Chapter 5
Some Notes on the Importance of Writing in Organization Studies[1]

John Van Maanen
Sloan School of Management
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

This is an argumentative missive designed to promote interest in the ways research reports are written. It is addressed to colleagues and hastily put together in order to meet a deadline. In what follows, I deal with all forms of research output in organizational studies, quantitative as well as qualitative. In a nutshell, my point is that all research reports can be read for their style as well as for their science. More important, however, if our job is to convince readers of the worth of our studies, we must pay greater attention to the narrative conventions of our trade than we presently do.

In making this case, I draw extensively on ideas put forth in *Tales of the Field* and, in a sense, am doing little more here than reformulating what I have already said. But since this is a new piece of writing, it does have its idiosyncrasies, among them: a focus that blissfully disregards the boundaries built by a number of substantive, theoretical, and methodological interest groups in organizational research; an attempt to be immodestly provocative in the service of calling out certain representational dilemmas that extend well beyond our field(s); and the adoption of a narrative style that is informal and cryptic, more in the tradition of a fireside chat or a series of postcards than a somber tome on sacred topics.

To get to the heart of the matter, the shameful truth of our organization research trade is that we traffic in communication, and communication implies that we intend to alter the views held by our readers. In large measure, our task is rhetorical, for we attempt to convince others that we've discovered something of note, made unusual sense of something, or, in weak form, simply described something accurately. In essence, we try to persuade others that we know what we're talking about and they ought therefore to pay attention to what we are saying. We do this by means of text—the written word.

Things get interesting here because, when it comes to writing, the literature in organizational studies is silent. While our findings, theories, and methods are well inscribed in an ever-increasing number of journals and books, there has been little attention given to just how these various writings persuade. How we get from printouts, survey responses, fieldnotes, experimental designs, statistical tests, or literature searches to representations of organizations in printed form is not discussed. Since some writings seem to generate a good deal more reader response (altered views) than others, it appears reasonable to ask why this might be so and inquire as to what authorial styles lie behind such success.

To some organizational researchers, this may seem an odd question since it contradicts what we should be doing—namely, going to the field and collecting data in the hope of explaining what organizations (or the people in them) do or should be doing. A student who wishes to sit back and worry about the plots and subplots of job design writings or the presence of irony (if any) in population ecology papers or the use of metaphor in strategy texts will be seen, no doubt, as a bit strange. The student should be out interviewing people, running data, and setting up lab experiments, not doing some silly "lit crit" in the library.

Other researchers, perhaps more forgiving of odd pursuits, might still find the question off-putting if not a little embarrassing. To study the allegedly straightforward writings of organizational researchers is simply not worth the effort. Writing in organizational studies is, after all, a minor, mop-up activity, something one does at the end of a research endeavor that amounts to little more than a kind of plain-speaking, technical account of what was done and what was found in a particular project. It is one thing to attempt to decode the narrative structure, characterization ploys, double entendres, plot lines, and authorial voices in the works of Gabriel Marquez, Saul Bellow, Joan Didion, or any other acknowledged star of the literary scene, but it is another matter entirely to worry about the same things in the works of Jeffrey Pfeffer, Victor Vroom, Paul Lawrence, or Michael Porter.

More fundamentally, however, I think our silence rests on the unexamined but nonetheless pervasive belief that if we did start looking closely at the ways our major and minor works are put together textually, we might not like what we find. Our silence on these matters results from the fear that if we looked closely at our use of imagery, phrasing, allusion, analogy, and authority, we might discover some literary chicanery or authorial trickery that would undercut our ability to make claims about the truth of our findings and theories. If style were shown to play an important persuasive role in accounts of organizational research, then a corrosive relativism might take over and authors of organization studies would become players in a mere game of words, trapped in the same "prisonhouse of language" thought to be occupied only by poets and novelists. From this perspective, it is best to act like an ostrich and not look.

My sense is that we hold these views (or parts of them) because we belong to a research culture built on some widespread myths about how our writings inform our readers and convince them (sometimes) that what they are getting is the real, unadorned, certified, honest-to-God truth. Such myths are all linked (in various ways) to a sort of Metamyth of Science, coded, in my view, by our deeply held assumption that it is possible, indeed necessary, to separate the facts of the "real world" from our ways of getting at those facts and from whatever prior assumptions we have about what constitutes those facts in the first place. This Metamyth has come under increasing attack during the last 20 or so years and I will not continue the barrage here. What I do want to examine are some correlative myths of research as a way of making the case for taking our writings on organizations more seriously than we now do. I have four in mind.

Myth 1: Organizational research reports are transparent; they involve few authorial choices and stand in a one-to-one correspondence to the world described in the report.

A cautionary tale is useful in debunking this myth. It is a personal tale and treated here as an allegory. When I returned from the field to sit and write what was eventually to become my doctoral thesis, I was told by my worthy academic advisors to simply write up what I had observed in the field. My data consisted of a lengthy stay in the organization of study, collections of informal and formal interviews, official documents, surveys, and the like, all inscribed by such means as fieldnotes, computer printouts, transcripts, and statistical tables. I turned first to write ethnographic descriptions, quickly became flustered and frustrated at how difficult this proved to be, and eventually turned to writing up first the results of my questionnaire probes. Here I was guided by a set of explicit analytic procedures and a boiler-plate model for textual organization. The fiction conveyed by the guides I used was that anyone having access to the same sort of data I had would write up the results in pretty much the same way. Quality control was simply a matter of ensuring the written product had all the relevant parts put together in the prescribed order and explained in terms of a prevailing orthodox theory.

My troubles with the ethnographic portions of the research write-up are perhaps not so surprising since many have labelled fieldwork-based writings as the least standardized and most problematic of all research reports. Yet, even with the far more rule-based reporting conventions associated with questionnaire studies, innumerable writing choices were demanded of me, choices of the sort that made a mockery of any claim that the data or method determine the report. To take but one choice point as an illustration: What orthodox theory would I use? Many explanatory frameworks could be brought to bear on the materials I had gathered. In the end, I picked those my advisors thought relevant and handled them tenderly in the write-up. But the point is that my report contains a set of solutions to writing problems for which no obvious criteria exist (then and now). What was put in and left out of my report was largely a matter of trying to work within a given narrative structure that I thought would lead to an acceptable thesis. Style and imagination (or lack thereof) consisted of working within the dimly perceived conventions of the structure I had selected (or, put another way, the structure that had selected me).

In brief, even the most standardized of research reporting formats present arbitrary narrative choices for an author. Canonical count-and-classify studies are no less subject to rhetorical analysis than fieldwork accounts based on primitive participant-observation techniques. The former may be even more subject to such study since their reporting formats have long since petrified into a few mock-science genres and their users may be unaware of the impact such genres have on readers and writers alike. At any rate, there is nothing transparent about research reports. Even the most popular varieties require an uncountable number of critical authorial choices of the sort that determine the style and rhetorical power of the report.

Myth 2: Organizational research reports are convincing because of their facticity.

This myth suggests that our research writings are valid on the basis of the facts they present. Somehow the nature and the volume of facts convince; a study of 100 organizations is more convincing than a study of 50 or 5. Statisticians are partially to blame for this fallacy, but not entirely, for ethnographers, too, seem eager to stuff their texts with detail on detail. While we would all agree that we typically leave far more out of our research reports than we put in (especially given our tendency to now condense everything into a short, journal-length article), we still seem to hold to a more-is-better ethic when it comes to reporting our research findings.

Yet, when it comes to producing convincing work, this more-is-better ethic doesn't get us very far. Many, if not most, of our most convincing and sacred of texts contain very little data or report very few findings. The work of Jim March, Karl Weick, Charles Perrow, Edgar Schein, and the notorious Tom Peters come immediately to mind in this regard. James Thompson published what must be regarded as an astonishingly data-poor book, but it has had enormous influence on the field. When it comes to creating a convincing text, clearly something more than the facts per se are at issue.

Myth 3: Organization research reports are convincing because of the theory they display.

This is the flipside of the previous myth. If the facts do not convince, then it must be the theory. Theory convinces because of its elegance, parsimony, problem focus, novelty, sweep, match with the data, or any other word or phrase commonly used to elevate the status of some theory in contrast to another. In these post-positivist days, this myth is popular since the facts are no longer seen to speak for themselves. Unfortunately, however, the belief that theory convinces is patently false since theory itself is both contentious and ephemeral. As we are belatedly coming to realize, holders of one theory are unlikely to be convinced of the worth of another based on research alone. More important, perhaps, theory comes and goes in the social sciences, and does so swiftly.

Consider the high "unifying" theories that once had their day: functionalism, structuralism, functional-structuralism, exchange theory, behavioral theory, and so on. So, too, with the "middle-range" theories of structural contingencies, institutionalization, resource dependency, and social networks. Schools of thought rise and fall: the Chicago School, the Carnegie School, the Vienna Circle, British Empiricism, French Structuralism, etc. All have had great success; all have faded with time. Builders of these proud towers of theory develop research programs, sponsor journals, speak with authority on the issues of the day, train apprentices, and, in general, behave as if Thomas Kuhn were but an annoying figment of the imagination. Invariably, however, new theory replaces the old and we carry on.

Organization theory is no slouch in this game. We have many innovative theorists able to attract followers to a given theoretical circle or gang. The enthusiasm generated for each approach sometimes seems similar to that generated by pet rocks or hula hoops; when fashions shift, no one wants to be caught pursuing last season's theory. This is not to say that theory can't be instructive or insightful or provide readings for organizational events that are inventive and altogether plausible. What theory allows is for a coherent story to be told. But, I submit, it is the story that convinces, not the theory.

Take, for example, a list of famous names and studies in organizational research: Bill White in *Street Corner Society*; Melville Dalton in *Men Who Manage*; Fritz Roethlisberger and W. J. Dickson in *Management and the Worker*; Howard Becker and associates in *Boys in White*; Paul Lawrence and Jay Lorsch in *Organization and Environment*; Don Roy in "Bananatime"; Peter Blau in *The Dynamics of Bureaucracy*; and, more recently, Bill Ouchi in *Theory Z*; Andrew Pettigrew in *The Politics of Organization*; and Michael Buroway in *Manufacturing Consent*. All are classic studies and all are still read today despite the discredited theory developed in each study. What these authors accomplished in their respective works was to create a narrative and use theory to abbreviate, organize, and embed certain obdurate facts such that a convincing account resulted. Theory was a tool in these studies, a kind of narrative guide or device that helped convince readers that some sense had been made of the world. When such tools travel to new settings or harden with repeated use, they are less effective and convincing. This, indeed, is the problem with theory, for once it escapes the hands of its creators, it often becomes orthodoxy and, its concepts reduced to jargon, whatever ability it once had to illuminate is subsequently dimmed.

Myth 4: The history of organization research is progress.

This is no doubt the most self-serving of all organization studies' myths. It suggests that we are now explaining more and more of organizational behavior in better and better ways. In crude form, the myth congratulates contemporary researchers for wresting theory away from the domination of airheads, vacuous armchair theorists, simpleminded empiricists, or, more commonly perhaps, not-so-reflective practitioners. Each new generation, it seems, pats itself on the back for bringing organization theory into the modern age. Whether this is accomplished by claims of hard reason, tested empirical observations, or the insightful application of interpretive canons, current work is always considered an improvement over past work. The writings of forebears are seen not as products of particular historical situations, but rather as documents created by those blinded by ill-formed theories, foolish ideologies, antiquated research methods, or personal biases untenable in light of our current knowledge.

This Whiggish view of our field is undermined in a variety of ways. Consider the head-swirling proliferation of theory and method groups within the field. Each argues that it has a special hold on truth that other theories or methods cannot approximate. Battle lines are drawn and the resulting conflict is real and defies negotiated settlements. The accelerating fragmentation and specialization of the field attests to the enduring substantive problems and epistemological dilemmas organization researchers have faced since the field began to emerge some 50 to 60 years ago.

The cardboard-cutout version of organizational science, popularized in textbooks, celebrates the steady progression of a field made possible by a stream of heroes who have come up with key discoveries or facts that, when sifted and refined, have led to explanatory and unifying theories. This view of science no longer carries much weight. This is not to say that good theories are not rich in scope and imagination, expansive but nonetheless held in check by clear visions of the nature of things. The "behavioral theory of the firm" developed by Cyert and March really did open things up for organization researchers, just as Goffman's ideas about "total institutions" enabled us to see old things in new ways. But one thing these theories are not are simple inductions from a set of observed facts gathered systematically from the social world. Nor do they simply replace, in one fell swoop, all that went before. The myth of progress is out of step with the often mundane realities of research and the importance of rhetoric when considering the widespread acceptance of some findings and theories of organizational behavior and the rejection of others.

* * * * *

To begin to appreciate the complicated ways our research reports are written, think first of just a few of the ways theory, data collecting, and reporting styles are mediated in organization studies. Obviously, the social and personal characteristics of the researcher are relevant to the work we do. Women, for example, have access to different parts of organizational life than men do, and may well be concerned with different aspects of this life than men. Older researchers typically carry out different kinds of studies than their younger colleagues. There are also the high and low politics of research. The power to describe and represent aspects of organizational life are not distributed evenly. We are usually one-up on those we study. Hence we have far more accounts of lower and middle managers than top-level ones. Grantsmanship enters the picture as well. Knowing how to shake a grant from some giving tree may be far more important to understanding particular research practices and topical choices than anything connected to the theory and method questions current in the field.

Research is institutionally mediated, too, since all studies are launched within particular traditions and disciplines. Thus, the current positions researchers take on the language and methods they use depends, in large measure, on where and with whom they were trained, where they currently work, what professional associations they belong to, and with whom they routinely interact.

Larger intellectual trends also influence organizational research. The number of deconstruction workers in our trade is on the rise just as the number of motivational experts and small group architects is declining. All research is undertaken at particular times and in particular places, thus historically and socially situating all work. Changes in organization research patterns arise from a number of sources, including: new faces in the field who find old theories, methods, or reporting styles unconvincing; shifts in training practices; crossovers brought in from other fields; and a host of other mediating factors having relatively little to do with the empirical or theoretical state of the art in organizational studies.

Add these features to the workings of narrative conventions carried by our research reports and there is good reason to believe that the game played by students of organizations is one of rhetoric, of convincing others that our narratives have merit. Given this situation, two responses have begun to be formulated. First, some organization researchers engage in a kind of table-thumping, back-to-the-basics hysteria. "Don't think so much about it, just do it." This is a back-to-the-future response that calls for the shoring up of the scientific aspects of our craft wherever possible and avoiding any and all self-reflection regarding the means of doing and representing our research. Subjectivity must not be allowed to raise its ugly head in our research reports; laws-and-causes are what we seek; hermenutics is for the Herman Nudnicks of the world. Go out and collect the facts, test your theories by subjecting them to these facts, and get on with the program. While there is a certain nobility in such a stance, it is so ignorant of current trends in the sociology and philosophy of science that it represents, at best, a sort of rear guard Luddite action that is destined to fail. Such views of science have already crumbled in the natural and physical sciences and their applicability to the social sciences has always been in doubt. Moreover, such a response plays directly into the hands of those who most vehemently reject the science of organizational studies. It is a little like locking the front door after the burglars have fled with the goods.

The second response to the increasing recognition of the role played by rhetoric in organization studies is an almost suicidal sharpening of the critical edge. Many critics of current organization research seem to be flying off into a sort of Alice in Wonderland world of theory and methods where the main idea appears to be not to do organizational studies anymore but to think and write about them. "Design the perfect study, but under no conditions try to (gasp) bring it off." There has always been something of a division of labor in the field between the theorists and the researchers, and this division seems to be growing of late. The former critique what the latter accomplish. Whether the critique is grounded in a High Science view of social research or an Interpretive Revisionist view, the writing is remarkably free of the contamination that might come from actually trying to construct a convincing account of ongoing organizational life based on first-hand information.

The critics can be shrill. They are the Howard Cosells of the research game-making judgments and pontificating on the action from the rarefied atmosphere of the Press Box high above the field of play. This is bothersome, since many students of organizations now view themselves primarily as theorists or methodologists though they've never done any original research themselves. Such self-proclaimed experts of organizational life are little more than Press Box strategists pushing such *bon mots* as: "Well, that sure looks like hegemony to me, why can't Andrew see it"; "Whoops, looks like Bob blew his study again by failing to get a proper control group"; "Tsk, tsk, old John must be slowing down these days, missed another clever agent of socialization sneaking by on the wing."

We are sometimes amused by these critiques. Occasionally, they are instructive. But, by and large, organizational researchers take their cues from players and coaches (typically ex-players) down on the playing field. As a matter of fact, the epistemological pronouncements from our organizational theorists or methodologists are as empty and derivative as the prescriptions issued by our own home-grown moral philosophers or philosophers of science. There are some very good primary sources in this domain, and creating a secondary literature comprised of third-rate advice and criticism will not substantively improve our craft (although it may provide employment for idle hands). The current discontent among many within organizational research circles will not be resolved quickly. There is, as I just noted, a rather wide gap separating the back-to-the-basics evangelists of the Scientific Creed and their high-flying critics, who can identify a "deep structure" at 50 paces and know an epistemological fallacy when they see one. The self-styled scientists of our trade want to do research without much thinking about it, while the all-seeing critics prefer to think about it without much doing it. Between these two poles must lie a workable middle ground.

What we need is the humble recognition that organization research is neither art (in the sense that it derives solely from the imagination) nor science (in the sense that it is a celebrated count-and-classify activity based primarily on the testing of simple analytic models). To promote such a recognition, I propose a 10-year moratorium on writings devoted strictly to method or theory, this period to be given over to the construction of simple narratives about organizational life. Such descriptive work might provide us with a deeper literary sensibility than we now possess and, hence, an appreciation of just what is involved in the telling of a good story, one that can attract a wide cross-section of readers. The writing of such narratives would shed considerable light on the value of current theories and methods without contributing to their further proliferation.

I propose further that we closely examine the works of those authors who have most influenced our field. I suspect we will discover that these authors have been more concerned with presenting a coherent point of view told with grace, wit, and felicity than with matters of theory and method. Such an investigation might also reveal that the acknowledged masters of our craft have uncanny abilities to formulate arguments and to develop them through apt analogies and striking metaphors. I suspect that their work contains little of the pale, bland, limp, passive, impersonal prose so beloved by journal editors. Moreover, I suspect that these works are minimally self-referencing but nonetheless full of personal touches and stylistic signatures that leave readers with little doubt as to who is narrating the tale.

These proposals suggest that instead of yet another course in descriptive or inferential statistics, the next generation of organizational researchers be offered a writing course to help them build some sensitivity to matters of rhetorical force. This would be a course in which a book is looked *at* rather than *through*. It is possible that a sort of ethnonarratology might develop in organization studies whereby researchers could learn various ways to deploy their tropes in the service of telling imaginatively about real people in real places at real times. My sense is that we need narrative conventions to guide our work, but we also need more of them than we currently use, together with an appreciation for the strengths and weaknesses of the conventions themselves.

To conclude by touching a final raw nerve, it is foolish and short-sighted to look for a proper way to write up organizational research that will generalize well across studies. Each research project presents its own set of representational difficulties, as do particular theoretical perspectives and methodological programs. It is folly to compel qualitative researchers to adopt a reporting style developed for quantitative studies. It is equally inappropriate to channel all quantitative studies into an experimental, hypotheses-testing format. We need to legitimize a diversity of reporting genres. To do so might make it possible to communicate beyond the limited, already-tenured audiences we now attract. Good research can be cast in many ways, and a single researcher can learn to write in many styles. To move in this direction we must pay a good deal more attention to our writing than we currently do. We assume a greater burden as authors than as conduits or scribes. But if we are to escape some of the representational predicaments of our field, there is no better place to begin than with the products of our research—the words we put on the page.

Notes

1. This paper is based on my participation in a seminar on Qualitative Research Methods for Management Information Systems held at the Harvard Business School on April 25-26, 1988. The seminar was sponsored by Professors James I. Cash, Jr., and Paul Lawrence. I am grateful to my co-participants for tolerating my fitful and ambiguous ramblings during the seminar. These ramblings are perhaps less ambiguous here, but they are no less fitful.

"Syllabus for Research Writing 101"

Anderson, C. *Style as Argument*. Carbondale: Southern Illinois University Press, 1987.

Becker, H. S. *Writing for Social Scientists*. Chicago: University of Chicago Press, 1986.

Clifford, J. *The Predicament of Culture*. Cambridge: Harvard University Press, 1988.

Clifford, J., and G. E. Marcus, eds. *Writing Culture*. Berkeley: University of Clifornia Press, 1986

D'Andrade, R. "Three Scientific World Views and the Covering Law Model." In D. W. Fiske and R. A. Shweder (eds.), *Metatheory in Social Science*. Chicago: University of Chicago Press, 1986.

Edmundson, R. *Rhetoric in Sociology*. London: Macmillan, 1984.

Fish, S. *Is There a Text in This Class?* Cambridge: Harvard University Press, 1980.

Geertz, C. *Writers and Authors*. Stanford: Stanford University Press, 1988.

________. "Blurred Genres." In C. Geertz, *Local Knowledge*. New York: Basic Books, 1983.

Gould, S. J. *Time's Arrow, Time's Cycle*. Cambridge: Harvard University Press, 1987.

Hollowell, J. *Fact and Fiction*. Chapel Hill: University of North Carolina Press, 1977.

Lieberson, S. *Making it Count*. Berkeley: University of California Press, 1985.

McClosky, D. N. *The Rhetoric of Economics*. Madison: University of Wisconsin Press, 1985.

Morgan, G. *Images of Organizations*. Newbury Park, California: Sage, 1985.

Mulkay, M. M. *The Word and the World*. London: Allen and Unwin, 1985.

Stocking, G. W., ed. *Observers Observed*. Madison: University of Wisconsin Press, 1983.

Van Maanen, J. *Tales of the Field*. Chicago: University of Chicago Press, 1988.

Weber, R. *The Literature of Fact*. Athens: Ohio University Press, 1980.