In the field of organization and management studies – as in any other area of inquiry – knowledge springs from reflection on experience. Whenever we are confronted by a phenomenon unknown to us, we are induced to formulate hypotheses on its nature and on its dynamics, to break it down into elements or factors and conjecture relationships among them. The process is circular: the mind projects hypotheses on the phenomenon investigated; experience tests the hypotheses formulated, thus enabling verification of whether their corollaries are confirmed by the ‘facts’ as perceived by the inquiring subject. The mind then once again takes the lead: the hypotheses previously formulated are corrected, and again subjected to the test of experience, in a process of interaction which continues until we feel that the phenomenon investigated has become sufficiently familiar for us to say that we know it and – if we must – confront it.

This process characterizes the generation of whatever form of knowledge: both commonsense knowledge (which each of us strives to obtain when dealing with everyday problems), and the ‘expert’ knowledge produced by those to whom society assigns the specific task of producing knowledge about particular categories of phenomena. What differ between them are the forms of thought and the modes of experience. In the production of commonsense knowledge, intuitions, instinctive reactions, and fragments of conscious hypotheses accumulate in the memory and in the unconscious, constituting implicit theories which we are not concerned to formalize and thus structure systematically. In the production of ‘expert’ knowledge, by contrast, every piece of the puzzle is
usually connected with the others until we produce a coherent set of hypotheses on regularities (correspondences, coincidences, symmetries, not only and not necessarily cause and effect relationships). These regularities are made explicit and formalized so that they can be communicated to others and also used by those others. In the production of commonsense knowledge, experience is accidental and occasional. In the production of expert knowledge, dealing with empirical ‘reality’ is programmed, and sometimes, as in the case of laboratory research, the experience is deliberately ‘produced’. But in both cases the nature of the process – the interaction between thought and experience – is the same.

And yet most theorists – especially social theorists – take it for granted that theorizing is their exclusive preserve, forgetting that everyone everywhere constantly constructs theories: that is, hypotheses about the real world or hypotheses about possible worlds. Paraphrasing Burke, we may say that humans build their theories, nervously loquacious, on the edge of an abyss. We all need theories so that we can act or justify what we have done. Theories are the carapace constructed by individuals and groups to protect themselves against their ignorance of the world; and theories give sense, prospectively or retrospectively, to social action. Commonsense theories and ‘expert’ theories ceaselessly confront each other; they challenge consolidated practices, and they are brought into question by new practices incorporating new theories.

The more fluid and continuous the interaction between reflection and experience, the more intuitively constructed models are tested by the facts; the more experience becomes a consciously-perceived source of new theories, and the greater the progress of knowledge in a given field of inquiry. The thesis that I intend to argue here is that in organization and management studies, especially in recent decades, the interaction between reflection and experience, theory and practice, knowledge and action has been defective and intermittent, if there has been any at all. This has significantly hampered – in my opinion - the progress
of expert knowledge. It is, of course, necessary to define what is meant by the ‘progress’ or ‘development of knowledge, at least in this field of studies.

Such progress is certainly not measured by the rapidity with which new theories replace previous ones; nor by the fact that theories arise which furnish more exhaustive and logically consistent explanations of organizational phenomena and managerial action: still plausible theories are replaced by others which seem ‘definitive’ only until the advent of further ones. Nor can it be maintained that there is progress when new theoretical propositions are empirically tested, because often the theories supplanted were equally so tested. On what basis, therefore, can we state that a theory is ‘good’, and that new theories are ‘better’ than the existing ones?

The fact of the matter is that the criteria used to validate theories cannot be ‘internal’ to the world of theories. I am very grateful to Stewart Clegg for having drawn my attention to the concept of ‘powerful theory’ by inviting me to a session of the Academy of Management Meeting held in San Diego in 1998, and during which we discussed – with Peter Frost, Karl Weick, Kathleen Eisenhardt, Nicole Biggart and John Jermier – what it is that makes a theory ‘powerful’. For those who share a relational conception of power (Dahl, 1957), it is not an attribute of an isolated entity, for it always implies the relationship between someone or something with someone or something else. Arguing that the ‘something else’ of theory is practice, I defined as ‘powerful’ a theory which empowers action, so that its quality or ‘goodness’ is evaluated on the basis of its capacity to affect practice. How and in what sense does theory empower actors? My idea is that “a theory empowers organizational actors when it spurs their imagination, points out new opportunities and ends, unveils new paths and new means to ends, and increases their freedom of action and their will to act” (Gagliardi, 1999, p.144). It seems to me that the concept of empowerment is particularly appropriate to mirror this inspiring rather than prescribing quality of theory.
I shall briefly analyse how actors come into contact with a theory, and the way in which the theory ‘acts’ on them. The actor experiences the theory as a ‘cultural artifact’ materialized in a discourse, in a text or some other medium: s/he reads it in a book; it is described to him/her by a consultant; s/he learns it on a management course. The theory is usually experienced in its entirety: it is analytically deciphered, but it is also morally evaluated, because the ‘hypothesis of order’ which it contains may be more or less desirable according to the recipient’s values system (or, in general, concerns). There is then a third aspect of theories which is usually neglected, and on which I have insisted in particular: the aesthetic dimension. A theory may be more or less attractive because of its formal properties: the style of writing, the images that accompany the text, the metaphors used, the rhetorical skills of the recounter can help make a theory alluring. The ontological, ethical and aesthetic aspects of a theory influence each other – just as we tend regard as true what we desire, so we find beautiful what we believe to be true – and they determine the way in which a theory is perceived and judged. An emotionally involving and aesthetically seductive view may therefore count more – in terms of a theory’s practical relevance – than the logical coherence of its propositions. Barley and Kunda (1992) convincingly argued that the history of American managerial thought – from 1870 to the end of the 1900s – exhibited a cyclical alternation between ‘devotion’ and ‘design’: that is, between rhetorics emphasising normative ideologies of control (e.g. human relations) and rational theories of management (e.g. scientific management), a sequence that parallels Etzioni’s (1961) taxonomy of compliance and control.

If this is true, or at least reasonable, it helps explain the scant impact that theories formulated according to the positivist orthodoxy have exerted – or
could exert - on managerial practice. Such orthodoxy demands of scientists that their reports be the exclusive fruit of detached observation, written in rigorously analytical language, purged of every stylistic device, and devoid of the charm of the imagination. It thus endorses Hume’s imperious recommendation: “If we take in our hand any volume... let us ask, Does it contain any abstract reasoning concerning quantity or number? No. ... Commit it then to the flames!”¹.

The scant ability of theories developed by academics to influence the practice of management may be also due, in general, to the fact that the target audience of academics consists more in their colleagues than in managers. In effect, the empowering quality of management and organization theories can be appraised in relation to two different audiences and two different kinds of practices: first, other theorists who can be empowered in the practice of their own theory work – because theorizing is itself a form of practice – and, second, the practitioners who have to manage organization and/or live within them. I believe that organization theorists tend to be more interested in the influence of their theories on the mode of theorizing of their colleagues – on their styles of thinking and writing – than in the possible impact of their theories on styles of management and patterns of organizational behaviour. I remember an interesting debate on the quality and importance of organizational theorizing that took place some years ago between Jeffrey Pfeffer (1993) and John Van Maanen (1995a, 1995b). Pfeffer complained about the lack of ‘paradigmatic purity’ and unity in the field. In response, Van Maanen cited as an exemplary model the ambiguous and open conversational style of Karl Weick. Nevertheless, both of them had the same thing at heart, namely the success of theories within the academic community: Pfeffer feared that the economists would invade the field of organization studies;  

¹ Donaldson used this quotation from Hume as the epigraph of his book For Positivist Organization Theory: Proving the Hard Core (Donaldson, 1996).
Van Maanen measured Weick’s success in terms of citations in academic journals and of the presence of his books on reading lists for Ph.D programs. It is clear that if academics show limited interest in the social and political importance of their work, the distance between management science and managerial practice is bound to increase.

The situation is paradoxical from this point of view. In fact, in all the advanced countries, management is an area of research which receives conspicuous resources. I believe that these investments are made for three main reasons: firstly, there is widespread intellectual curiosity in a phenomenon which by its nature stands at the crossroads of disparate disciplines and is an extraordinary proving ground for interdisciplinary work; secondly, there is a strong collective interest in critical reflection on a phenomenon that clearly conditions the quality of social life; thirdly – and perhaps above all – those who invest (governments or firms) expect that the knowledge produced will be applicable to concrete action, in the sense of both improving the quality of organizational life, and of making organizations more efficient and better governed.

The factors that I have cited so far as explaining the distance between theory and practice in this field of studies (namely, the scant attractiveness of theories for practitioners because of their formal characteristics, and the interest of the theorists in influencing more their colleagues than managers) probably have deeper-lying and more remote causes which can be uncovered by more closely analysing the characteristics and internal structure of the community of management scholars and experts. Precisely because of the conspicuous investments made by the advanced societies in this field, this community is today of vast size; but it comprises a series of sub-communities that can be arranged along a continuum at one extreme of which is solely intellectual interest in theories, while interest and involvement in the practice of management lie at the other.
Broadly speaking, we may say that the members of this vast community gravitate around three main nuclei of interest: management research, management design, and management education. The first nucleus comprises more academically oriented scholars interested in studying management and organizations as social and cultural phenomena; the second, scholars more interested in ‘management theory’ viewed as normative science (how organizations should be designed and managed); and the third, those more specifically concerned to translate the expert knowledge about management – produced by management researchers and management designers – into management ‘practice’, through training or consultancy. In this last group, those who study and those who are studied overlap, in that the sub-community extends its off-shoots into companies, where it includes internal consultants, specialists, and even line managers interested in rationalizing their work (Gagliardi, 2006).

These sub-communities often act as separate worlds, each with its own journals, its own ‘sessions’ (even at single-theme conferences), its own paradigms, and its own reference values (Barley et al., 1988). In particular, the pragmatics of academic discourse and the pragmatics of practitioners’ discourse are quite different. On one hand, academically oriented scholars, barely concerned with the practical relevance of their thinking, are reluctant to extract from their theories causal frameworks which could meet the problems and requirements of practitioners. On the other hand, the practitioner-oriented discourse is very often conceptually weak, due to excessive haste in translating new ideas into simple causal models. This fuels a reciprocal tendency to differentiate and distinguish, and scant interest in communicating, and gives rise to a vicious circle in which everyone loses something: the academics lose the opportunities and creative stimuli that could derive from more immediate exposure to the reality of management; the practitioners miss chances to access richer and more sophisticated models for the interpretation of organizational reality (Gagliardi, 2006).
This structuring of the field and these institutional arrangements are difficult to alter, because they spring from cultural attitudes distinctive of the Western industrialized societies, which have robust roots in the profound cultural change usually associated with the advent of ‘modernity’. The ‘modernity’ considered to be the product of the Enlightenment and the scientific-industrial revolution had, and still has, among its cardinal principles the division of labour and the specialization of tasks. Binomials like ‘theory and practice’, ‘thought and action’, ‘pure’ knowledge and ‘applied’ knowledge express distinctions and fundamental oppositions. They are used to classify and give sense to our experience, to delimit areas and spheres of influence, to define and protect distinct professional identities, to structure relationships and hierarchies among different social groups. The ideological – and therefore preconceived – preferences for one or the other of the two poles of the opposition respectively characterize the culture of academe and that of the professions, two ‘strong’ cultures which resist attempts at mediation and seek to bring within their orbit (to return to their own logic) ‘intermediate’ and weakly characterized institutions of education or research.

Such intermediate institutions are induced to adopt accreditation strategies inspired by the values that they perceive as predominant in a particular historical period or in a particular cultural context, thus relinquishing their possible role as ‘bridge’ institutions between the two worlds of ‘thinking’ and ‘doing’. By invoking the primacy of practice, one may pursue existing practices and become incapable of innovating them, or even interpreting them; that is, become unable to rationalize and make transferable – to the extent that it is possible – the tacit knowledge which they embody (Gagliardi, 1984). By contrast, the conviction that the prestige of a professional school can be increased by giving it an academic imprint may induce an artificial increase in the degree of difficulty and the level of theore ticity of study programmes, thereby furnishing education
which is no longer either ‘general’ or ‘vocational’ (Guillet de Montoux, 1991). I have often met researchers in the fields of ‘strategic management’ or ‘organizational behaviour’ resolute in their conduct of second-order research in, respectively, economics or sociology – their purpose being to gain acceptance by the more ‘academic’ segments of the scientific community, whose approval they most wanted – rather than using economics or sociology to conduct first-order research in strategic management or organizational behaviour.

These cultural attitudes also explain the ambiguous social status of problem-driven research, which is sometimes viewed, also by those who engage in it, as “fieldwork of a strategic and restricted sort” (Schein, 1987; Gagliardi, 1991). Problem-driven or applied research is usually contrasted with basic research, also called ‘pure’ or ‘disinterested’. These adjectives speak volumes about the social status of applied research: if basic research is pure and disinterested, applied research can only be – by default – ‘impure’ (and therefore dirty); and because it is ‘interested’ it is suspected of not having value in itself but as only being instrumental to something else. Moreover, ‘problem-driven’ research (which by definition generates knowledge relevant to practice) uses mainly qualitative research methods which do not have the same ‘scientific’ respectability as investigative methods based on measurement of the observable aspects of phenomena.

The prestige of quantitative methods has been consolidated by the fact that for decades many researchers have taken the equation between a positivist approach to research and the ‘scientificity’ of that research for granted. In the field of organization and management studies, the continuing preference for these methods among academically-oriented researchers is indisputable. *Administrative Science Quarterly*, the journal which such researchers consider the most prestigious in the field, devoted a special issue to qualitative methods in organizational research as far back as 1979, when it was edited by Karl Weick. Many considered this event to be a turning-point in the accreditation of
these methods in organization studies. The special issue was introduced by an essay by Van Maanen (1979) bearing the significant title: ‘Reclaiming qualitative methods for organizational research’ (my italics); and the heading of the essay’s first section was even more telling: ‘The territory is not the map’. But it is sufficient to read the abstracts of the articles published in 2009 by ASQ to realize that thirty years later the situation has not substantially changed. I now quote the theoretical propositions that summarize the findings of the projects illustrated in some articles selected at random from those published in 2009:

“…large-scale social movements external to an industry can influence the creation of new market opportunities.” (ASQ, 54/1); “…regional agglomeration and network centrality exert complementary, but contingent, influences on organizational innovation.” (ASQ, 54/1); “…in one bank's benchmarking program…prestigious firms and firms linked to the bank through executive migration were disproportionately likely to be recruited as benchmarking partners and, once visited, to be highly influential.” (ASQ, 54/1); “…charities operated by paid personnel and full-time management show higher levels of rationalization.” (ASQ, 54/2); “…discoveries of corporate deviance that damage the legitimacy of the responsible organization may also have consequences for other organizations…[because]… audiences generalize from deviance by one organization to others that are similar.” (ASQ, 54/2); “…the strength of the negative effect of bonuses on promotions varied across managerial groups due to differences in managers' bargaining power.” (ASQ, 54/3); “…a strategy of simple rules, which combines improvisation with low-to-moderately structured rules to execute a variety of opportunities, is viable in many environments but essential in some.” (ASQ, 54/3); “…ambiguity in a rating organization's classification system increases with the number of conflict-of-interest relationships it has with producers of the products it rates.” (ASQ, 54/4).

I do not intend to make superficial and summary criticism. I have no doubt that all these propositions have been rigorously tested, and that many readers, as well
as the reviewers who recommended their publication, found these results interesting. But what can one say about their practical importance? I doubt that a manager in the industrial sector in which these studies were conducted would find these results useful or even surprising: I fear instead that in some cases the relations ‘discovered’ would strike them as obvious.

We therefore return to ‘problem-driven’ research. I believe it important to emphasise that such research entails a conception of experience of the real world which is entirely different from that entailed by the positivist paradigm dominant in the more traditional sectors of the academic world. It is to this different conception that problem-driven research owes its richness and its capacity to generate new knowledge. The ‘pure’ academic does not ‘experience’ the reality of ‘doing’, with which s/he often does not even come into contact. S/he generally obtains data, and therefore abstractions, about the real world; but s/he does not have ‘sensations’ of it, where the preliminary condition for clinical research is the capacity to come physically into contact with the reality without the filter and the censorship of intellectual abstraction. This ‘intimacy’ with the real world – which springs from sensory experience – makes it possible to draw on what Langer (1967, 1969) called ‘knowledge by acquaintance’, the intuitive knowledge of everything which both the organizational actors and the researcher know but are unable to put into words, or which they may even not know that they know. The sharing of experience by the researcher and the researched, and their reciprocal help in rationalizing it, is essential in this research paradigm. In my experience of clinical research, the help that my clients have received from my diagnoses has been much less important than the contribution made by these experiences to my effort to construct theories. And the findings of applied research often possess for the organizational actor (besides the researcher) a quality of ‘revelation’ or ‘unveiling’. It is as if someone – precisely by removing a veil – makes the ineffable sayable, showing us something that was before our eyes but which we
could not see; something that we knew but did not know that we knew. Our reaction is: ‘how true that is!’, and we are therefore inclined to embrace that belief and let it guide our actions. This subjectivity of evidence, this finding the proof of a statement in our own experience makes the theory not only a vehicle of knowledge but also a source of energy.

In an early article (Simon, 1967), the Nobel prize-winner Herbert Simon argued that whereas in basic research a scholar who finds it impossible to give a satisfactory answer to the question initially posed can modify and simplify that question until it is proportional to his/her capacity to answer it, a researcher confronted with the problems of real organizational life raised by users does not have this option. This observation of Simon has induced me to believe that the mismatch between the problem and the researcher’s present ability to answer has its own advantages: the wish to help or the need to make oneself useful lead on to formulate more ambitious research agenda which, though they may be handled in an over-hasty and superficial way, may well stimulate the development of creative modes of cognition of organizational phenomena. In a certain sense, moral pressure can create fruitful intellectual pressure.

In the same article, Simon argued more generally that the ideal conditions for the development of organizational and managerial knowledge can be created only within business schools well rooted in the two different social and value systems of the scientific and the business worlds but independent from both of these. The experience acquired in the around forty years that have passed since Simon wrote his article shows that the dilemma of ‘location’ more or less close to one or the other of these two worlds is the crucial strategic issue for every business school. Those that concentrate on research often end up by characterizing themselves academically; others prefer to be functional to firms (and they are sometimes managed like firms), essentially becoming professional training centres at their service; yet others – fortunately more than a few – are able to act as authentic ‘bridge institutions’ between the two systems. These last
are given the task – essential in my opinion – of definitively legitimating problem-driven research, reducing the gap between theory and practice which I have described and becoming the true engines of theories empowering for action.

Those who know the writings and work of Richard Normann will not be surprised that I have devoted the first of the lectures bearing his name to these topics. Normann was one of the most brilliant problem-driven researchers of his time. His books are rare examples in the managerial literature of works which have been greeted with respect and interest by academics because of the originality, elegance and plausibility of the theories put forward, but at the same time eagerly read by managers for their empowering qualities. As Mintzberg said of Normann, he was “...an intellectual presence with about as fertile a mind for inferring concepts from practices as anyone I have read in this field”. And perhaps precisely because his theories were inferred from practice they possessed and possess that quality of ‘revelation’ which surprises the receivers and induces them to put those theories into practice. My analysis has highlighted the need to reduce the distance between theory and practice, making the former useful and the latter better. Richard Normann has shown us that this is possible, and he has shown how it can be done.
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