions to follow (and foil), and qualitative work, like quantitative work, comes in many different forms.

Second, my title indicates that the products of our research endeavors are ink stained. Whatever else we may do in the way of learning of organizational life, we must also write about it and convey our understandings to others through a publication process initiated by endless keystrokes. Narrative conventions associated with qualitative work are also varied, and writing styles followed consciously or unconsciously by authors are not perfectly correlated with methods. Thus, participant observation studies may take a realistic or confessional turn, just as historical studies might be given an objectivist or subjectivist wash.

Third, the intellectual field within which the "different strokes" that are my concern appear is an ambitious yet ambiguous one. Organizational studies rely on a strange brew of reference disciplines, including sociology, history, economics, psychology, anthropology, political science, and even literary, media, and communication studies.[1] Each discipline has humanistic as well as scientific wings, and qualitative work is undertaken, written, and read on both sides of the house. The scholarly circles within which qualitative work circulates are thus many. Some are relatively insulated, well armed with favored techniques and consensually approved reporting styles; others are emerging and eclectic, mixing parasitic and novel elements while shaping their aims and tricks of the trade by topical interests and pragmatic, on-the-fly concepts and concerns.

Such variety is neither a scandal nor (necessarily) a strength. It does suggest that equating qualitative research with ethnographies or case studies or historical descriptions is simpleminded. Qualitative methods are many, they are everywhere, and they do not easily boil down to formula. Moreover, qualitative work shapes and is shaped by numerous philosophical and theoretical traditions. Constructivist views of the world of the sort spun out of social and cultural anthropology or resting on the symbolic interactionist perspective in sociology inform a good deal of qualitative work but certainly not all. Nor is qualitative work restricted to the so-called individual or group levels of analysis. Powerful qualitative studies examine organizations writ large, as well as industries, societies, and the global character of contemporary political economies, capitalist and otherwise.

This leaves us with the question of just what distinguishes a particular work as qualitative. Elsewhere I have noted that we are perhaps best off thinking of qualitative research in terms of some of the organizing principles that mark the concerns and activities of those who do the work (Van Maanen, 1983). This is akin to Kuhn's (1970) celebrated definition of science in terms of what scientists do. Some principles that guide much qualitative work include a focus on meaning, the use of analytic induction, maintaining a close proximity to data, an emphasis on ordinary behavior, and attempts to link agency to

structure through accounts based on the study of events (routine or otherwise) over time. But, as with most recipes for social practices, exceptions are the rule.

What makes qualitative research particularly difficult to pin down is its flexibility and emergent character. Qualitative research is most often designed as it is being done. It is anything but standardized or, more tellingly, impersonal.[2] As Becker (1993) pointed out, qualitative work allows for—indeed, insists on—highly contextualized individual judgments. It is a style of research that makes room for the unanticipated, thus focusing more on specific cases and exceptions than on abstractions and generalizations. In the end, qualitative researchers come to know a good deal about the specific social worlds they study and find it difficult if not impossible to reduce these worlds to a few representative and measurable dimensions.

This is another way of saying that qualitative work generally sidesteps the hypothetical-deductive research model in favor of an inductive, interpretive approach most often marked by a reliance on multiple sources of information. Data gathering or technique-dependent definitions of such work are faulty because they cannot absorb the diversity of methods subject to the qualitative label.[3] The aim of most qualitative studies is to produce a more or less coherent representation, carried by word and story, of an authorially claimed reality and of certain truths or meanings it may contain for those within its reach.

It is sometimes easier—although dangerous—to evoke qualitative work by looking at what it is not: A text stressing variables, operational definitions, and tests of propositions derived from a muscular theory that maps the world in terms of cause-and-effect forces is not a qualitative study; variable analysis marked primarily by examining arithmetic covariation is not a qualitative study. In quantitative research, the author attempts to interpose an intentionally standardized data collection process that operates between the social settings and situations studied and the analysis of those settings and situations. What is analyzed is not the setting or situation as it is experienced or represented more or less in the raw, but the data. Quantitative data can take many forms, such as answers to items on questionnaires or formalized observations of behavior but, in the end, such data must be amenable to frequency counts and statistical analysis. The products of such work are numbers, figures, and tables that summarize selected properties of the data.

Qualitative work produces narratives—nonfiction division—that link events to events in storied or dramatic form with beginnings, middles, and ends. Story elements are explicitly connected, thus *emplotting* a research report with an apparent causal structure that itself is made theoretically plausible through argument and analogy. There are many narrative forms available to qualitative researchers, but the idea is to create historically situated tales that include both highly focused portraits of what identifiable people in particular places at certain times are doing and a reasoned interpre-

tation for why such conduct is common or not. The sociological specificity, historical particularity, and narrative form are distinctive features of qualitative work. The end result is its effect on the reader who, ideally, is both enlightened by the narrative and persuaded by the explanation the writer offered.[4]

We must not make too much of these distinctions, however, for they are heavy with evaluative freight and lead to rigid conceptual categories devoid of nuance and shared features. Quantitative research is not the evil twin of qualitative research. A problem in much of the methodological discourse and elsewhere is that dichotomies substitute for differences. Dichotomies produce sharp contrasts and thus sever, leading to a rather pat and pointless definition of qualitative research as everything that quantitative research is not. Dichotomies reflect a rigid, perhaps obsessive concern for opposition as expressed by the use of the structuralist's bold slash to mark loaded contrasts: A/Z, Good/Bad, Clean/Dirty, Right/Wrong, Truth/Fiction, Qualitative/Quantitative. Differences, however, as Jacques Derrida (1976, 1978) would have it, provide a comparison and thus point to relative, not absolute standards.

Such relative standards are evident in the matters of exploration and verification in research. Qualitative work is often characterized as exploratory, aiming at discovery, description, and theory building. Quantitative work, when set up as part of a dichotomy, is about justifying or verifying by test the empirical basis and generality of theory claims. The slash between the two, however, is anything but fat and wide, for exploratory work is never dreamwork or a leap into the dark; lots of reasoning enters, information gathering is always selective, and any exploration is governed in large part by theory that determines (at least partly) what counts as fact, evidence, story, and so forth. Nor is justification and the testing of ideas devoid of exploratory elements and novel analytic twists. Rather than a dichotomy, we have a difference whose particular shape varies from study to study.

Rigid dichotomies also obscure the inevitable interdependence of quantitative and qualitative work. Quantitative research is typically most interested in making general statements that take the form of defensible propositions about analytic classes or abstracted properties of social life. Qualitative work is usually most interested in coming to terms with specific instances of social phenomena and how broad principles or theoretical suppositions work out in particular cases. In this sense, the two imply (and deserve) one another, since any given case must display some general principle and the announcement of any general principle must assume that a specific case can be found for its illustration. While it appears rhetorically impossible to talk about research methods in organization studies without contrasting qualitative and quantitative work, neither approach can stand alone.

Finally, both qualitative and quantitative research confronts the dismal but stubborn fact that any given study—qualitative and quantitative—stands on

shaky epistemological grounds. All methods are flawed in some way or another. Not everything can be examined at once, and limitations of scope and depth abound. Quantitative researchers must rely on their own experientially based understandings of social life to make sense of their findings, just as qualitative researchers must rely on all sorts of categorical and distributional data to locate their work. Like it or not, both must make use of data and methods that are questionable from their own epistemological orientation just to get on with the business of social research. It is, however, a business that shifts with the times.

We live now in an age of scholarly declassification. Blurred distinctions, fuzzy sets, and intellectual poaching all mark academic life, and theorists of many sorts avoid discrete, sharply delineated concepts set off from one another by empty spaces and turn instead toward continuous, overlapping concepts that slip and slide Escher-like into one another. The rise of Giddens's (1979, 1984) structuration theory provides a marvelous example of just such a move that has been taken up by a number of organizational researchers. But declassification extends well beyond the small worlds of organization research as scholarly disciplines intermingle, borders open, categories collapse, theories blend, authority disperses, voices multiply, and hodgepodge becomes the order of the day. Geertz (1983: 20), as scholar turned stand-up comic, surveys the scene and finds

> philosophical inquiries looking like literary criticism (Cavell on Beckett or Thoreau, Sartre on Flaubert), scientific discussion looking like belles-lettres morceaux (Lewis Thomas, Loren Eisely), baroque fantasies presented as deadpan empirical observations (Borges, Barthelme) . . . parables posing as ethnographies (Castaneda), theoretical arguments cast as historiographical inquiries (Edward Said), epistemological studies constructed like political tracts (Paul Feyerabend). . . . One waits only for quantum theory in verse or biography in algebra.

Scholars remain somewhat uneasy with such category scrambling in organizational studies, however, particularly on this side of the Atlantic. A good deal of our research talk—occurring in seminar rooms, corridors, taverns, tribal gatherings, and texts—reflects a discomfort with the expansion and diversity of our distinctly low-consensus field. Some of the talk pits high-flying theorists against in-the-trenches researchers. The former see the latter as dumb-as-a-post empiricists, while the latter think the former should be peeled from the ceiling and sent on their way. Some of the talk is framed by the professional debates (and status seeking) in the field as members of one theory circle try to boost their reputations, resources, and recruits at the expense of other circles. And, of course, some of the talk simply reflects the way a new generation in any research-dependent and self-defined scientific field is trained and comes to regard many of its forebears as little more than

cognitively challenged plodders whose dated research styles and out-of-fashion ideas are a source of bemusement and ridicule.

A good example of such hostility is the current horror many students of qualitative methods develop for what they call, with spite, positivism. On occasion, the invective is deserved, as when some crude number-crunching device substitutes for thought. But students more frequently learn to use the term as an abusive catch-all for anything they don't like about theories, approaches, and findings coming from outside the theory and research circles with which they identify and are comfortable. This is particularly bothersome at the moment, because I can think of very few organizational researchers—qualitative or quantitative—who actually adopt the tag positivist.[5]

The problem here seems to be the familiar one of essentializing differences into dichotomies and then privileging one side of a dichotomy over the other. Such a process does not go unrecognized. In a pithy (and, in its entirety, rather droll) editorial statement appearing twenty years ago in the *Administrative Science Quarterly,* Karl Weick (1977: 138-139) took up these matters and noted that the journal "is not a sanctuary for the 'cute school' of organizational analysis . . . any more than it is a sheltered workshop for the 'opposite' group which presumably must be called the 'brute school.' "

A pose against privilege is struck in these lines and, while a dichotomy is put forth, it is simultaneously, if subtly, deflated by the judicious use of purposely snotty labels and quotation marks. How such a pose has played out in the journal is a matter of record and one I will soon take up. But before doing so, I offer some contextual remarks as an introduction and frame for my reading (and counting) of the qualitative research published in *ASQ.*

FORESHADOWINGS

The ideal reader of this book is a prospective, retired, certified, manqué, or practicing organizational researcher of either the qualitative or quantitative persuasion or both. Exclusivity is not my aim, for I am convinced that one form of research should inform the other. At any rate, a rigid compartmentalization between and among methods is impossible to maintain, since the differences are at best analogic, not digital, a matter of degree, not essence. More to the point, we seem to become obsessed with methods and insidious distinctions only when we have no story to tell.

Much storytelling, both manifest and latent, appears in the monographs and articles of organizational studies. Master themes embedded in the field at large concern the ways various forms of organization and administration alter the way we think, work, and live, as well as the ways organizations make us

smarter or dumber, better or worse, freer or more enslaved. Such grand themes reflect the growing power of organizations to give shape to our lives, and researchers as authors often wind up telling of the kinds of paradise gained or lost as a result of such power.

There is certainly enormous variation in how these themes are realized in print. Research reports can bundle various compositional elements such as theory, methods, and evidence in many ways but, over time, reporting conventions emerge and help shape the stories that are told. This is evident in the presentational order and style of research reports in *ASQ*. Articles published in the early volumes of the journal—both qualitative and quantitative—show considerably less format standardization and language formalization than those of later volumes.[6]

Closer to my concerns here, method discussions attached to qualitative research articles in the first ten or so years of the journal's history are characteristically short and spare, sometimes consisting of a single paragraph or a footnote (occasionally less) and blissfully unconcerned with elaborating or even justifying a given approach. Similar work published from the late 1960s on carries in the text itself solemn, lengthy, detailed, and increasingly adroit rationalizations of the methods used to carry out a particular study. Convincing readers today that a given study is more or less safe for science takes up a good deal of journal space.

Changes in the appearance of qualitative work may also signal broader shifts. Particular field methods, theoretical interests, and analytic techniques all rise and fall in legitimacy and use. Such changes are not necessarily a mark of progress or advancement, although they are often accompanied by such claims. Method shifts may reflect changes in the availability or scarcity of research funds, just as theoretical moves may reflect the social organization of the field as subfields grow or decline in number and influence. Work published in a mainstream journal such as *ASQ* offers a convenient place to track such changes in the field over time.

Over the years, *ASQ*—like most other research journals publishing organizational studies—has come to present work that typically falls into an empiricist genre, with strong scientific claims for constructing reliable and valid knowledge about behavior in and of organizations.[7] The name and dedication of the journal speaks to this aim—"advancing the understanding of administration through empirical investigation and theoretical analysis." Across the journal's forty-plus years, organizational research discourse has been defined, refined, challenged, and amended in various ways. Controversy over what form a proper organizational study should take surfaced early in the journal's history and has never really gone away.[8] A part of this debate centers on the respective roles qualitative and quantitative research should play in the field at large. An early manifesto issued in the name of peaceful coexistence (and still one of the best) was put forth by Boulding (1958: 14-15), who, in a

remarkably prophetic review of the first two volumes of *ASQ*, had this to say of what he called descriptive and historical work:

> It would be a great pity if a morbid fear of being "unscientific" were ever to lead to a suppression of this type of writing. The thing that distinguishes social systems from physical or even biological systems is their incomparable (and embarrassing) richness in special cases. Generalizations in the social sciences are mere pathways which lead through a riotous forest of individual trees, each a species unto itself. The social scientist who loses this sense of the essential individuality and uniqueness of each case is all too likely to make a solemn scientific ass of himself, especially if he thinks that his faceless generalizations are the equivalents of the rich variety of the world. I am not arguing of course that we should cease to make generalizations; this would be to abandon science altogether. I am merely urging that we should not believe them.

Whether or not blame rests fully on the morbid fear carried by solemn scientific asses, it is the case that at least some of Boulding's anticipatory anxiety was warranted. While qualitative work played a relatively large and prominent role in the journal (and the field) for a time, it went into a slump of sorts, from which it is slowly and sporadically recovering.[9] The slump cuts across the social sciences generally and is associated with particular historical periods, certain kinds of qualitative work, and the rise of quantitatively defined variable analysis emphasizing the formulation and testing of propositions drawn from existing theory and research circles.

This is a story that can be told best by looking at the publishing record of *ASQ* since its founding by James Thompson in 1956. While the story to be told is not so much about *ASQ* as about the relative place over time of qualitative research within organizational studies, an analysis of the journal is useful for examining that place, since it serves as something of a flagship for organizational scholarship as the field's oldest, most read, and quite possibly, most respected English-language journal now in circulation.[10]

FORTY YEARS OF *ASQ*

Looking across time at *ASQ* requires a number of classification decisions. Most critically, articles must be assigned categorical significance. The categories I use are but slight deviations of the four used by Boulding (1958) to type the content of volumes one and two of *ASQ*. The first is called "theory" and includes essays written to argue a given analytic, philosophical, or normative position into (or out of) existence. The category largely comprises writings that draw on other published writings (often the author's own) for empirical illustrations and theoretical exemplars. Little if any original data are presented

in these articles beyond the anecdotal. A catch-all or general purpose function is served also by this category, for in it I dropped those articles I could not comfortably stuff into one of the other three categories. A good illustration is Mechanic's (1962) much cited treatment of the organizational power of lower participants. The article puts forward a series of interconnected theoretical propositions of which some are supported by previous (and published) research studies, some by personal experience, and some by plausible but hypothetical examples. While the work draws on qualitative research results, it does not, in the main, present original empirical materials and hence seemed to fit my theory category best.

The second category includes articles that take up methods. It differs from theory in that articles assigned to this category deal less with organizing or assessing a given body of work than in examining the ways in which selected research questions have been and might be addressed. The work in this category tends to be more exhortatory than critical. It contains the fewest number of articles of the four categories and would be fewer still were it not for three special issues of the journal, each dedicated to a methodological cause: "Laboratory Studies of Experimental Organizations" in 1969 (vol. 14, no. 2), "Evaluation of Change Programs" in 1970 (vol. 15, no. 1), and "Qualitative Methodology" in 1979 (vol. 24, no. 4).[11]

The third and fourth categories include original research articles of the qualitative and quantitative sorts, respectively. I have struggled to compare the two in previous sections and will not go beyond my earlier comments other than to note that narrative representations characterize the articles coded as qualitative, while numbers, statistics, and testing hypotheses mark the quantitative. The former generally eschews variable analysis, the latter embraces it. Qualitative studies are further broken down later in this section.

The assignment of a particular article to a given, singular category is, of course, a matter of judgment—in this case, my judgment alone. For this reason, my numbers, like all numbers, should be taken with a grain of salt. Coding is a troublesome business. My problems began with simply identifying what was to count as an article.

For the record, I did not count editorials, news and notes, book reviews, short comments on research or research methods, critical responses to specific authors, or authors' responses to specific critics. I did count articles appearing in special issues. Far more problems arose in assigning articles to categories for, in truth, many authors not only provide extensive theory and method discussions but also mix qualitative and quantitative evidence and argument. My four categories are therefore hardly pure types. My imperfect solution to case-specific coding dilemmas was not to drop ambiguous articles from the pool or to assign an article to multiple categories but to type each article in one and only one category according to what I judged to be its "dominant" character or emphasis. This said, however, I must also point out that my aim

TABLE I.1 Categories of Work Published in the *Administrative Science Quarterly* (1956-1996) by Decade

	Theory	*Method*	*Qualitative*	*Quantitative*	*Total*
Vols. 1-10 (1956-65)	53 (25%)	18 (8%)	61 (28%)	83 (39%)	215 (20%)
Vols. 11-20 (1966-75)	45 (14%)	27 (8%)	26 (8%)	234 (70%)	332 (31%)
Vols. 21-30 (1976-85)	47 (16%)	31 (10%)	35 (12%)	183 (62%)	296 (28%)
Vols. 31-40 (1986-95)	14 (6%)	4 (2%)	36 (16%)	178 (77%)	232 (22%)
Total	159 (15%)	80 (7%)	158 (15%)	678 (63%)	1,075 (100%)

was not to produce definitive and air-tight categories (an impossible task, to be sure) but merely to track in broad and comparative strokes the rough place of qualitative work in *ASQ*.[12] Table I.1 provides a summary glance at the kinds of work published in the journal since 1956.

Several trends are apparent in Table I.1. Most striking perhaps is the rise of quantitative studies and the decline of other types of work. Qualitative articles bottom out in the second decade (vols. 11-21) but come back over the next two. Theory and method articles shrink most noticeably in the past decade (vols. 31-40). Opinions will differ, of course, as to what the proper proportions in each category should be or whether the changes in proportions have gone too far and fast or not far and fast enough. As an admirer of qualitative studies, I lament its decline and applaud its recovery even if modest. But this is another story. My purpose is to convey the publishing history of qualitative work in *ASQ* as a representative organizational studies journal.

With respect to Table I.1, the reader should keep in mind that the numbers alone do not tell the story, and many stories are possible. One story, for instance, is that editors and referees in any given period are simply and quite properly doing their jobs, and while they may see, for example, many qualitative papers, most are without merit, poorly constructed, uninteresting, and thus rightfully rejected. More submissions will not change the distribution but better ones might. Another story, equally plausible, is that relatively few qualitative papers appear in a given period because few are submitted. After all, qualitative research in organizational studies may be uncommon at times. Or perhaps those who do it periodically seek other outlets for their work. Some may prefer to publish research monographs or chapters in edited books that allow for lengthier research products than is typical for journals. Colin Firebaugh (1997: 772), the current editor of the *American Sociological Review*, put this account forward in a recent and refreshingly direct plea to potential contributors to the journal:

> The *ASR* publishes more quantitative articles because we receive more manuscripts of that type. The mix of articles reflects the mix in the manuscript pool. . . . The most recent data show that qualitative and theory papers are published at the same rate as quantitative manuscripts. If you want to see different types of articles published, please help us by submitting them.

TABLE I.2 Categories of Qualitative Research Published in the *Administrative Science Quarterly* (1956-1996) by Decade

	Vols. 1-10 (1956-65)	*Vols. 11-20 (1966-75)*	*Vols. 21-30 (1976-85)*	*Vols. 31-40 (1986-95)*	*Total (N = 158)*
Case studies	27 (44%)	15 (58%)	21 (60%)	13 (36%)	76 (48%)
Ethnographies	19 (31%)	5 (19%)	6 (17%)	11 (31%)	41 (26%)
Interview studies	13 (21%)	3 (12%)	3 (9%)	4 (11%)	23 (15%)
Linguistic studies	0 (0%)	0 (0%)	3 (9%)	4 (11%)	7 (4%)
Mixed (qualitative and quantitative)	2 (3%)	3 (12%)	2 (6%)	4 (11%)	11 (7%)
Total	61 (100%)	26 (100%)	35 (100%)	36 (100%)	

Both stories no doubt carry some truth, but neither addresses the temporal question: Why did qualitative articles drop off so precipitously during the mid- to late 1960s through the 1970s? In this regard, it is worth noting that this is the period in organizational studies when the influential Aston Studies were being published (in *ASQ* and elsewhere). It is a period marked by the popularity of contingency theories and the "discovery" of the environment as both an independent and dependent variable in the modeling of organization behavior. Comparative studies of organizations and organizational structures were on the rise, and such studies involved the analysis of large, standardized data sets and promised to put macro-organizational studies firmly on the scientific map. Not coincidentally, perhaps, this was also a period during which the IBM 360 computer became widely used, along with statistical software packages that made the analysis of large data sets considerably easier and more convenient than had previously been the case.

This was also a period of explosive growth of enrollments in American colleges and universities including, significantly, business schools. An enlarged supply of freshly minted Ph.D.s were needed to staff the teaching and research positions opening up. Jobs were plentiful. In this context, time-consuming, seemingly old-fashioned theory-building rather than theory-testing research techniques may well have fallen from favor among those pursuing research careers in organizational studies. That qualitative work would shrink during such a period seems, in retrospect, hardly surprising.

All this is speculative. These are not matters I can address with the materials in hand. What I can do, however, is look more closely at the kinds of qualitative articles published in *ASQ* to see just how qualitative research itself may have shifted over time. Table I.2 summarizes by decade the changing mix of qualitative work. I began with twelve categories for the work. Because this made for very small cell sizes, I kept recoding and cutting back on categories of qualitative research until I reached the magic number five, which seemed to me not too many, not too few, but just right. The five categories are case studies, ethnographies, interview studies, linguistic studies, and mixed qualitative and quantitative studies (with an emphasis on the qualitative materials).

While the numbers are small, the frequency, range, methods, settings, and compositional characteristics of the studies within the set are of interest. Case studies and ethnographies are by far the most common forms of qualitative research appearing in *ASQ* (about 75 percent of the total). Case studies concentrate on event sequences in particular locales and ethnographies on cultural portraiture and what it is like to be someone else. Various levels of analysis are represented in each category (e.g., groups, organizations, industries, states), and both include comparative works. Historical case studies, while few in number, are nonetheless present across all periods.[13]

Fieldwork of a continuous and intimate sort (i.e., reliance on long-term relations with so-called key informants) characterizes the ethnographies. Case study authors are more likely to rely on secondary data sources, brief rather than extensive forays into the field, and somewhat standardized (if open-ended) interviews with representative members of the particular unit(s) of study.[14] Induction—building concepts and theories from the ground up—is the favored (although not exclusive) mode of analysis claimed by authors of both ethnographies and case studies.[15] Ethnographers, however, are associated—sometimes closely, sometimes remotely—with given cultural theories and consequently tend to tell stories informed by the analytic traditions from which they come: materialist, symbolic, functional, linguistic, structural, cognitive, and so on. In both cases, however, authors emphasize local details, events, and members' perspectives. Conclusions are of a narrative sort and not easily detachable or decontextualized. Discipline-based theorizing tends to be light and used more to establish plausibility and invoke generality than to be tested. Boulding (1958: 5) considered such writings to be "travel over the field of study" and vital to organizational research:

> In every field there is a need for writing where the main objective is to extend the reader's field of acquaintance with the complex cases of the real world. Such writing does not have to be very exact or quantitative; it does not even have to formulate or to demonstrate hypotheses. It constitutes, as it were, travel over the field of study. Travel is certainly not enough, even for a geographer, but we would feel, I imagine, that a geographer who had never traveled would be under a serious handicap. Similarly the student of organizations who has never, even vicariously through reading, been in a hospital, a bank, a research laboratory, a large corporation, a Soviet factory, a revolution, an Egyptian civil service department, and so on, has missed something. His generalizations are apt to be based on too narrow a selection of the field.

The list Boulding rolls out specifies a number of delightfully diverse qualitative research sites plucked from articles appearing in the first two volumes of *ASQ*. The ethnographies and case studies that followed continued to honor Boulding's travel mandate of taking readers places they presumably had never been, but authors also began to attach more theory—both off-the-

shelf and grounded—to their work than was true for the early period. Such explicit theorizing—no doubt nudged on by editors and reviewers—added to the specification of just what a given case or ethnography was about and thus sharpened (and limited) its focus and perhaps helped preserve a place for case studies and ethnographies in the journal.[16]

Comparing case studies and ethnographies, the former appear more frequently than the latter, but the gap between the two narrows considerably in the past decade, where ethnographies make up a little more than 30 percent of all qualitative work appearing in the journal. Part of this shift may reflect the slow but steady development of an organizational culture discourse within and across various research communities as well as the spread, and homecoming, of ethnographic research techniques. Ethnographers are as likely these days to be plying their trade in hospitals, research labs, tourist hotels, high-tech corporations, and the local public schools as in the remote villages of highland Burma or the urban neighborhoods of north-end Boston or southside Chicago (Clifford, 1997).

Interview studies are another form of qualitative research counted in Table I.2. My coding is stringent, since I included in the category only those studies that were primarily interview based. Such studies appear to have almost dropped off the research map at *ASQ*. Although interviews remain a much mentioned and more or less standard feature of qualitative work, particularly case studies, few research reports now rest on interviews alone, or so it is claimed.[17]

Linguistic (or language use) studies represent a relatively recent addition to *ASQ*. They rest on the researcher's interpretation of naturally occurring talk or the production of text taking place in and around (and usually about) organizations. This work is generally quite theory driven, informed by such related analytic fields as conversational analysis, linguistic anthropology, sociolinguistics, and narrative or literary theories. Interviews and ethnographic observations help frame such studies, but the researcher's interest is not with a case or culture per se but with matters such as speech acts, turns of talk, communication genres, and the interpretive practices of organizational members.

The final category of Table I.2 is called "mixed" and is filled up by articles that blend quantitative and qualitative methods, and like linguistic and interview-based studies, the number appearing in this category is small. The articles of this category exhibit a good deal of number mongering and crunching. However, as mentioned previously, to be assigned to this category, the qualitative orientation of the writing must (in my judgment) hold sway over the quantitative. This is occasionally difficult to discern. An example of the coding problems I produced for myself with this category is Elsbach's (1994) numerically dense study of talk in and about the California cattle industry. But her numbers serve largely to provide a context and topical frame

for the account giving (and accepting) practices of members of the industry, which she uncovered through extensive fieldwork and content analysis. This I tagged qualitative (and mixed). A counterexample is my own study of police socialization (Van Maanen, 1975). In this article, a quick and glossy ethnography frames a longitudinal analysis of measured job attitudes treated as focus variables. This I coded quantitative.

That, in a numerical nutshell, is the place of qualitative research in *ASQ* over forty years. It is an up-and-down and (partly) up picture, with some period variation existing among the five types of qualitative work coded and reported. Taken as a whole, this work is conceptually diverse and of rather high quality and interest. Much of it is distinctive. Sobriety, attention to detail, care without obsession, a balance of the abstract and concrete, and (usually) an easy rather than relentless use of theory, imagery, and metaphor are integral to the continued legitimation and place of qualitative work in the journal.

Such matters are, however, set partly by fashion and the available genres of the time. Someone writing with Gusfield's (1958) literary sensibilities in "Equalitarianism and Bureaucratic Recruitment" or with Strauss's (1962) functional orientation in "Tactics of Lateral Relationship," or with Katz's (1965) disregard of method in "Explaining Informal Work Groups in Complex Organizations" might find it difficult to publish in *ASQ* today without attending to current methodological reporting conventions, changes in the theoretical interests of readers, and the apparent decline in the use of what Daft (1980: 623-624) called a particularistic but inherently ambiguous "high variety language." This is not to say such articles are without contemporary merit. To some, they are classics, having told stories that linger in the mind. But we should remember that texts do not remain the same over time, despite the fact that as written products they are fixed. From the point of view of their reception, research writings constantly change as readers reinterpret and redefine their merits (and demerits) in light of changing theoretical projects in the field and additional evidence coming from other studies. Of course, from the point of view of inception, styles of writing up (or down) qualitative studies change too in light of the continuous reinterpretation of past work. But some characteristics of qualitative research reporting have stayed the course. Three are particularly impressive.

First, I think much of the writing exhibits a relative freedom from analytic and technical jargon. Putting readers into the shoes of those studied continues to be a priority for most qualitative researchers, and this descriptive aim is best met through the use of a general rather than a specialized language. Theory is a must these days, to be sure, but theory seems more to animate than to motivate qualitative work, except perhaps in the linguistic domain. Authors still borrow concepts from broad public discourse (e.g., games, theater, politics, popular culture) and make much use of so-called native or

member terms in naming, framing, and organizing research reports. For better or worse, Glaser and Strauss's (1967) thirty-year-old text, *The Discovery of Grounded Theory,* remains a source of inspiration to qualitative researchers and is still frequently cited.

Second, a good deal of qualitative work draws on fields that are somewhat removed from the mainstream of organization research and theory. Such areas include fieldwork-based sociology, semiotics, ethnomethodology, phenomenology, history, critical theory, and current streams of thought in cultural anthropology, symbolic interactionism, and narrative theory. This provides a distinctive conceptual landscape (and reading list) for both neophyte and experienced researchers. It also promotes what I regard as the slight but noticeable literary air associated with qualitative research writings, even in organizational studies, where loan words come mostly from science, not the humanities. Readers untutored in the research traditions, topical interests, or even theoretical questions central to a qualitative paper can nonetheless read and understand the work. Such writing is inviting rather than intimidating and, when well done, stimulating rather than boring. The voice reporting qualitative work is not always the omniscient voice of science (the voice from everywhere) but occasionally, and perhaps increasingly, the personal voice of a situated author with a story to tell. In this sense, narrative may well be the latent paradigm associated with qualitative work, rather than any of the manifest disciplinary, organizational, or research theories floating around and through such work.

Third, and finally, the methods of qualitative research, as critics gleefully point out, remain loose and unspecified. Any given study tends to be methodologically promiscuous. Even singular methods escape formalization. Interviews, for instance, carry situational properties that will not go away, and most experienced interviewers recognize that a "typical" qualitative interview is, at best, a construct known only in the ideal (e.g., Mishler, 1986; McCracken, 1988; Kvale, 1996). We know, too, that participant observation is always biographically shaped, and fieldwork guides still cannot get much past the simple cautionary tales of seasoned veterans recounting their experiences (e.g., Emerson, 1983; Sanjek, 1990; Lareau and Shultz, 1996). All this suggests to me a rather broad indifference (if not hostility) on the part of qualitative researchers for the endless efforts of other organizational scholars to define methodological rigor and, on its sturdy back, develop general and refined theory. In most respects, qualitative research reports continue to be improvised and crafted, inspired as much by artistic, aesthetic, and humanistic concerns as by social theory and research design. I am not unhappy with this steady state of the art. Yet such remarks border on ideological pontification. Some examples are needed. They take up the rest of this book.

COMING ATTRACTIONS

The following chapters present thirteen sterling illustrations of substantively focused and theoretically relevant qualitative research reports. All are articles appearing in *ASQ* sometime between 1958 and 1995. All are favorites of mine, and a few are rather famous across organizational research communities. Most important, however, they exhibit, as a group, a broad range of research styles and careful scholarship. They are models of qualitative research put forth in journal-length style. I regard each article in this collection as something of an exemplar and thus worthy of our field's highest reward, imitation. They are not the only exemplars I might have selected from the *ASQ* archives, nor do I maintain the illusion (or delusion) that other readers of the archives would make the same selections. For this collection, I deliberately sought variety in methods, topics, analytic style, level of analysis, publication period, and so forth. I also wanted to avoid celebrating work in which I had an acknowledged hand as a prepublication reader, however slight my hand and however much the work deserves celebration. Such criteria eased my task considerably.

The studies are spread across four sections. Each part of the collection is roughly distinguished by a topical concern rather than a particular level of analysis. Part I presents three studies of organizational processes. Part II includes three articles about groups in organizations. Part III offers four representations of organizational identity and change, and Part IV includes three depictions of the societal and institutional environment. These categories should not be taken as representative of the topics with which qualitative work is solely concerned. I used these categories to group the articles I had already selected as personal favorites of mine and hence candidates for inclusion in this collection. On my original list were about thirty articles, which I sorted into topical areas. I then eliminated those articles that seemed to form a class by themselves or closely resembled another article in the category that I happened to like better. I wound up with about twenty articles spread across the four categories and made the final selections subject to the criteria described above.

As the section heads denote, there is a considerable range of theoretical interests covered by the collection. There is substantive range as well, for the organizations studied in the collection include factories, churches, universities, engineering groups, fisheries, voluntary organizations, basketball teams, public organizations, pop music recording firms, and more. In terms of method, there are six case studies in the collection, four ethnographies, two mixed-method reports, and a single linguistic study. As for the historical periods represented by the readings, three come from the foundational era of 1956-1965 (vols. 1-10), two from 1966-1975 (vols. 11-20), three from

1976-1985 (vols. 21-30), and five from the recent period, 1986-1995 (vols. 31-40).

The authors of the works display a variety of disciplinary backgrounds, including sociology (the majority), political science, communications, management studies, and history. Some of the authors were graduate students at the time their articles were drafted (Robert Gephart, Nicole Biggart, Patricia Denton, and John Maniha). Others were well-established and well-known scholars (Burton Clark, Robert Cole, Richard Cyert, and William Dill). Many were relatively junior members of their respective guilds (Peter and Patricia Adler, Ann Langley, James Barker, Mayer Zald, Paul Hirsch, Reed Nelson, Petter Holm, Charles Perrow, and James March). Only one of the thirteen articles comes from a special issue (Cyert, Dill, and March); the rest went through the regular *ASQ* submission, review, and publication process.

Perhaps the most intriguing and engaging feature of this collection is the writing. Each piece can be read on its own with some pleasure even if the theoretical claims and substantive topics are remote from a reader's scholarly interests. Writing to inform as well as to please is no easy matter, and how to do it can only be insinuated, not preached. There probably are rules for writing the persuasive, memorable, and publishable qualitative research article but, rest assured, no one knows what they are. Examples must be our guide. In the end, all we can do if we are truthful is to tell a story. Such a tale will inevitably include certain nonrepeatable, particularistic elements side by side with authorially drawn analogies to stories of other fields and other times, both near and distant. The stories told here are of just such a sort. We need more.

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NOTES

1. Unlike organization studies in Europe, organizational studies in the United States stands on its own as a rather autonomous field, complete with internal rifts and divisions between and among those who are called macro- and micro-organizational theorists. It is a field that represents a growing assembly of rather diverse scholars frequently trained and/or working in business schools and held together largely by convenience, will, and a few summer meetings. For better or worse, organizational research in Europe, notably in the United Kingdom and France, is tied more closely to the reference disciplines mentioned in the text, especially sociology and psychology. Useful and recent discussions of these matters are found in Hofstede (1996), Koza and Thoenig (1995), Child (1995), Üsdiken and Pasadeos (1995), Guillén (1994), Chanlat (1994), Boyacigiller and Adler (1991), Cooper and Cox (1989), and Hinings (1988). The Atlantic, it seems, separates rather than connects the research worlds of Europe and English-speaking North America.

2. These are precisely the features of qualitative work many critics find most troubling. Because qualitative research is not impersonal and systematic but tries to take into account unfolding historical and situational detail, such work is said to be unable to produce the sort of objective and reliable knowledge necessary for prediction. Quantitative work, in contrast, tries to be systematic and impersonal but is faulted by other critics who say it leaves too much out and promises far more predictive power than it can deliver. Epistemological debates turning on these matters have been around for a very long time and are not likely ever to be settled. As critics of our research endeavors, epistemological theorists are necessary but annoying, since their job is to uncover and question all the taken-for-granted research conventions that make given lines of study possible. They are very hard to please. Becker's (1993: 227) advice for dealing with such Cassandra-like characters in our midst is to "listen to them, be polite, take what is usable and finesse the rest." This is good advice, and more of it can be found in Becker (1996).

3. A qualification is necessary here. Adherence to this or that method of data collecting does help to define membership in a particular theoretical school, and such an association can sometimes lead to a technique-oriented definition of qualitative or, for that matter, quantitative work. Symbolic interactionists are recognized, for instance, by the cult of participant observation surrounding their work, ethnomethodologists for their passion for audiotaping and videotaping anything that speaks or moves, status attainment researchers for their systematic use of path analysis, and network researchers for their use of matrix algebra and scaling techniques. How tight such conceptual linkages are is often brought home when several methods are combined in a single study and the work hailed as a breakthrough, a daring challenge to methodological monotheism—as when, for example, a discourse analysis is combined with an ethnographic description or a network study is grafted onto survey research. While virtually all qualitative studies are blends of various techniques, published method discussions usually acknowledge or imply that the work relies more on one method than others.

4. The narrative turn in qualitative work is perhaps most advanced in ethnography, where Clifford (1997: 67-68), among others, argued that a "literariness has returned." It has returned along with strong claims about the prefiguration and rhetorical nature of all social science data. More broadly, Ewick and Silbey (1995) pointed out that narrative in social research can serve as an object of study, a method of study, and/or a product of study. When narrative inheres in the scholarly production of books and articles, social researchers themselves serve as storytellers. It is worth noting, however, that narrative as a product of social research is not intended to be decorative or merely a way of making a work readable, palatable, and audience friendly. Narrative is a cognitive device, a way of ordering and interpreting the social world. Good things are now being said of narrative in many circles. See, for example, Mitchell (1980) and Bruner (1990), for broad discussions of narrative; Reissman (1993), for a look at some techniques of narrative analysis; and Czarniawska-Joerges (1997), for a study of organizational narratives put forth (thankfully) in narrative form.

9. Much article-length qualitative work in organization studies appears, as always, in journals off the mainline, such as the *Journal of Contemporary Ethnography, Qualitative Sociology, Human Organization, Human Relations, Organization Studies, Journal of Management Studies, Journal of Applied Behavioral Sciences,* and more recently, *Organization, Journal of Management Inquiry, Journal of Organizational Change Management, Qualitative Inquiry,* and *Studies in Cultures, Organizations and Societies.* A good deal of qualitative work is also published in monograph form and as chapters in edited volumes. Of most interest, perhaps, is that a good deal of qualitative work is cleverly brought into quantitative journal articles as an adjunct to variable analysis. Qualitative cases are often needed to put a numerically driven story into words by offering plausible examples of what a specific variable relationship might mean. My view on much of this is that the more qualitative work is tamped down, ignored, or marginalized in mainline journals, the more repression charge it accumulates and the more likely, as Freud might tell us, it will return, rebound, erupt in quite possibly unexpected but vigorous ways. This may be represented best by the specter of postmodernism hanging over the organization studies field and making many of us nervous (some cheerfully so). See, for example, Clegg (1990), Gergen (1992), Reed and Hughes (1992), Smircich, Calás, and Morgan (1992), Brown (1995), Friedland and Boden (1994), and Boje, Gephart, and Thatchenkery (1996).

10. This rather sweeping assessment is based not so much on *ASQ*'s circulation rates (holding steady at about 4,500 subscribers worldwide) but on its consistent placement among the top three to five most cited journals in the Social Science Citation Index (SSCI). As the SSCI figures make clear, *ASQ* articles are cited more frequently and live longer in academic communities than articles appearing in other organizational research journals. What is also clear is that a publication in *ASQ* bestows legitimacy on an up-and-coming organizational scholar, and it is probably the case that more than a few academic careers have been made (or broken) by publishing decisions made by the journal. For a somewhat different view on the ranking of organizational research journals, see Sharplin and Mabry (1985).

11. The effect of the fifteen special issues of *ASQ* published as of 1995, while tangential to my concerns, does raise intriguing questions. Special issues are put together to raise reader consciousness, which, like the Titanic, is deemed by an editor to be worthy of raising. As such, special issues are meant to increase scholarly interest in a particular area. Whether or not they do so is debatable. The 1971 special issue "Laboratory Studies of Experimental Organizations" (vol. 14, no. 2) edited by Karl Weick, seems, in retrospect, more an obituary than a tribute or call to action, for it marked the end of an era rather than a beginning. The 1983 special issue "Organization Culture" (vol. 28, no. 3) edited by Mariann Jelinek, Linda Smircich, and Paul Hirsch signaled the rise of a subfield in organization studies that is still going strong. Of particular interest to this volume is my own 1979 special issue "Qualitative Research Methods" (vol. 24, no. 4). The data presented in Table I.1 on the publication rates of qualitative research in *ASQ* are none too clear. Comparing all volumes prior to the 1979 special issue with all those that followed (vols. 25-40), there is no change—15 percent of the articles in the period preceding the special issue are qualitative and 15 percent following the special issue are qualitative. If I equalize the examined volumes between the pre- and post-special issue (vol. 9 to vol. 24, no. 3 and vols. 25-40), the percentage of qualitative articles appearing in the journal increases from 10 to 15 percent over the period. This cut of the publication evidence can perhaps be justified on the grounds suggested by Daft (1980), that the early years of *ASQ* were experimental or unsettled and therefore unhelpful when depicting publishing trends.

12. As an example of an exemplary quantitative content analysis, this motley, judgmental, count-and-classify approach of mine leaves much to be desired. It owes more to my daughter's Sesame Street calculator than to the Cadillac of a computer parked on my desk. It is, at best, a quick and personal glance at the journal's history. But, I hasten to add, such a glance required me to read forty years' worth of articles published in the journal. Some were read leisurely with pleasure, some were read carefully with annoyance. Most were read swiftly with coding on my mind. To gain a reliability estimate of my coding practices would require at least one other reader, but I could find no volunteers and had no budget for hired hands and eyes.

13. I include as historical case studies Dale's (1956, 1957) delicious biographies of Alfred P. Sloan and the Du Pont Company. With Dale's work, biography became extinct in *ASQ*.

5. The reverse is true as well, for much of what I consider quite sound qualitative work is labeled by my quantitatively oriented colleagues as undisciplined, soft, squishy, and lacking rigor. Some of the problems created by both quantitative and qualitative research bashing may stem from the way students are trained. For example, in a generic graduate course on research methodology, students are usually asked to examine various research strategies, techniques, and philosophies and report back, parrotlike, the advantages and disadvantages of each. Contrasts are emphasized, and students often understandably wind up preferring what their teachers prefer. Such courses seem to produce a learned incapacity to go out and actually do research of different sorts, for learning about the armed rival methodological camps in no way prepares one to face field data, which are far messier than any one camp is willing to admit. This is a point that comes through quite clearly in many of the selections included in the edited volume Frost and Stablein (1992) put together to honor what they considered exemplary organizational research projects.

6. In this regard, Davis's (1971) observations of what makes a given piece of research interesting (and publishable) are relevant. Among the reader-response propositions he puts forth is one suggesting that authors who deny weakly held assumptions of their audience will have their work noted (or, more exactly, footnoted), while those who deny strongly held assumptions will have their sanity questioned. The difference between the inspired and insane is located, according to Davis, in the strength and tenacity to which an audience holds onto its assumptions when they are violated or attacked. Such assumptions ground stylistic and representational conventions as well as substantive or theoretical claims. Assumptions surrounding the proper format, mode of expression, and rhetoric all appear to be strongly held among readers of and reviewers for established research journals—and are often inscribed as submission guidelines on the back pages of the journals themselves. This suggests that work challenging such guidelines and the assumptions on which they rest best look elsewhere for publication. On the role of rhetoric and style in science, see, for example, Gusfield (1976), Bazerman (1981), McCloskey (1983), and Locke (1992). See, too, Perrow (1985) for a wry treatment of what kinds of contributions are most likely to be welcomed by mainstream journals and why researchers of all ages and types should try—occasionally at least—to direct some of their work toward such fussy publications.

7. This is not to say that stylistic innovation and various forms of genre bending are absent from the journal. Superb examples of highly stylized writing from the very early days of *ASQ* include Richardson (1956), Presthus (1958), Gusfield (1958), Stinchcombe (1959), and Boulding's (1958) rogue and neglected essay cited throughout this introduction. Some of the same provocative (and evocative) playfulness shows up periodically across the journal's history. Editors, it seems, are willing to loosen the house strictures against an author's personal expressiveness now and then, with the apparent rule being the more eminent the author, the greater the loosening. Certainly, the field of organizational studies has its share of good writers and even a few powerful stylists with uncanny abilities to put forth gentle ironies, work with active grammatical constructions, and develop arguments supported by apt analogies and metaphors. Such relatively rare but felicitous trope deployment in *ASQ* beyond the foundational era is exemplified by Weick (1976, 1993), Brown (1978), March (1981), and, most recently, Leavitt (1996).

8. Challenges to what some claim is the "orthodox" research discourse promoted at *ASQ* sometimes appear in the pages of the journal itself. See, for example, protests lodged by Benson (1977), Morgan (1980, 1983), Astley (1985), and Astley and Van de Ven (1983). Several studies on the nature of this supposed orthodoxy exist. The most detailed and more or less affectionate is Daft's (1980) examination of the language used in the journal from 1959 to 1979. Boje, Fitzgibbons, and Steingard's (1996) much less affectionate appraisal took the journal to task for overlooking what they call "critical postmodern discourse." Weick (1985) also examined the character of the journal from the lofty position of a former long-term editor and, perhaps not unexpectedly, found things more or less in order, with few traces of a smothering orthodoxy at work. Recently, the editors have encouraged controversy in the form of personal thought, argument, opinion, and protest (of a mild and mannerly sort) through the publication of invited essays (and commentary) appearing in a stand-alone segment of the journal called the "*ASQ* Forum." See, for example, Sutton and Staw (1995) and Stern and Barley (1996).

14. Case studies can be quantitative as well as qualitative, representative as well as deviant or exemplary. In an important sense, however, all studies are case studies if only because all analyze social phenomena that are highly specific in time (by historical moment) and place (by group, organization, industry, nation state, or planet). The qualitative case studies of *ASQ* are close readings, put in writing, of certain specified social scenes and mix representational and interpretive elements. The worlds studied are generally seen as temporal sequences of events rather than sets of forces. The most important question is not so much "What is a case?" but, rather, "What is this case about?" Putting forth a distinctive, logical, and well-argued answer in terms that go beyond the case materials themselves is no easy task but separates the good from the not-so-good cases. Superb discussions of contemporary case studies are found in Feagin, Orum, and Sjoberg (1991) and Ragin and Becker (1992). An intriguing comparison of sociological case studies written by French (including French Canadian) and U.S. authors is provided by Hamel (1993).

15. Students are sometimes surprised and disappointed to discover that some case studies and ethnographies they regard as exemplars of inductive, bottom-up grounded theory are actually constructed on a rather tight, a priori conceptual framework in which field materials play only a minor illustrative role in telling a story driven by top-down, theoretical concepts. This is usually the reason why data and analysis are joined together so effortlessly by an author who produces a tidy and wonderful "just-so" story. To some extent, all fieldwork-based studies are subject to such a critique, for theory penetrates deeply and unavoidably into what we see, hear, record, and critically, write. The role of theory in contemporary fieldwork and qualitative research generally is taken up in several recent and sophisticated method texts. See, for example, Atkinson (1990), Emerson (1983), Wolcott (1995), and Silverman (1993). See also Manning's (1979) treatment of metaphor as a theoretical framing device underpinning fieldwork-based organization studies.

16. For better or worse, theory has worked its way deeply into qualitative work. "Having a theory" is today the mark of research seriousness and respectability. Theory is, of course, convenient and helps to organize and communicate unwieldy data and simplify the terrible complexities of the social world, matters that may well be more important to the field than whether or not a given theory is true or false. It also bestows rewards, since those who profess and publish theories that others find attractive are feted and promoted—or, if their theories prove unattractive, are unpublished, reviled, and fired. What is clear, however, is that the authors of case studies and ethnographies appearing in *ASQ* have increasingly tried to frame their work theoretically, making sure that the theories credited or discredited by their facts and stories are of current interest to readers. One can now be sure that when "travel over the field of study" makes its way to print, a theoretically informed guide will be along for the ride.

17. The hedge is important here, for no study represents a pure methodological type. As noted in the text, most qualitative studies appearing in *ASQ* mention interviews as one of several data sources, but few claim interviews as an exclusive or even primary source. This, as Table I.2 indicates, is more true today than yesterday. A few examples of splendid research articles that are primarily interview-based include Blau (1960), Wildavsky and Hammond (1965), Wildavsky (1972), Sutton (1987), and Vaughan (1990).

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