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BARBARA
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Writing Management

Organization Theory
as a Literary Genre

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Management and Organization: A Field, a Theory, a Practice, and a Genre

Each practice has its own theory: thus management practice has its theory, usually of a normative character. An academic practice differs from others in that it has a theory of its own practice, and a theory of another practice, in this case, management. A theory of the academic practice, of which this book is an example, also has a normative character, although in this text the meaning of 'norm' emphasizes 'the usual' rather than 'the ideal'. In other words, I am not concerned with the a priori criteria of 'good' or 'bad' writing on management, although I assume that good writing is desirable to the readers as much as to the writers. Such criteria are, however, constantly constructed in specific times and places, and could therefore only be described a posteriori, together with a reflection on their emergence.

While there exist many texts occupied with the creation of management and organization theory in terms of methodology (a seminal work in this respect is Burrell and Morgan 1979), not much is said about writing management and organization theory (with recent exceptions being Golden-Biddle and Locke 1997, and a special issue of *Studies in Cultures, Organizations and Societies*, 5/1 (1999), edited by Steve Linstead). In this respect, management as a subdiscipline of social studies is lagging far behind economics (McCloskey 1986), sociology (Brown 1987; Van Maanen 1988; Agger 1990; Capetti 1995) or anthropology (Clifford and Marcus 1986; Geertz 1988). This is most likely due to the fact that management, or business administration, as a relatively new university discipline, still feels allegiance to the eighteenth-century ideals of (natural) science, which assumed writing to be the domain of literature and not of science (albeit producing copious writings on the subject):

Writing is an unfortunate necessity; what is really wanted is to show, to demonstrate, to point out, to exhibit, to make one's interlocutor stand at gaze before the world . . . In a mature science, the words in which the investigator 'writes up' his results should be as few and as transparent as possible. (Rorty 1982: 94)

The realization that writing is the organization researchers' main activity seems to be growing, together with reflection upon how they write and what

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textual devices they use, or could be using, in writing. But this reflection is neither easy nor obvious. Management, as a young academic discipline, is not yet prone to disciplinary reflection. There are even some difficulties as to what discipline it is to begin with.

A FEW WORDS ON SEMANTIC SEDIMENTS

The safest route for somebody undertaking a disciplinary reflection is probably via direct experience. Thus let me begin with what I know best—that is, my own job. I happen to be a professor in *företagsekonomi*, which should translate into English as ‘Business Economics’, but it does not. It has usually been translated as ‘Business Administration’ but recently, as a result of some inconsistencies between this umbrella-like name and its subdisciplines, has increasingly been translated as ‘Management’ (for more detailed analysis, see Engwall 1992).

There are at least two reasons for these mistranslations. A historical one has to do with a wish to indicate the end of German influence, traditionally very strong in the Swedish system of higher education, and the beginning of the US influence. Another, emerging reason had to do with the fact that, with the exception of business finance, most of the *företagsekonomiska* subdisciplines use organization theory and not economic theory as their theoretical basis.

As to subdisciplines, they may vary from one Swedish university to another, but the standard offer would be accounting, marketing, public administration (thus trouble with ‘business administration’ as the main subject; recently it has been called ‘public management’), international business, and . . . organization. The latter had replaced the earlier ‘administration’, reflecting more general changes in the field (the 1960s shift from ‘administration theory’ to ‘organization theory’), but creating havoc at the juncture between theory and practice. The original ‘administration’ dealt basically with personnel problems. As the present ‘organization’ deals mostly with theory, ‘personnel’ has left our departments and found refuge in Education, Sociology, or Applied Psychology Departments. Hence it is quite often that a specialist in organization theory holds the general chair and his or her speciality is not well represented at the level of applications.

This is a taste of Swedish academia: had I wanted to extend this disciplinary map to the USA or the UK, not to mention France and Germany, innumerable intricacies would emerge. I would have to account for Organizational Behaviour and Organization Development, HRM and Industrial Relations, Financial Accounting and Management Accounting, and so on and so forth.

While such a historico-geographical approach is undoubtedly needed and valuable, I will choose another route. The protagonist of this chapter is

management and organization theory (treated here as one subject¹) and its applications. The fact that such a discipline does not nominally exist, and appears in the academic surroundings under a variety of institutional guises, is nothing more than an indication of its evolution and transformation.

IS MANAGEMENT THEORY A PRACTICAL SUBJECT?

When, around the beginning of the twentieth century, our forefathers (plus Mary Follet) began forming the subject that later acquired a variety of names, they did so on the promise to solve any problems companies and administrative organizations might possibly have. Later, this role of 'company doctor' was further developed, especially by the Tavistock Institute, which was given large sums of money by the British government to help all companies facing trouble in post-war Great Britain (see e.g. Jaques 1951). At the same time, however, this kind of knowledge was becoming a strictly academic subject, with Ph.Ds and professors, refereed journals and international conferences.

Where are we now? Is ours a practical subject, which produces practitioners and improves practice? Or is it an academic discipline eager to remain in close contact with practice, with the purpose not of dictating the order of things, but of *reflecting and provoking* via basic research and theory? After all, is it not a strange idea that researchers from outside an organization can tell practitioners how to run their companies and how to be better managers; that these 'experts' are expected to reform and reorganize living organizations? Common sense would suggest that it is so. After all, if researchers are so good at organizational action, one might well ask why they are wasting their talents on academia.² And yet this assumption has persisted since the birth of this discipline. Researchers arrive, and, like car mechanics or physicians, they examine the 'body' of the organization, make a diagnosis, and prescribe a 'cure'. The discipline had, after all, made an oath to its practical utility. Does it still hold?

When Plato laid the foundations for Western philosophy (which have only recently been deconstructed), he promised the Athenian politicians to solve all problems that might arise: from guidelines as to how to conquer Sparta to methods for raising one's sons (Plato cannot, I am sorry to say, be accused of having been a feminist). When Durkheim laid the foundations for modern sociology, he promised to solve any problem French society might have: from German neighbors to poverty and crime. Both disciplines have ended up at an advanced theoretical stage, while the practical problems have multiplied, if anything. Should we conclude that all those promises were hypocritical, made with the intention of justifying oneself to society on false premisses?

¹ There exist organization studies that do not concern themselves with management, but I hope that most of what I have to say can be applied to these as well.

² In the same vein McCloskey (1990a) asks economists, 'If you are so smart, why aren't you rich?'

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Indeed they were a means of justification. Those promises were, however, born not out of hypocrisy but out of enthusiasm and naïvety, so typical of the coming into existence of new ways of thinking. A similar contemporary development can be quoted here: that of environmental science, this new interdisciplinary subject asserting itself by promises of salvation. Yet, every acute observer of miscellaneous modernist projects is able to predict straightaway that this promise will not be kept. It is not that disaster is inevitable, but that, in order to stop it, a coordinated effort of a political, economic, and social nature is required. The task of environmental research is perhaps not primarily to solve problems but to bring them to light (Wolff 1998).

That which was once enthusiasm and exalted optimism, with time turns into frustration if things go wrong, and wisdom if things go right. Things have gone rather well for business and management sciences, which means that we are now in the position to afford some reflection upon *what* we are doing and *why*. If we are a practical subject devoid of intellectual ambition, then it is time to stop wasting society's funds: back to schools of commerce and to two-month correspondence courses in accounting and business techniques. If we are an academic discipline, however, we must ask ourselves what could be our contribution to the general debate concerning the desired shape of our society in the twenty-first century.

Contrary to appearances, we have much to contribute. However often we weep over the lost civil society, it is clear that such a society can exist only as a parallel to organized society. Without understanding much of it, we have become organized through and through (or, as Perrow (1991) calls it, we have become 'a society of organizations') and even those who are against organizations as such organize to protest against them. This is where the very duties and possibilities of organization science lie: in a theory of organizational society, with economics and politics seen as the two main forces of construction of that which constitutes society. There are no individual consumers or producers: they all watch the same television and read the same newspaper (in Sweden, in two main versions). The last of the individual producers and consumers was a quaint German living in Lapland, living off nature and help (returned) from neighbors. The Swedish immigration authorities have done all that was in their power to get rid of him.

What legitimacy do we have, what kind of theory can we offer, what status will the knowledge so produced have, and of what practical use might it be? Let us have a look at each of those issues.

THE EMERGENCE OF BUSINESS ADMINISTRATION

In 1935, in the USA, Thurman Arnold, a Yale lawyer who served as the Chairman of the Antitrust Commission, wrote an amazing book, called *The Symbols of Government* (where 'government' stands for 'governance' and not for the state

authority), presenting an unusual view of reality, which, surprisingly enough, can easily be transferred from the USA in the time of the Depression to, let us say, Sweden of the 1980s (a feat accomplished by Brunsson 1985; 1989).³ Arnold depicted society as composed of the subsystem of norms, values, and ideologies (including religion and science) where ideals went hand in hand with formal logic, and then the subsystem of practice—meaning mostly business practice—messy but effective. Most of the time in opposition, they were nevertheless connected to each other. The official version of this connection was that the world of norms dictated the rules to the world of practice. In Arnold's version, however, it was rather more likely that the world of norms legitimized practice as it went along, cleaning out some of its indecencies as required. Of course, such a liaison was neither simple nor unproblematic. There were whole gray zones of problems, and business administration, he claimed, just filled one proudly:

The ways of thinking of the business world have, of late years, also demanded scholarly recognition. Business schools have become an accepted adjunct to great universities. Again, that way of thinking which demanded logical arrangement and preconceived fundamental principles, compelled the professors of the business habits of the time to call their subject a 'science.' The 'science of business administration' has a congenial sound to the scholarly ear, and indicates that so far as possible it will avoid creating a mere 'trade school.' More freely translated it means that, in deference to scholarly thought, it will avoid studying business as it is, to study it as it ought to be, since the close examination of living institution is the enemy of all logical classification. Yet the business-school professors were so close to life that the best ones refused to become sufficiently theoretical to avoid academic suspicion that they were tradesmen rather than scholars. (Arnold, 1935: 89–90).

There are many interesting threads to pick up in this opinionated utterance. It is of relevance here, because it contains all the problems that will be typical of business and management sciences during the first half of the twentieth century: being 'adjunct' to universities, even if accepted and sometimes (in times of prosperity) envied; being exposed to the 'scholarly' expectations that are never properly met; and having some members of the profession who rebel and refuse to be 'scholarly', choosing to be faithful to their 'practical' vocation. I believe that many of these problems can now be solved, when the notion of 'science' and 'theory' is redefined, and I will return to it in later sections. For the time being, though, let us look at Arnold's utterance as related to its historical context.

It is hard to say whom Arnold meant when he spoke about those who adapt and those who rebel against scholarly demands, but it is appropriate and instructive to focus attention on Chester Barnard as a symptomatic personage

³ This section has no ambitions of presenting a complete historical narrative, which would have to start with Babbage in England, continue with Taylor in USA, Fayol in France, and Mayo in USA again. It takes up a specific aspect—legitimation—of organization theory within the discipline of business administration. For historical analyses of US management science, see O'Connor (1996) and Shenhav (1999).

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of those times. As the practitioner he was, he decided to share his experience with all who needed it. To this end, he wrote a book *The Functions of the Executive* (1938). This classic attracted academic attention again fifty years later, when Oliver Williamson was entrusted with organizing a series of lectures at Stanford University in honor of Barnard, which resulted in a book of essays edited by him (Williamson 1988).⁴ In this collection, various authors point out two interesting characteristics of Barnard's book. To begin with, his was no Iaccoca story: Barnard put his experience into a highly abstract form, typical of the then legitimate social theory, so that even the most enlightened minds of our times admit they had trouble dealing with his dense and formal text. The second interesting observation is that this classic of organization science represents all that business and management sciences are *not* supposed to be: not only is this an abstract theory, but it is also arrived at through *speculation*, and not through empirical study. There is no doubt that Barnard wrote on the basis of his experience, but so did all the speculative theorists, from Plato through Thomas Aquinas to Immanuel Kant.

This was not, however, the meaning of theory as understood in the 1940s and the 1950s.

'THERE IS NOTHING SO PRACTICAL'

'There is nothing so practical as a good theory,' said Kurt Lewin, on emigrating to the USA to do research on democratic leadership (Lewin *et al.* 1939), and on the force of persuasion and on group dynamics in general (Lewin 1953). Lloyd Sandelands (1990) decided to take a closer look at that famous statement to see what implications it might have for organization science. His reasoning turned historical: there were at least three different interpretations of the famous dictum and they seemed to mirror a transformed way of thinking.

Apparently, Lewin himself wanted to build a bridge between theory and practice using experimentation. To him, theory was an idea that had to be tested: using practice, one would find out if the idea held or, if not, what was wrong with it. Even if this may sound convincing and rather practical, it is impossible, claims Sandelands. Since theories are formulated in a completely different language from action plans, they must first be translated into such plans. This could be done by researchers (who would then behave as practitioners) or by the practitioners themselves. But it is not theory that they would be using; they would merely be inspired by theory in their construction of action plans. In the same way, one may be inspired by nature or by a work of art. The aim of theory is reflection; the aim of action plans is action.

The developments in the social sciences brought more and more complications to Lewin's original statement. To begin with, it has been pointed out that

⁴ For a more deconstructive reading of Barnard's book, see O'Connor (1996).

experiments rarely, if ever, verify or falsify a theory. Even in natural sciences replications bring more problems than solutions (Latour 1988a). To continue, there are serious doubts about the transferability of laboratory experiment results to any other kind of social reality. It is not that the laboratory is 'unreal' or that reality is 'out there'. It is simply that laboratory reality is a reality *per se* that may or may not have traits in common with other realities. These doubts were deepened by the critique of the correspondence theory of truth (Rorty 1980) and, as a consequence, the idea of 'representation' (one situation standing for another) came into question.

But testing was not the only way to join theory and practice. Another way was, as it were, to 'theorize' practice. After all, plans of action are based on certain—tacit or verbalized—action theories. And, since practitioners construct their own theories of action, researchers should be able to come up with competing ('better') theories, as well as with meta-theories—that is, theories about the practitioners' action theories (Argyris and Schön 1974). Sandelands (1990), however, pointed out that this leads to the creation of an additional problem rather than the solution of the original one. When it comes to practice, people are not conscious of the action theories they are applying. Everyday theories are much more incomplete and intuitive of character than are academic ones. If one asks successful entrepreneurs how they go about things, the answer is often a set of platitudes that are at best trivial and at worst quite evidently false. Do they not know what they are doing? Not exactly, nor do they have to. One does not need a diploma from a school of engineering to be able to drive a car.⁵ What is more, it is highly improbable that researchers should be able to come up with a better theory of action than do practitioners, unless they become practitioners themselves.

Instead of simplifying our dilemma, Argyris and Schön complicated it even further. Yes, practitioners do have action theories, which, albeit different in content, are quite similar to scientific theories in form, which means that they are made of different stuff (or, to put it more elegantly, belong to another sphere of discourse) from practical knowledge. So, now we have to interpret first actions, then action theories, then the relationship between the two, then scientific theories, and the relationship between the two types of theory and practice.

Does it mean that research and researchers have nothing else to offer to the practitioner but the typical academic hair-splitting? They have much to offer, but within their special area of competence and not in competition with the practitioners themselves. The social sciences are a system of institutionalized reflection, whereas business and public organizations represent institutionalized action. It is obvious that the two are different and separate, but it is equally obvious that they have much to offer one another. Action is the subject of the

⁵ The irony of events would have Milton Friedman use the same argument to a dramatically different purpose: he says that people are able to use the results of theoretical (economic) reasoning without understanding it (Friedman 1953).

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researchers' reflection; reflection offers to action the possibility of change and renewal. The two together—starting with the notions of 'action research' and 'the reflective practitioner' (Schön 1983)—have ultimately become institutionalized in the consulting profession, which mediates between the two worlds but which also strives for a special competence of its own, based on just this unusual combination of action and reflection.

But let us proceed to Sandelands's third interpretation, which treats theory as a kind of practice. The product of theoretical thinking is tropes, or rhetorical figures (see Morgan 1986), which, though still not connected to practice, can provide practice with new inspiration and evoke interesting associations, just as art does. Tropes such as 'garbage can', 'bounded rationality', or 'muddling through' entered the practitioners' vocabulary with no need for formal schooling in works that produced them.

This is, in a sense, an opposite solution to the previous one. While action theory theorizes practice, this approach 'practices' theory. Consequently, research and education within business administration are a practice in its own right, with its own rules and its own action theories. Its product is *theory and reflection* (or, in the case of students, the habit of reflection). As a consequence of this, the relationship between organizing processes in a company and organization science in academia can also be redefined. The contact between the two has to be close precisely because it becomes increasingly evident that ours is a discipline that is inspirational. An academic discipline, when isolated from practice, becomes dull and self-centered. Practice without reflection becomes routine and repetitive. As theoreticians, we should be telling practitioners about what they could never come to think of themselves, and not about what they know already and better.

All these solutions are of great insight and value, and all are applied to a great many situations. Still, the chasm between theory and practice gapes as wide as ever, says Sandelands (1990), causing quite a few legitimacy problems (see Astley and Zammuto 1992). At this juncture two observations must be made. One is that, when speaking about theory, Sandelands had in mind a specific type of theory (which I shall call a 'logico-scientific' one); the other is that, while speaking about practice, he concentrated on tacit knowledge, no doubt of central importance to practice. There are, however, other types of theories and other types of practical knowledge.

PROBLEMS AND SOLUTIONS REDEFINED

The recent way of thinking about theory and practice and the relationship between the two tends to redefine the problem. First of all, the critique of the correspondence theory of truth (Rorty 1980) brought us the understanding that theory, belonging to the domain of language, cannot 'represent' reality in any iconic sense. Events do not unfold in a logical sequence; only sentences do.

Thus it has become clear that field research is not an 'imitation' of practice and theory is not its 'reflection'. Accordingly, Brunsson (1982) postulated that the main role of business and management science as a discipline is to furnish a language of description (I would say interpretation) and reflection to practitioners and theoreticians alike. The concepts of 'science', 'knowledge', and 'theory' are changing, and making room for new meanings.

This change was accelerated by the 'postmodern revolution'. The idea that scientists should be out improving reality was all a part of the Modern Project, claimed Lyotard (1979/1987). This project, whose roots lie far back in time but which came to fruition in the twentieth century, was characterized by an extreme arrogance masked as a program of emancipation. It was arrogant towards nature, which was to be controlled and exploited by humans, and it was arrogant towards those doomed to the need of emancipation, such as developing nations, workers, women, and children (Mitchell 1987). A logical part of this view was the superordinate value assigned to science, which replaced religion, thus endowing scientists with a privileged status.

What research has to offer now is not emancipation, but freedom from old versions of social reality (Rorty 1992a). Rorty agrees with Lyotard that we should give up the ambition to emancipate other people, because such a program always conceals an arrogant assumption of superiority and 'knowing better'. But this does not mean that we cannot try to convince other people that many versions of the world are possible, and, rather than criticizing their versions, we can try to persuade them of the attractiveness of our own.

In the specific case of organization theory, the task of the researcher, as I see it, is to free practitioners from the 'iron cage'—from the trap that the world they have constructed for themselves has become for them. By convincing them that it does not exist 'out there' objectively and immutably, but that it is constructed by people in a joint effort, the researcher can also persuade them that other constructions are possible. But the decision to change and the choice of alternatives are in the hands of the practitioners (and always have been, modern ambitions notwithstanding). It is perfectly legitimate for them to say: 'Now we are convinced that we are the constructors of the world we live in, but we still like the way it is, thank you.' But by showing practitioners some of the unexpected aspects of what is regarded as self-evident in organizations, researchers provide an opportunity for shared reflection, for thoughts that might lead either to change or to confirmation of the status quo.

How can one construct a theory that can accommodate such requirements? One possible starting point is the refutation—by the new pragmatist philosophy—of the metaphor of science as 'the mirror of the mind', where the mind itself was supposed to be a receptor of the 'true world' (Rorty 1980).

The ground for this kind of refutation had been prepared by the many variations of constructivism—from Schützian phenomenology, carried on by Berger and Luckmann (1966) and Holzner (1968) to Goodman's (1978) 'irrealist' claim that we are all world-makers. The realist ontology has turned into a belief, and a very practical one. Levine (1993) recalls a routine conversation

with a 'true' scientist who invariably asked him at parties whether a deconstructionist (or a constructionist, for that matter) would die falling off a high tower. To which Levine supposedly (and routinely) responded that he would not know before seeing the event, but that he expected any deconstructionist of his acquaintance to steer safely away from the edges of high towers. It is practical to *believe* in the world of causes 'out there'; it works most of the time. This does not equal saying that there are ways of describing this world that represent it 'as it is': 'We need to make a distinction between the claim that the world is out there and the claim that truth is out there' (Rorty 1989: 4). As a consequence, the sophisticated notion of *episteme* (knowledge) becomes equivalent to the primitive *doxa* (opinion) (Rorty 1991a). What we know is what we believe to know. Researchers, like all other people, have opinions on all kinds of matters (indeed, the 'self' can be seen as a constantly reweaving web of beliefs) and test them in action; as a result, what works matters usually more than what is 'true' (besides, for whom is it true? for how long?). It is—sometimes—the *object of those opinions* and—always—the *conventional* (for given time and space) *way of expressing it that form differences between 'science' and 'everyday knowledge' or 'science' and 'literature'*.

Science, in this view, is a conversation of humankind, and its logic of enquiry is rhetorical (Oakeshott 1959/1991; McCloskey 1986). Much of this conversation takes place in written form, thus making it legitimate to apply various kinds of literary analysis to it—for instance, genre analysis.

IS MANAGEMENT AND ORGANIZATION THEORY A GENRE?

A genre is usually conceived as a system of action that became institutionalized and is recognizable by repetition; its meaning stems from its place within symbolic systems making up literature and culture, acquiring specificity by difference from other genres (Bruss 1976: 5). Is organization theory a genre in this sense?

Following Sandelands's third interpretation of the connection between theory and practice, I propose to treat theory production as a kind of practice.⁶ This kind of practice produces insights that can serve as a source of inspiration and interesting associations to another practice. Consequently, disciplines such as business administration, management science, etc., should be eager to remain in close contact with management practice, with the purpose not of dictating the norms, but of reflecting and provoking. Thus, the art of writing (and of speaking—the persuasive skills in general) become extremely important, and their critical development a crucial task in its own right.

It is this understanding of organization science—as a practice, that is, as a

⁶ I hasten to add that these operations are not approved of by Lance Sandelands himself. It is important to differentiate between the author and the text. It is the latter that I engage here.

system of action that became institutionalized and recognizable—that makes the notion of organization theory analogous to that of a literary genre. After all, all organization researchers do is read (listen) and write (speak). So in that sense Astley and Zammuto (1992) were correct: organization researchers are involved in a linguistic practice. But there is more to it than ‘just talking’: it is important to point out that texts are actions (strictly speaking, material traces of such, but they both result from action and provoke further action), and actions are texts, in the sense that they must be legible to qualify as actions at all, and not, let us say, movements or behaviors.⁷ ‘Action’ and ‘text’ are good metaphors of each other, but even more than that (Ricoeur 1981: 197–221). Actions, especially institutionalized actions, produce texts; texts not only ‘fix’ other actions—their production and interpretation assume actions.

Actions, in order to be legible, must relate to some context accessible to those who attempt to make sense of them (Harré and Secord, 1972) and such a relation can be seen as a constraint. Just what such constraints and the ways of dealing with them are is well rendered in a description of a literary genre:

All reading (or writing) involves us in choice: we choose to pursue a style or a subject matter, to struggle with or against a design. We also choose, as passive as it all may seem, to take part in an interaction, and it is here that generic labels have their use. The genre does not tell us the style or the construction of a text as much as how we should expect to ‘take’ that style or mode of construction—what force it should have for us. And this force is derived from a kind of action that text is taken to be. (Bruss 1976: 4)

The term ‘choice’ should not mislead the reader into assuming a rational choice: what Bruss calls ‘a passive choice’ can also be understood as following the ‘logic of appropriateness’, as March and Olsen (1989) call the usual logic of action that aims not at the choice of an optimal alternative but at an action that will be recognized and accepted by an audience residing within the same institutional set-up.⁸ A student of mine described his methodological choices with the help of a garbage-can model: a given method results from a meeting between a certain type of advisers, doctoral students, accessible companies, and fashionable methods at the same time and place (Strannegård 1998).

In the sense of evoking expectations by using a label, organization theory is undoubtedly a genre; perhaps, in fact, more a genre than a discipline. A useful reflection could then focus on what are the constraints and possibilities of this genre, how does it develop historically, and what are its actual and potential connections to other genres.

Genre analysis is often used as a classificatory device (for the most famous example, see Northrop Frye, 1957/1990). Although a system of categories as such is relatively easy to construct and has a strong heuristic power, its application to concrete works is more problematic. After all, a genre is but a space within which one can position various works, and it would be their vicinity or

⁷ It should be clear by now, that in this notion of action no agency is implicated: actions are events to which intentions have been ascribed.

⁸ On the difference between the logic of action and the logic of decision, see Brunsson (1985).

distance to other works that would establish their genre. Genre analysis in literature places most works between genres; disagreement thus remains as to where the genre borders should run and whether it makes sense to draw them at all (Lejeune 1989). The best-known attempt at genre analysis within organization theory, Burrell and Morgan's (1979) classification of main paradigms, revealed its heuristic power in provoking massive protests and reclassifications. One can thus envisage an alternative to creating an interpretative space which will be able to contain and relate to each of many other approaches without ascribing strict positions to them. McCloskey (1986: p. xix) suggested, for instance, that literary criticism can offer economics a model for self-understanding: 'Literary criticism does not merely pass judgments of good or bad; in its more recent forms the question seems hardly to arise. Chiefly *it is concerned with making readers see how poets and novelists accomplish their results*' (emphasis added).

Such reflection, or self-reflection, makes a genre more distinct and more elaborated. The analysis of a genre is one of its main constitutive forces. Social scientists busy themselves constructing the institutions they describe. Describing what they do, organization researchers can increase the legitimacy of their own genre.

Not everybody is of that opinion. There are voices saying that problematizing what one does is not a good way to institutionalize it, that attracting attention to the process, inevitably exposing its messiness and lack of a priori criteria, is the last thing a discipline in need of legitimation wants (Pfeffer 1993). This might be true in the case of disciplines that are just beginning, which are vulnerable to any doubt, but different cycles in life require different legitimation tactics. The most established disciplines, such as philosophy, mathematics, or theoretical physics, like nothing more than a public soul-searching in order to renew and relegitimate themselves. This is helped by the fact that the very attempt to define a genre, as Lejeune (1989) pointed out, is paradoxical: it can only be done by exploring the gray zones and borderline cases. Genres blur as soon as you look at them at close range.

Neither paradoxicality nor the presence of conflict needs to debilitate a field; on the contrary, they enhance its controlling power. Institutions emerge and renew themselves 'by generating just the right kind of tension or even conflict, creative rather than destructive' (MacIntyre 1981/1990: 171). Delineating borders facilitates transgressions, stabilizing gives a basis for experimentation, routinizing permits improvisation. As language renews itself via paradox (Lyotard 1979/1987), so social practices renew themselves via tensions and contradictions. The approach suggested here can thus be seen as a loan from literary theory that will problematize organization theory, thus enabling it to reinvigorate itself.

Until now, I have argued for loans from literary theory in pragmatic terms: the dialogue between management theory and practice can thus be improved. But what legitimacy does such a move have? Does it not shake the position of scientific theory as distinct and separate from literature and journalism? In the

two chapters that follow I attempt to build such a legitimacy base by showing that narratives abound in scientific theory. I begin by contrasting narrative and logico-scientific knowledge (Chapter 2), and then apply the distinction to exemplars of organization theory (Chapter 3).

A reader convinced by my reasoning may then proceed to successive chapters that illustrate possible modes of such rapprochement. Chapter 4 shows that such a vicinity is neither new nor unknown in the annals of social sciences. Chapter 5 applies the notion of plot to the activity of structuring field reports. Chapter 6 suggests taking detective stories, a highly plotted genre, as a model for organization studies. Chapter 7 returns to the concerns of managerial practice, and their place in management theory.