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Submitted papers sent	8	7	7	8	30
Papers accepted after first review	0	5	7	0	12
Papers accepted after second review	7	2	0	4	13
Papers rejected	1	0	0	4	5
Book reviews	4	2	1	7	14

Albert Martin*

New Directions in Organizational Behaviour? **

Organizational behaviour research possesses some endemic problems which constrict its progress. This introduction to Management Revue's special issue on organizational behaviour debates six deficiencies in the current research: an unclear conception of how to understand the "organizational" in organizational behaviour; the lack of studies which connect micro- and macro-behaviour; a sterile concentration on the study of relationships instead of mechanisms; a superficial understanding of the practical side of organizational behaviour research; an ignorance of the upheavals in the world of work; and a stagnation in theoretical developments. The last section introduces other articles in this issue of Management Revue.

Key words: **Organizational Behaviour, Micro-Macro Link, Behavioural Mechanisms, Praxeology**

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** Introducing article by the editor.

Why new directions in organizational behaviour? Is organizational behaviour research not prospering? Didn't it produce much in the way of interesting knowledge? To deny the merits of organizational behaviour research would certainly be foolish. It has profoundly advanced our understanding of the world of work and it has given us valuable prompts to improve working conditions.¹ Nevertheless organizational behaviour research possesses some endemic problems which constrict its progress. In what follows I will enter into six debates on current research. The deliberations centre on widespread misunderstandings and bad habits that need rethinking and thus lead to new directions in organizational behaviour research. In the last section I describe the intentions and content of the articles in this issue of Management Revue and how they contribute to tackling the debated problems.

1. What is organizational behaviour?

The first question deserving greater attention is simply in what sense do the studies assembled under the heading of organizational behaviour truly speak about *organizational* behaviour, i.e. about behaviour that is peculiarly "organizational". Certainly job satisfaction and the other main topics in the literature on organizational behaviour such as job performance, justice perceptions, leadership, and group decision making, have something to do with organizations. Jobs are normally done in organizations, fairness refers to payment and careers which play a major role in organizations, leadership is a question of significance for every superior, and the groups under study are often work groups which do their jobs in organizations. But that is perhaps all. Looking at the research field of "organizational behaviour" it seems as though it would suffice to speak of "behaviour studied within organizations" and consequently there would be no need to refer to a term like "organizational behaviour" at all. See, for example, the readings of Huseman/Carroll (1979) where organizational dimensions are covered in only one out of nine chapters and even in this chapter the question of the organizational impact is not really discussed. Instead one can find some very generalised deliberations on organizational design, the matrix organization and the contingency approach. The only exception is an article by Chris Argyris which critically describes some differences between the needs of individuals and the requirements of jobs. Similarly in the readings of Staw (1991) very few of the forty articles contain any reference to the characteristics of organizations. One article entitled "corporations, culture, and commitment" does indeed touch directly on our question, but on closer inspection it merely provides some shallow insights along the lines of culture being critical to developing "intensity and dedication"- characteristics that are described as ingredients of successful firms. Five of the articles in the Staw readings appear to contain at least an indirect reference to our question because their subject matter contains something relating to the management of organizational innovation. But on delving deeper it transpires that two of them don't really get near the question at all. A third states on a very general level, (which has nothing to do with specifics of organizations), that power interests

¹ For reviews on organizational behaviour research see Mitchell 1979, van de Ven 1981, Cummings 1981, 1982, Staw 1984, Schneider 1985, Ilgen/Klein 1988, O'Reilly 1991, Warner 1994, Wilpert 1995, Mowday/Sutton 1993.

may dominate economic interests in the innovations process. A fourth describes bureaucratic barriers to innovation but without reference to behavioural processes. Only the last of the five articles gets to the core of our problem, simply because its subject “the learning organization” deals by definition with the intermingling of individual and organizational aspects. But the result of this treatise is rather meagre: “A learning organization is qualitatively different from knowing, understanding, and thinking organizations. Its managers and members behave differently. We believe that several core behaviours are evident in learning organizations: openness, systemic thinking, creativity, personal efficacy and empathy” (McGill/Slocum 1995: 480). That our two examples from the literature are no exceptions becomes clear if one has a look at the readings of Thompson (2003), which collates 25 articles “... that have exerted a tremendous impact on the field of organizational behaviour” (Thompson 2003: XV). In fact not one of these articles deals with the question relating to which specific features in the nature of an organization affect the behaviour of its members and participants.²

2. How do behaviour in and behaviour of organizations relate?

Interestingly enough the first article in the Thompson readings is an essay from Staw (1991) which takes quite the opposite perspective in asking not what impact organizational attributes could have on the behaviour of individuals, but whether theories about individual behaviour can help to explain the behaviour of organizations. Staw asserts that in many cases the behaviour of organizations can be identified with the behaviour of concrete persons, (the CEO; the entrepreneur; or some other powerful person), and therefore could be explained in the same way as individual behaviour. But this position is not really convincing. Organizations are by definition collaborations of their participants. Therefore, even if a representative of an organization is in a strong position, he or she will act in a particular social context, which inevitably shapes his or her actions. To say that the behaviour of organizations is expressed by the behaviour of its representatives does not resolve the question of what is organizational behaviour. Much more compelling are the thoughts of Staw on organizational structures and forces which can *counteract* possibly harmful behavioural tendencies of organizational members. But again these deliberations only go to show how important it is to investigate the way in which the behaviour of organizations evolves out of the interplay between collective players and socio-structural and cultural facts. Regrettably, impressive empirical studies into this problem are missing. Certainly a lot of significant contributions exist concerning the link between “microbehaviour” within social systems and the “macrobehaviour” of these systems (c.f. Schelling 1978; Alexander/Giesen/Münch/Smelser 1987; House/Rousseau/Thomas-Hunt 1995; Bohnen 2000), but they stay on a methodological level and up to now have not really inspired research into organizational behaviour.

² The problem connecting “organization” with “behaviour” is fairly well known to organizational behaviour researchers, but the solutions offered are not satisfactory. So, for example, Mowday/Sutton (1993), in their otherwise very interesting treatise, equate “organizational” with “contextual” and in so doing essentially ignore the problem of how organizational and individual phenomena are contingent on each other in the first place.

3. Why so many relationships and no mechanisms?

In organizational behaviour research, as in other branches of behavioural science, the mainstream follows a kind of “boxes with arrows” methodology in which the researchers try to “explain” the variance of a dependent variable. To do this they select “important” independent variables, give them some plausible causal ordering, choose a procedure to measure these selected variables present in a suitable population, and try to test the assumed relationships between the variables with the help of pretty elaborate statistical methods. There is nothing wrong with this. The problem lies in the kind of explanation you are looking for, the selection of which is, more often than not, misguided. To understand this, one has to be aware that the lists of determinants included in the researchers’ regression equations simply represent causal factors. Yet it does not suffice merely to show that these factors produce the predicted effect: “To cite the cause is not enough: the causal mechanism must also be provided or at least suggested” (Elster 1989: 4). Actually the bulk of organizational behaviour research is in search of causal determinants but forgets to ask for the causal mechanisms which make the empirical effects come about.³ Certainly the editors of research journals demand that their authors deliver the theoretical underpinning of their studies. However, a closer look usually reveals that theoretical reasoning on its own does not really show how behavioural mechanisms work but at best “explains”, (in a fairly loose manner), why one should expect to see the relationships specified in these studies. This is not the same as investigating behavioural mechanisms. To explain an empirical relationship, one doesn't have to look for behavioural mechanisms - one can deduce an empirical proposition from very different kinds of premises. To describe the functioning of behavioural mechanisms is much more than to give some reasons why a (statistical) relationship should exist. To cite but one example, the famous group-think model clearly specifies the relationships between its explanatory variables: “Group-think ... is a linear model of how seven antecedents increase the likelihood of premature concurrence-seeking (groupthink), which leads to eight psychological symptoms of group-think, which lead to eight symptoms of defective decision making, which lead to poor decision outcomes” (McCauley 1998: 143). But does the model explain anything? Janis, the discoverer of the group-think phenomenon is quite realistic: “The problem of why group-think occurs is more difficult to investigate than the problem of who is vulnerable and when. But the ‘why’ is the heart of the matter if we want to explain the observed phenomena of concurrence-seeking” (Janis 1982: 254). He also states quite candidly that he could contribute only a few preliminary suggestions to a true explanation (ibid.). Indeed 25 years of groupthink research have shown that many more determinants than are mentioned in Janis’s group-think model can be detected which “explain” to some extent the variance in intensity of group-think symptoms. But these deliver only peripheral insights, the crucial insights come from knowledge of the dif-

³ Besides this, one has to notice that in the social world in which “everything relates with everything”, one will always find statistical effects which can be quite easily interpreted as representing plausible causal effects. The statistical methods used do not really make a difference: whether working within simple variance-analytic designs or with the help of voluminous Lisrel-models, statistics on their own say nothing.

ferent behavioural mechanisms which may produce the group-think phenomenon (Esser 1998; Fuller/Aldag 1998).

4. Are statements about the practical consequences of research results reliable?

Not in every case. Nevertheless, more often than not, organizational behaviour researchers feel the urge to give their insights a practical flavour, which is often unsuccessful. So, for example, it is inappropriate and exaggerating to deduce far-reaching recommendations from small-scale results – a frequently notable bad habit. Especially precarious is a kind of thinking, which I would like to call “data-praxeology”. Data-praxeologists look at their data as if it represented the world in all its essential aspects when in fact it is only a perspectively conceived and narrow selection of some aspects of a much more complex world, (“measured” in somewhat doubtful ways). Data analysis in the world of data-praxeologists gains a life of its own and the members of this artificial world transfer their conclusions, (that in the first place only refer to the data), without any qualms onto the real world. The resulting practical recommendations are dubious from the start. The very nature of scientific propositions forbids deductions of any unconditional and undifferentiated practical statements (c.f. for example Bunge 1967). In order to arrive at concrete and acceptable practical solutions, one always has to make a sound assessment of the situation in which the knowledge emanating from a study can represent but one element at best. Alongside this there is a whole lot more information to be assessed such as, for example, the application prerequisites of a practical creative measure, the feasibility of suggested actions, possible side-effects, the installation of control devices, the costs, the involved interests, the tacit value judgments, the acceptance of the persons who will be affected. And last but not least one should not forget the logical quality of ones research results. The most empirical investigations in organizational behavioural research still follow the rightly criticized significance test methodology which allows only statements of an opaque probabilistic meaning and very low information content (Meehl 1978; Martin 1989). That’s worse enough but in making practical recommendations it gets even more worse, because the vague statistical tendencies one has detected suddenly are treated as if they were quasi deterministic regularities. I do abstain from listing concrete examples on the many “sins” made in trying to milk some practical substance out of one’s scientific endeavours, a simple look in the research journals documents the unsatisfactory state of the art of making organizational behaviour practical.

5. Where are the changes in the world of work to be found in the organizational behaviour literature?

From its very outset organizational behaviour research possesses the aura of a non-critical, in a way positivistic and technocratic, undertaking. And indeed the cliché is understandable. Many studies draw an unrealistic picture of a harmonious organizational world. In addition the constructs and theoretical approaches in organizational behaviour research are frequently of a one-dimensional sort, which in no way represent the complex subjects they deal with. Furthermore the “managerial implications” that are routinely given, often enough promote the impression of being blatantly one-

sided (see above). Lastly, the world of organizational behaviour seems to be a predominantly psychological world in which problems are primarily located in the individual person, not in the social and economic intricacies of organizational life. Three aspects of organizational behaviour literature contribute to this picture: the questions asked; the disregarding of the context in which the research questions are embedded; and the manner in which the questions are addressed. As to the first, one wonders about the lack of attention the upheaval of the labour markets gets in the organizational behaviour literature. See, for example, the 88 Articles in the Volumes 1993 to 2003 of the Series "Research on Organizational Behaviour". Not even one of these deals with new forms of employment. Some questions never appear on the research agenda, although they are of utmost societal relevance. To list just some examples: Why do white collar workers show no solidarity with their blue collar colleagues who are suffering job losses, even if they are to expect the same fate but with a certain time lag? (Theriault 2003). What detrimental effects result from the often ambiguous posture of employers against the career aspirations of their middle managers? (Kotthoff 1997). Why is it possible that able and reflective employees acknowledge an often superficial and egotistic team-ideology? (Sennett 1998). As to the second the tendency of organizational behaviour research is to ignore the societal conditions in which the behaviours studied are embedded. For example, it would be much more interesting to study the corruption of social conditions that allow such a perverse phenomenon as charismatic leadership to emerge, than to look for the impression management manoeuvres a superior might employ. The tendency to block out the societal context is especially prevalent in the realm of industrial relations. Certainly a lot of work has been conducted within the industrial relations' division of the social sciences. But the point to be made is that the ties that exist between the topics of the industrial relations literature and the concepts and theories used in the organizational behaviour research are at best very loose (for some exceptions see Herriot 2001; Coyle-Shapior et al. 2004). Finally, one cannot fail to notice that the manner in which organizational behaviour research treats its research questions often has the effect of belittling their importance. Consider, for example, conflicts in organizations which not infrequently assume an incriminating character in that they may damage self-perception and destroy long-standing, close social relationships. In contrast, organizational behaviour literature often treats such conflicts as mere technical problems to be settled easily if only the right instruments are used.

6. Are there any new answers to the old questions?

One of the first questions of organizational behaviour research concerns the relationship between job satisfaction and job performance. After countless studies on this topic still no real conclusion can be made. Although the empirical evidence provided only small or medium correlations not everybody accepts this, as exemplified by Brief when he notes: "Nevertheless, I still suspect a consistent, significant job satisfaction/task performance relationship is out there to be found" (Brief 1998:43). He argues that job satisfaction primarily affects "contextual performance", i.e. performance that is achieved by extra-role behaviour. This does not necessarily improve performance in the immediate workplace, but strengthens the co-operation between the

members of an organization and consequently produces results at the organization level rather than at the individual level. Brief therefore demands: "It is time to aggressively pursue the job satisfaction/task performance relationship at the organizational level of analysis" (Brief 1998:44). Perhaps after some more long years of research we will find clarification. But for now one can predict that the success of this research endeavour will not depend on the number of the empirical studies but on the quality of the theoretical foundations that will guide these studies. Generally, the missing link between job performance and job satisfaction does not lie in the personal or situational variables that one needs to take into consideration. The genuine reason lies in the very nature of the conventional job satisfaction construct. Because it conceptualises an all-inclusive attitude, it is not reasonable to expect it to determine fully any specific behaviour, (such as performance), which is always the result of multiple forces. This does not mean, however, that a broad attitude such as job satisfaction does not have behavioural effects. What it needs is an adequate theoretical treatment on a more aggregate level. On a more basic level the job satisfaction construct has quite a different meaning and is much more interwoven with fundamental behavioural processes, (March/Simon 1958:). In a similar way many of the constructs which are used in organizational behaviour research need theoretical refinement. But in this fact lies another weakness where theories are concerned: even in the newest textbooks one can be certain to find nothing but the old favourites. As to motivational theories there are, of course, Maslow, Herzberg, Adams and Porter/Lawler. And ruling in the research papers is the so-called SEU-paradigm, which states that humans always select the behaviour which promises them the utmost utility. It is true that some SEU theories are highly differentiated (Hollenbeck/Klein 1987; Katzell/Thompson 1990), but at the core they say the same as already described in detail by Vroom (1964). The main criticism about these approaches is that they do not really move from their static viewpoint because they simply assume that the various positive and negative utilities of an act are summed together. Certainly there are some promising approaches which try to consider behavioural processes as sense-making, identity-seeking, attributing and willing, but what is missing are theories which integrate these ideas in order to describe the process in which organizational behaviour manifests itself. Finally, another remark concerning the content of organizational behaviour research needs to be made whereby we return to our first point, the "organizational" in organizational behaviour. Organizations are co-operative entities. This truth does not attract the attention it deserves.⁴ There are indeed some interesting approaches, such as, on a more macro level, the deliberations of Burns (1990), or, on a more micro level, the studies of Tyler (1999). But in the majority of cases the fact that the behaviour of organizational members is deeply imbued with the social character of organizational life does not gain much credence.

⁴ Even at the most fundamental level of organizational life, namely the decision to participate at all, the (potential) quality of the relationship between the individual member and the organization (or its representatives) will play a decisive role.

7. The contents of this issue

This issue of the Management Revue is not tasked to resolve the problems of organizational behaviour research outlined above. The articles have their own claims to make. They deliver nevertheless some contribution to tackling the problems described, either via explicit discussion or, more often, implicitly by virtue of the route they take.

In the first article *Adrian Furnham* gives a critical overview of what is state of the art in work in work and organizational behaviour. He describes terminological differences; political influences on the study of work behaviour; and the viewpoint of psychologists and sociologists on the life of work. Then he deals with the question of why speak of “organizational” behaviour at all, (the first of the problems I stated), and gives an answer which takes account of the social nature of organizations, (see the last of the problems I mentioned). Furnham then discusses some severe weaknesses in work and organizational behaviour research: amongst others, political correctness; anecdotes rather than data; no powerful theories; a derivative methodology; and attempting tractable rather than important problems. Despite the negativity of this section his overall conclusion is that recent developments promise a good future for work psychology and organizational behaviour. The reason for this he sees in some good pieces of theoretical work, in the quality and quantity of research articles in international journals, and in the breakdown in disciplinary demarcation.

The article from *Ulrich Hoffrage and Torsten Reimer* deals with some interesting heuristics people use in decision making and problem solving. In order to explain the way they function, they have to describe fundamental behavioural mechanisms and therewith touch upon our question number three discussed above. The authors centre their deliberations around the question: “Can it be rational not to use information even when it is available?” They answer this question in the affirmative. They do not want to question that it *is* frequently better to know more than less. Instead they want to draw attention to situations in which it is beneficial not to use available information when making decisions. In the first part of their article Hoffrage/Reimer explain the concept of fast and frugal heuristics and introduce the notions of bounded rationality and ecological rationality. In the second part they focus on the recognition heuristic and on the Take The Best heuristic. Both heuristics work well even if they are characterized either by a lack of knowledge or by ignorance of available information. Hoffrage/Reimer discuss the ecological rationality of these heuristics, provide empirical evidence for their propositions and illustrate some applications.

The article of *Johannes Lehner* asks how environmental and situational conditions determine the use of implementation tactics. Lehner distinguishes, amongst others, between autocratic and participative tactics. He can show empirically that the perception of environmental threats will lead to the use of more autocratic tactics. His results also show a decrease in the influence of environmental factors through the formulation of a strategy and that the existence of such a formulated strategy will lead to more participative tactics. It is worth noting that Lehner derives his hypotheses by studying behavioural processes in the interaction of management and employees. To take an example: in a highly uncertain environment the motivation to gain control over the

flow of events will increase on the side of management and, at the same time, the subordinates involved will accept more restrictive measures than in a placid environment. Lehner's argument shows that one should not be satisfied with the black box methodology which is typical of the contingency approach in organization theory and that it is possible to connect micro- and macro-analytical aspects of organizational behaviour, (the second question I discussed above).

Guido Strunk, Michael Schiffinger, and Wolfgang Mayrhofer in their article diagnose as a deficiency in current organizational behaviour research the emphasis on individuals in organizations, whilst larger social units such as the organization itself are neglected - a problem I also mentioned above. Furthermore they criticize the lack of attention paid to the temporal aspects, dynamics and complexity of organizational behaviour phenomena in general and of larger social systems in particular, and declare a scarcity of research methods that can be applied to dynamic systems. Then the authors present in some detail basic concepts of "complexity theory" and diverse methods used within the scope of these theoretical concepts. In a further step they apply two methods from chaos research to test the following hypotheses: "Are professional careers in the 1990s more complex than they were in the 1970s?" and "Are these "new careers" complex or random processes?" The results concerning the first hypothesis are mixed, concerning the second they show that career paths are not random but complex, dynamic structures that can be put down to deterministic processes. In the last part of their article the authors persuasively discuss the opportunities and limitations of complexity theory to enrich organizational behaviour research.

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