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Big-B versus Big-O: What is *organizational* about organizational behavior?

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Summary This paper is an empirically grounded essay about the current state of organizational behavior (OB) research and productive future directions. We report the results of a survey of OB scholars about the current importance of various research topics and their importance in an ideal world. We compare the survey responses with an archival analysis of papers published in leading OB journals over a 10-year period. We suggest that many of the topics that our respondents perceive to be 'under researched' can be summarized with one particular definition of OB that emphasizes *organizing* behavior. Considering all three definitions together, we highlight the limitations of the traditional (*Big-B* and *Contextualized-B*) definitions and discuss the benefits of a more organizational (*Big-O*) approach. Copyright © 2001 John Wiley & Sons, Ltd.

Introduction

The purpose of this *empirical essay* is to be provocative. This paper is *empirical* in that it surveys leading organizational behavior (OB) scholars to identify which topics they believed are currently important in the field and which should be important in an 'ideal' world and we compared their responses to an archival analysis of papers published in leading OB journals over a 10 year period. However, the paper is an *essay* because it is less like a traditional theory-building or theory-testing study, and more like an empirically grounded opinion piece. We use the empirical evidence as a springboard to raise questions about the boundaries and trajectory of research in OB, and to highlight areas of the field that may deserve more attention in the future. We explore three potential definitions of organizational behavior, and we argue that the empirical results suggest some provocative, but useful, answers to the question: 'What should organizational behavior be?'

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What is organizational behavior?

The field of organizational behavior (OB)[1] has emerged from the disciplines of psychology, sociology, political science, and economics, although it is primarily identified with psychology (Schneider, 1985). It has often been defined as studying behavior of individuals and groups within organizations (e.g. see annual review articles such as Mitchell, 1979; Schneider, 1985; or introductory texts such as Duncan, 1978; Organ and Bateman, 1986; Wagner and Hollenbeck, 1995). Thus, the field has historically focused on the behavior and attributes of individuals and groups, while focusing less attention on the organizational aspects of OB. Because of this focus, OB has come to be synonymous with what is referred to as 'micro-OB' (e.g. Staw, 1984).

One concern that stimulated us to prepare this essay is that OB – as currently defined – seems to be having less and less of an impact on how research on organizations is conducted. For example, O'Reilly (1990) pointed out that, among general management journals, the market share of micro-OB research has dropped dramatically over time. This drop may have happened because management researchers have expressed less interest in research that fits a traditional narrow definition of OB. Yet, as a field, we have rarely examined the implications of how OB has come to be defined, or what the alternatives might be.

There are many potential reasons for the decline observed by O'Reilly, but at least two have been noted by multiple reviews of the field. First, reviewers have noted that OB research tends to focus more on empirical studies rather than theory, and they called for more careful attention to theory (Mitchell, 1979; Staw, 1984; Ilgen and Klein, 1989; O'Reilly, 1990). In addition, reviewers have noted that our field tends to borrow heavily from related fields without necessarily contributing new insights (Ilgen and Klein, 1989; O'Reilly, 1990).

In this paper, we report on a survey of leading OB researchers that asked these scholars to identify theoretical themes that are actually present in the current OB literature, to suggest themes that would receive more attention in an 'ideal' literature, and to identify gaps between current and ideal research foci. By identifying topics that researchers see as under-researched and over-researched, we hope to encourage research on important OB topics that may recapture the attention of the management field. In addition, by suggesting topics that are under-researched, we hope to point out areas where the field requires new theoretical contributions to ensure its vitality. Such areas represent opportunities for OB research to contribute important insights to our peer disciplines rather than unilaterally borrowing from them.

Note that by focusing on under-researched *topics*, our focus differs from an important methodological debate that has been actively engaged over the last few years (e.g. Pfeffer, 1993; Van Maanen, 1995; for a review see Fabian, 2000). Our goal here is not to consider methods for conducting research, but to consider which topics represent interesting and important questions for researchers in organizational behavior.

Although our survey allowed us to ask leading researchers about the actual and ideal importance of a wide array of potential research topics, it is impossible to be comprehensive in any single survey. Thus, one of our goals in this paper is to propose a broader definition of organizational behavior that could summarize the results of our survey and provide a useful agenda for future research. At the end of the paper, we use our results to discuss two traditional ways of defining organizational behavior that appear to us to fall short of providing a useful agenda for research. We also propose a third definition

[1]We will refer to OB as a 'field' throughout this paper. However, it could certainly be argued that OB is a subfield within a larger disciplinary umbrella (often referred to by labels such as 'organizational research,' 'organization science,' or even 'management'). They key distinction we intend to convey by using the term, however, is that OB can be distinguished from other organizational sciences (e.g. 'organization theory' or 'macro-OB').

of organizational behavior, that we believe better summarizes the results of the survey and that provides a useful principle for directing our attention to research topics that may be more ideal.

Methodology

We report two kinds of data: (1) *a key word frequency search* that identified the percentage of articles in leading OB journals that dealt with various research topics[2]; and (2) *a mail survey* that was sent to members of the editorial board of the *Journal of Organizational Behavior* and asked them to rate the 'actual' and the 'ideal' importance of a wide range of research topics. Each of these data collection efforts is described more fully below.

Journal article sample

We identified a sample of leading journals that published a substantial number of empirical articles in organizational behavior as described by the domain statement of the Academy of Management OB Division. We focused on six journals which had published a total of 2461 articles during the 1990s: *Academy of Management Journal* (*AMJ*, $n = 360$); *Administrative Science Quarterly* (*ASQ*, $n = 141$); *Journal of Applied Psychology* (*JAP*, $n = 802$); *Journal of Organizational Behavior* (*JOB*, $n = 397$); *Organization Science* (*OS*, $n = 168$); *Organizational Behavior and Human Decision Processes* (*OBHDP*, $n = 593$). We included in our database all articles published in these six journals from 1990 through 1999.

Specifying key words for topics

We defined research topics in terms of common key words that appear in a key word field in the PSYCHINFO database. These key words are chosen by authors at the time their article is accepted for publication, so they represent the authors' view of the most important general topics that are addressed by their research. For example, when Hinds and Kiesler (1995) published their analysis of telephone, e-mail, and voice mail communication in seven departments of a large telecommunication firm, they chose the following keywords: boundary spanning, communication technology, electronic communication, hierarchy, and structure.

To develop the list of topics used in our survey and literature search, we downloaded the key word field from all the articles published from 1990 to 1999 in the six journals above. We then scanned the contents of these key word fields, and selected terms based on two criteria: (1) they concerned aspects of organizational behavior; and (2) the term appeared relatively frequently across journals. We compiled an initial list of approximately 100 terms, and then edited the list down to 49 by combining similar terms (e.g. absenteeism/attendance, emotions/affect), or by eliminating terms that were ambiguous (e.g. interaction has both a social and a statistical meaning). Our goals in culling the list of key terms were to accurately reflect the scope of the field, to minimize the difficulty to completing the survey (see below), and to allow the results to be interpreted more clearly. In addition to the topics that seemed to be historically common, we included three topics that seemed to us the represent emerging areas of interest as evidenced by recent scholarly conferences and edited volumes or special issues (cross-cultural, legitimacy, trust).

In total, we selected a list of 49 key words (or key word combinations) that are listed in Table 1.

[2] We will use several words (i.e., 'key word,' 'research topic,' 'term' and 'theme') interchangeably to refer to the substantive focus of research within OB and, in particular, to the articles used in the sample analysed here.

Survey

Our survey was performed for a November 1999 conference that was designed to allow the incoming JOB editorial board to discuss the journal in the broader context of thematic trends and opportunities in the field of organizational behavior. We e-mailed the survey to 70 randomly selected JOB board members (out of the 111 members of the JOB Board).[3] We received 25 responses, for a response rate of 36 per cent.

Although the JOB editorial board represents a convenience sample, we believe it captures a reasonably good cross-section of OB scholars. In addition, these scholars are in a good position to assess the current state of organizational behavior. They are active researchers themselves, most have served on the editorial boards of several of the other journals included in this study, and they play a key role on the board of JOB (one of the few journals that focus exclusively on OB themes). Thus, they provide a unique opportunity to collect and aggregate the opinions of credible, informed, and diverse observers and practitioners of OB research.

Survey participants asked to judge the actual and ideal importance of research topics represented by 49 key words. For each of the key words, we asked respondents to answer two questions (How important is the topic today in the literature? How important should this topic be ideally in the literature?). They answered each question by rating each keyword on a five-point scale (1 = not important; 5 = very important). We will refer to these questions as the 'actual' and 'ideal' questions.

Key word frequency search

For each key word, we computed the percentage of articles in the six journals that listed each key word. We considered the key word fields of all articles ($n = 2461$) published during the 1990s for the six journals studied.

Methodological limitations

We raise two cautions to keep in mind when considering the results that follow. First although we provide objective data for the key words we consider in our study, these key words were derived from our subjective analysis of the key word fields of the six journals. Although we made every effort to select key words that were representative, we cannot guarantee that we selected key words that represent the most important or most frequent key words across the journals. Second, key words, by their nature, are fairly ambiguous and our methodology could magnify that ambiguity by isolating words outside of their surrounding context. We tried to avoid ambiguous terms whenever possible, but we clearly missed at least a few ambiguous key words. For example, the key word 'conflict' that we included in our survey is ambiguous because it might appear in a paper that deals with *interpersonal* conflict or in a paper that deals with *intra-personal* conflict (e.g. role conflict). As a result of this ambiguity, when we asked respondents to rate the current and ideal importance of 'conflict', individuals may have rated different meanings of 'conflict.' Despite these limitations, we feel that the data represents a useful empirical starting point for discussing the field.

Results

Table 1 reports the data from the survey and the key word frequency search. The table lists key words in descending order based on the gap between how the respondents rated the ideal importance of a

[3]The remaining board members were randomly assigned to receive a survey on a different topic.

Table 1. Gap between actual and ideal level of research on different organizational behavior topics and actual appearance of these topics in selected organizational journals

Gap between ideal and actual ratings	OB Topics (key words)	OB Scholar ratings of the actual and ideal amount of attention to this topic		Percentage of articles in selected journals that cite this key word (1990–1999)					
		Ideal	Actual	*JOB*	*ASQ*	*AMJ*	*JAP*	*OBHDP*	*OS*
1.1§	Norms	3.2	2.1	0.00	0.02	0.00	0.00	0.01	0.00
0.9§	Communication	3.6	2.8	0.01	0.01	0.02	0.01	0.01	0.09
0.9§	Performance	4.0	3.2	0.15	0.13	0.19	0.21	0.13	0.10
0.9§	Organizational change	3.9	3.0	0.01	0.02	0.01	0.00	0.00	0.03
0.9§	Family	3.3	2.4	0.03	0.01	0.01	0.02	0.01	0.00
0.8§	Risk	2.9	2.1	0.01	0.01	0.02	0.00	0.08	0.02
0.8†	Cross cultural	4.1	3.4	0.01	0.00	0.01	0.00	0.00	0.00
0.7§	Trust	3.7	3.0	0.01	0.01	0.02	0.01	0.00	0.02
0.7‡	Interdependence	3.1	2.4	0.01	0.04	0.00	0.01	0.00	0.00
0.6§	Cooperation	3.4	2.8	0.01	0.04	0.02	0.00	0.02	0.02
0.6‡	Learning	3.4	2.8	0.00	0.03	0.01	0.02	0.03	0.13
0.6‡	Networks	3.0	2.5	0.01	0.07	0.02	0.00	0.00	0.02
0.6†	Emotion/affect	3.3	2.7	0.03	0.03	0.02	0.01	0.01	0.02
0.5†	Turnover	3.4	2.9	0.04	0.01	0.03	0.03	0.00	0.02
0.5†	Politics	2.8	2.3	0.01	0.04	0.01	0.00	0.00	0.02
0.5*	Burnout	3.3	2.8	0.05	0.01	0.00	0.00	0.00	0.01
0.4†	Participation	3.0	2.6	0.01	0.01	0.01	0.01	0.00	0.01
0.4*	Team/group	3.9	3.5	0.03	0.13	0.10	0.10	0.15	0.11
0.4*	Feedback	3.1	2.7	0.02	0.01	0.01	0.02	0.04	0.01
0.3	Conflict	3.5	3.3	0.07	0.03	0.03	0.02	0.01	0.02
0.3	Structure	2.7	2.4	0.04	0.07	0.04	0.02	0.02	0.07
0.3	Alliance	2.4	2.1	0.00	0.02	0.01	0.00	0.00	0.04
0.3	Incentives/pay/reward	3.5	3.2	0.01	0.03	0.01	0.01	0.01	0.01
0.3	Personality	3.0	2.7	0.02	0.01	0.01	0.04	0.00	0.00
0.3	(Over-) confidence	2.1	1.8	0.00	0.01	0.01	0.01	0.05	0.00
0.3	Relationship	2.4	2.1	0.05	0.04	0.04	0.03	0.01	0.05
0.3	Influence	3.1	2.8	0.02	0.03	0.03	0.02	0.02	0.03
0.3	Supervisors/supervision	2.8	2.4	0.06	0.00	0.05	0.03	0.02	0.01
0.2	Commitment	3.6	3.5	0.11	0.03	0.08	0.05	0.01	0.02
0.2	Absenteeism/attendance	2.8	2.6	0.02	0.01	0.01	0.01	0.00	0.00
0.2	Legitimacy	2.1	2.0	0.00	0.01	0.00	0.00	0.00	0.01
0.2	Psychological contract	3.0	2.8	0.03	0.01	0.00	0.00	0.00	0.00
0.2	Motivation	3.4	3.2	0.04	0.01	0.01	0.02	0.02	0.02
0.2	Socialization	3.0	2.8	0.00	0.01	0.01	0.01	0.00	0.00
0.1	Control	3.0	2.9	0.03	0.04	0.04	0.03	0.01	0.02
0.1	Identity	2.3	2.3	0.00	0.01	0.01	0.00	0.00	0.01
0.1	Strategy/strategic	3.2	3.1	0.02	0.05	0.08	0.02	0.05	0.08
0.1	Leaders/leadership	3.8	3.7	0.04	0.05	0.03	0.02	0.01	0.04
0.1	Culture	3.5	3.4	0.01	0.04	0.01	0.00	0.00	0.04
0.1	Performance evaluation	3.2	3.1	0.02	0.01	0.01	0.03	0.02	0.00
0.0	Justice/fairness	3.4	3.4	0.03	0.03	0.04	0.03	0.02	0.01
0.0	Negotiation/bargaining	3.0	3.0	0.01	0.01	0.02	0.01	0.08	0.02
0.0	Self-efficacy	2.9	2.9	0.03	0.01	0.00	0.03	0.01	0.00
0.0	Stress/strain	3.2	3.1	0.13	0.01	0.03	0.02	0.01	0.01

Continued

Table 1. (*Continued.*)

Gap between ideal and actual ratings	OB Topics (key words)	OB Scholar ratings of the actual and ideal amount of attention to this topic		Percentage of articles in selected journals that cite this key word (1990–1999)					
		Ideal	Actual	*JOB*	*ASQ*	*AMJ*	*JAP*	*OBHDP*	*OS*
−0.3	Job satisfaction	2.9	3.2	0.09	0.00	0.02	0.03	0.01	0.00
−0.4*	Decision making/decision	2.9	3.3	0.02	0.03	0.07	0.07	0.30	0.11
−0.4*	Organizational citizenship	2.8	3.2	0.01	0.00	0.02	0.01	0.00	0.00
−0.6‡	Goals/goal setting	2.8	3.5	0.02	0.00	0.02	0.05	0.03	0.00

Note. Key words are presented in order of the gap between the *JOB* editorial board's ideal and actual rankings. Ideal ratings = How important should this topic be ideally in the literature? Actual ratings = How important is this topic today in the literature (5 = very important). The entries in the last six columns represent the percentage of all articles in each journal during 1990–1999 that specified each key word.
For Ideal–Actual column: $^{*}p < 0.10$; $^{\dagger}p < .05$; $^{\ddagger}p < 0.01$; $^{\S}p < 0.001$.

topic and how they rated the actual importance of the topic in the current literature. Because we display the key words in order based on the size of the gap, the key words at the top of the table are topics that respondents saw as under-represented in the literature. Those key words appearing lower in the table were seen as adequately represented or, in a few cases, as over-represented.

Note that by referring to topics as 'over' or 'under-researched', we are relying on the aggregated assessments of a set of informed raters. On the survey itself, we intentionally left our criteria somewhat vague (e.g. the 'ideal' question simply asked 'How important should this topic be ideally in the literature?'). We don't know how our raters assessed the 'ideal' importance of a topic (e.g. Potential impact on the social sciences? Relevance for managers or front-line workers?) Later in the paper, we will present our own interpretation of these results when we discuss three definitions of OB, but at this point, note that the table essentially reports the average judgements of a set of informed raters.

We want to draw attention to some patterns in Table 1. First, it is noteworthy that our respondents, on average, wanted to see more research on almost all the topics. If we focus on the ideal–actual gap, then topics which rank positive are ones that respondents feel deserve greater emphasis than they are currently getting. Of the 49 key words on the survey, the respondents thought that significantly more emphasis should be placed on 16, and significantly less emphasis should be placed on only one! While this survey suggests that there are many directions for interesting future research, it also suggests that journal editors will need to make some tough choices. Given a limited number of journal pages, it may be difficult to create room for 16 new topics while decreasing coverage of only one.

A second set of patterns to notice is the locus of specialization and competition across topics and journals. Some topics are a special focus for particular journals (e.g. *learning* and *OS*, *decision making* and *OBHDP*, or *networks* and *ASQ*), Whereas other topics are more generally popular across all journals (e.g. *performance*) or are ignored by all of them (e.g. *cross cultural*, *legitimacy*, *socialization*). In addition, some topics seem to be avoided by some journals (e.g. *JOB* publishes less about *teams/groups* than the other journals), while other topics seem to be points of competition (e.g. *strategy* for *AMJ* and *OS*; *structure* for *ASQ* and *OS*).

Third, some journals also seem to have not only captured some topics, but to be branded by them. For example, *OBHDP* publishes 30 per cent of its articles under the key word *decision making*. Furthermore, when the decision-related themes of *negotiation* and *risk* are also included, these three themes alone account for approximately 45 per cent of all *OBHDP* articles (this figure is approximate because it does not account for overlapping key words on the same article). *JOB*'s top four themes

(*stress*, *commitment*, *job satisfaction*, and *conflict*) account for about 40 per cent of its published articles.

What are the implications of the ratings of topic importance and the patterns of journal publication? First, from a researcher's perspective, there appear to be many topics that are under-researched, and these topics may provide ripe areas for further research. However, it may be somewhat hard for researchers to know where to target research on under-researched topics. While it is clear that *OBHDP* is the major outlet for *decision making*, it is not clear what might be a good journal to send research on *norms*, *trust*, *interdependence*, or *co-operation*. This may make research on such topics appear to be riskier than it actually is. From a researcher's perspective, it may be encouraging to know that leading researchers in the field think topics are worth studying, even if these topics are not currently well-represented in the journals.

Second, from the perspective of a journal editor, it is clear that many of these under-researched topics are up for grabs and could offer a competitive opportunity. There is room for journals to make a significant impact on how our field understands these topics (as, for example, *OS* has done by publishing a substantial number of papers on *communication* and *learning*).

Three Definitions of OB

Having presented our empirical results, we now proceed to our essay-based interpretation of these results. In this section, we interpret the implications of our results for three different definitions of organizational behavior, two which have an established historical tradition, and one which is somewhat more novel. These definitions matter. Depending on how we define organizational behavior, we can broaden or limit the importance and impact of our work. We suggest that the two historical ways of defining organizational behavior have directed our work away from important topics that were viewed as under-researched by our respondents. We refer to Tables 2 and 3 to provide a visual shorthand summary of the three definitions of organizational behavior. As a written shorthand, we will refer to these three definitions as the 'Big-B', the 'Contextualized-B', or the 'Big-O' definitions of OB. Each definition is discussed below.

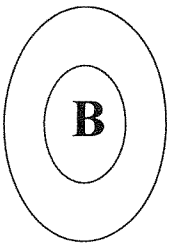
Big-B definition of OB

One possible way of defining organizational behavior is to emphasize interesting *behavior*. Our shorthand for this definition is 'Big-B' organizational behavior (see Table 2).

Some researchers, for example those who endorse a more contextualized definition of OB like the one we consider in the next section, may regard Big-B research as a kind of ideal type that does not really exist among the OB community. Yet, many researchers defend the importance of research on *stress/strain* or *goal-setting* or *decision-making* because these topics represent interesting behavior that is relevant for organizations. Indeed, one of the historical strengths of OB research is that it draws from many disciplines that study many kinds of interesting behavior. This positions us, as a field, at the intersection of the social sciences and potentially allows us to make contributions that would be broadly recognized in many fields. Consider the following definition of organizational behavior from Benjamin Schneider's (1985) review of the field in the *Annual Review of Psychology*:

> 'OB is the confluence of individual, group, and organizational studies flowing from industrial–organizational psychology and organization and management theory with headwaters in psychology (social, psychometrics), sociology (organizational, work, occupational), and management (scientific, human relations)' (Schneider, 1985, p. 574).

Table 2. Two traditional definitions of OB

oB	Big-B. Emphasizes interesting *behavior* that may be relevant for organizations. Disadvantage: Doesn't satisfy *Core Competence Test*: Is this a topic on which OB researchers have unique insights that are not likely to be shared by researchers in related social science disciplines like psychology, sociology, political science or economics?
	Contextualized-B. Emphasizes behavior that occurs *in an organizational context*. Disadvantage: Doesn't satisfy *Organizational Centrality Test*: How much would we understand about organizations if we understood everything there was to know about _____? Many behaviors that occur in organizational contexts are relatively peripheral.

In our minds, the exciting part of Schneider's definition is that it highlights the potential for OB research to tie together many fields in a way that breaks new ground and enhances the fields from which we borrow.

On the other hand, when we emphasize interesting B at the individual, group, or organizational levels, we run the risk the missing what is essential to O. The results of the survey suggest that a number of Big-B topics fell at the bottom of the list, meaning they were rated as over-researched. Of course there are many reasons that a topic might fall at the bottom of the list – e.g. because researchers are bored with the topic or because most questions about the topic have been answered. However, there are active research traditions in *stress/strain* and *goal-setting* and *decision-making* and it is unlikely that most questions about these topics have been answered. Instead, we suggest that topics like these may have been rated as over-researched because they represent interesting B but they do not tell us much about O. As interesting B, they are valuable topics for the social sciences, but if they do not tell us much about O then they may not be topics in which OB scholars have a distinctive competence.

Because what we are claiming may be controversial, let us start by critiquing our own research. In the past, both of us have done research on *decision making* (Heath, 1995; Sitkin and Weingart, 1995) and on *goal-setting* (Heath *et al.*, 1999). Both of these topics rank at the very bottom of the list.

Table 3. An alternative definition of OB

Ob	Big-O. Emphasizes behavior that is central to the task of *organizing*. Advantages: Points out centrality of organizing; eliminates peripheral behaviors; calls attention to process; requires cross-level research.

Although we would prefer to believe our research is central and cutting edge, we have to agree with our respondents that these topics, although interesting B, are probably over-researched in the field of OB. For example within the topic of decision-making, consider the specific case of research on escalation of commitment (Heath, 1995). This topic is very interesting B, and it has a long pedigree in journals of management and organizational behavior (e.g. Staw, 1976; Whyte, 1986; Brockner, 1992). However, although escalation is indeed interesting B, we question whether it is interesting O. Escalation certainly occurs in organizations, but it is a behavior that is common in many arenas. Research on escalation is active among researchers that do not care at all about organizations, for example among economists and marketers that are interested in escalation of commitment in consumer choices (Thaler, 1980; 1985; Boulding *et al.*, 1999). Furthermore, it is not clear that escalation behavior brings us as close to understanding organizations as do other potential research topics (e.g. *trust*, *interdependence*, or *co-operation*). Escalation is indeed interesting B, but despite its distinguished pedigree in our field, may not be very O.

As we look at our results, we suspect that there are other topics that rank relatively low because they are interesting B but not O. In addition to *goal-setting* and *decision-making*, this is probably also true of topics such as *stress/strain* and *self-efficacy* (see Table 1 for others). All of these topics are interesting. Someone should certainly study them. Our question is whether they should continue to receive the same amount of attention within OB that they have traditionally received. The researchers we surveyed believe that there are more fruitful topics for OB research, and we agree.

To summarize our argument about how the Big-B definition of OB falls short, we propose what we will refer to as the *Core Competence Test*. To perform this test, researchers should ask themselves the following question: 'Is this a topic about which OB researchers have unique insights that are not likely to be shared by researchers in related social science disciplines like psychology, sociology, political science or economics?' If the answer is no, then perhaps we should follow the advice we sometimes provide to our students, and emphasize those topics where our training and knowledge are likely to give us a unique competence.

Contextualized-B definition of OB

The second definition of organizational behavior emphasizes behavior *in an organizational context* (see Table 2). This is perhaps the most common way of defining the field of OB – it is certainly the most typical definition we advertise to the students who read our introductory textbooks on organizational behavior:

> 'Organizational behavior, as a field of study, concerns all aspects of human action in an organizational or group context' (Duncan, 1978, p. 6).
>
> 'OB, as an area of study, can be described as the application of concepts, theories, methods, and empirical generalizations from the behavioral sciences to the analysis of behavior in organizations' (Organ and Bateman, 1986, p.5).
>
> 'Organizational behavior differs from related fields [psychology, sociology, political science, anthropology] in its focus not just on individual or group behavior, but specifically on individual and group behavior *in organizations* (Northcraft and Neale, 1990, p. 26).
>
> 'Organizational behavior is a field of study that endeavors to understand, explain, predict, and change human behavior as it occurs in the organizational context' (Wagner and Hollenbeck, 1995, p.4).

We refer to these kind of definitions as *Contextualized-B*. Such definitions may solve some problems with Big-B definitions because they specify that behavior must occur in an organizational context. For example, under this definition, simple laboratory studies of escalation, or strain, or self-efficacy or goal setting would not be seen as central OB topics. Indeed, when researchers study behavior in context, their research may be richer because they may be forced to integrate concepts from multiple fields or to consider multiple levels of analysis (House *et al.*, 1995). Unfortunately, defining OB as Contextualized-B also creates problems. Below, we point out two problems: *relabelling* and *peripheral topics*.

Relabelling

Suppose that a marketing researcher studies escalation of commitment by using a word problem that asks consumers how they would react to a sunk investment in a theatre ticket. Such a study is an example of potentially interesting B. Suppose a management researcher relabels the word problem, changes the dollar amounts, and asks, students how they would react to a sunk investment in an R&D facility. The second researcher relabels the problem to contextualize the behavior in the setting of an organization, but it seems reasonable to question: Does this relabelling make the behavior more organizational? Consider a potentially more controversial question. Suppose another researcher moves her study of goal-setting from undergraduates in a lab setting to factory workers in a field setting. Does this change of context make the behavior more organizational?

We suggest that both of these researchers are merely relabelling a Big-B phenomenon. The behavior is interesting, but it is not necessarily more interesting because the labels of a lab study have been changed to make the context more organizational or even because the context of the study has been moved from the lab to the field.

The relabelling problem is important for our field because it may contribute to our tendency to borrow from other disciplines without contributing much in return. OB borrows from many fields – psychology, sociology, economics. This is healthy and has the potential to make our research more thoughtful and scholarly. However, at some point we must contribute in return. If we merely borrow concepts from other fields and study them in an organizational context, it is not clear that we are contributing much to knowledge in the social sciences. Our guess is that we run a serious trade deficit – we import a great deal without exporting very much. One potential explanation of the trade deficit is that we are trained to be better scholars than researchers in other fields and more attuned to reading and citing research in fields outside our own. Our question is whether, if other social scientists read our work, they would feel that they had gained new insights. If we continue to define our field as Contextualized-B, we may not be as likely to successfully export new knowledge because we will spend our time borrowing ideas about B and merely studying them in an organizational context.

The relabelling problem is also serious because it may contribute to our tendency to lag the disciplines that we borrow from. Borrowing takes time and forces us to follow (e.g. apply), rather than lead, in the production of knowledge. Reviewers have noted that OB tends to lag the disciplines – noting that developments in the basic disciplines tend to take several years to emerge in OB (O'Reilly, 1990; Ilgen and Klein, 1989). Ilgen and Klein (1989) argue that when researchers borrow or incorporate perspectives from other disciplines, they need to demonstrate that constructs and concepts from the one discipline are indeed appropriate to the other. They say that this kind of 'demonstrative' research, which simply demonstrates that concepts from one discipline can be used in another, often 'does not advance knowledge in either discipline much' (p. 345). After reviewing the field of OB from a cognitive perspective, they concluded that by 1989 the cognitive literature in OB was 'almost exclusively demonstrative' (p. 346), a sobering statement given that the cognitive revolution in psychology had been in full swing since the early 1970s (e.g. see Anderson, 1995). As long as we define our task as Contextualized-B, then we are very likely to engage in a slow process of borrowing concepts from other disciplines, merely relabelling them to fit organizational contexts. For example, it probably

wouldn't advance knowledge to show that employees have schemas about their companies just like people have schemas about other social phenomena; or to show that stressful events at work have similar physiological effects to stressful events in other areas of life. In sum, while Contextualized-B might seem to be an attractive definition of OB, we feel it contributes to the relabelling problem and, thus, tempts our field to be net borrowers (and slow borrowers at that).

Peripheral topics

It might be possible to avoid the relabelling problem if we defined Contextualized-B as behaviors that are *unique* to organizational contexts. This definition prevents the escalation researcher from simply relabelling his or her lab study because escalation is not a behavior that is unique to organizational contexts. Under the 'unique to context' definition, we might expect researchers to study topics like *job satisfaction*, or *organizational commitment*, or *absenteeism* or *turnover*. These topics are certainly more unique to the organizational context than other contexts (e.g. markets), and indeed they have been some of the most central topics in the field (e.g. see reviews by O'Reilly, 1991; Ilgen and Klein, 1989; Schneider, 1985; Staw, 1984; Mitchell, 1979).

However, note that the results of the survey suggest that these topics are not always rated high on the call for additional research. In part, we propose this is because such topics describe behaviors that, although they are unique to the context of O, are relatively *peripheral* to O. By *peripheral* we mean that such topics refer to phenomena that are not central to understanding how organizations accomplish their task of organizing.

To draw an analogy, suppose someone told you that a new academic journal had just appeared called *Sports Behavior*. You subscribe to the journal because you are a sports fan and are interested in seeing academic research on sports. As a sports fan, you could get excited any number of interesting questions: How do athletes respond to paradigm shifts in technologies (e.g. new jumps in figure skating, new strokes in swimming, new defences in basketball)? How did the management of baseball teams change when free agent rules eliminated the notion of lifetime contracts? What techniques do football coaches use to adjust play on-line during a game? Why do players have difficulty recovering from slumps? How do successful teams manage star performers and teamwork simultaneously? Perhaps you would even enjoy reading articles on novel sports behaviors like: How do highwire artists in circuses learn complex new moves in an environment where failure is costly?

As you page through the first few issues of *Sports Behavior*, you realize that the journal is not quite what you had expected. You find papers that ask players how happy they are with their position and their coach. You find rigorous studies that attempt to predict which players will miss practices. You find solid (but small) effects that indicate that players who are more committed to their team are less likely to leave it. You find debates about the factor validity of the widely-used 'happiness with coach' scale. Would you be satisfied with your purchase?

We suggest that you might not be. The behaviors in these articles, although they occur in the context of sports, are relatively peripheral to sports behavior. We suspect that many of these behaviors are also relatively peripheral to organizational behavior. To capture this idea, we propose the *Organizational Centrality Test*: 'How much would we understand about organizations if we understood everything there was to know about ?'[4] Note that even when topics are unique to the context of organizations, they do not necessarily pass the Centrality Test. Even if we understand everything there was to know about *job satisfaction* and *absenteeism* and *turnover*, we would not necessarily understand much about organizations. For example, consider absenteeism. When we think about how to distinguish very different organizations with their success in organizing and responding to the demands of their environments (e.g. the New York Yankees

[4]To elaborate the Centrality Test, consider the following additional questions: How easy is it to imagine an organization where _____ is not a central concern? How deep, basic, or causal is _____ (as opposed to obvious and salient)?

versus the Chicago Cubs or Dell versus the IBM PC division), we think it unlikely that a central feature of the more successful organizations is that people are less likely to be absent from work.

An Alternative Definition of OB

Big-O definition of OB

We propose another way of defining organizational behavior that we think addresses some of the problems with the two traditional definitions of OB described above and that captures some of the results of our survey. Under the Big-O definition of OB, topics should be *more central in research* when they capture something that is *more central to the task of organizing* (see Table 3). Drawing on Weick (1979), we purposely emphasize the verb-form of 'organizational.' When we think of *organizing* behavior rather than organizational behavior, perhaps we are more likely to develop theories that address how people solve the dynamic problems of aligning goals and coordinating action.

Under this definition, researchers would devote relatively more attention to topics that help us understand how groups of people organize and carry out their goals. As we view the results of the survey, a number of topics toward the top of the list fall under the Big-O definition because they help us understand the task of organizing. For example, if we understood more about *social norms*, we would understand more about how groups of people implicitly coordinate their action when they face a complex environment. We would also understand more about organizing if we understood how organizations can facilitate effective *communication* across divisions and hierarchical levels. We would understand more about organizing if we understood when and why people are willing to *trust* each other and *cooperate* beyond their own narrow self-interest. We would understand more about organizing if we understood *organizational change* – how to shift directions in a large group of people who are behaving in traditional, stereotypical ways.

Traditionally, when researchers have wanted to move from Big-B to Contextualized-B research, they have pursued some version of the relabelling strategy: instead of studying their phenomenon in a basic, general form, they have studied it in a specific applied organizational context; often this has meant moving from a laboratory to a field setting. Does this same strategy hold when we want to move from Contextualized-B to Big-O?

Our answer is no. We contend that Big-O does not depend on any particular methodological approach or unit of analysis. To illustrate, consider a beautiful study (Zucker, 1977) that satisfies our definition of Big-O, but violates all of the potential methodological 'recommendations' above: it is not a field study, but a laboratory study; it does not study organizations, but collections of individuals; it is not highly contextualized, but highly abstract and general.

In her study, Zucker extends the basic study of Sherif (1935) and Jacobs and Campbell (1961) on the autokinetic effect. The autokinetic effect is based on a visual illusion – when people sit in a dark room and stare at a small stationary point of light, they will believe that the point of light is moving because their own eyes engage in small, involuntary saccadic movements. Sherif showed that if you ask individuals to gauge how much the light 'is moving', they will guess quite different amounts. However, Jacobs and Campbell showed that if you put groups of individuals in a room at the same time, they eventually converage to a single guess that represents an (arbitrary) social norm. In fact, if you then replace the individuals in the room, one by one, the social norm will persist over generations (although gradually weakened). These studies illustrate how even minimal social interaction leads people to define the world in consistent ways.

Zucker conducted a study that followed in this tradition but with a Big-O focus. In addition to the issues of norms and coordination above, Zucker was interested in how the normative process is altered when some participants are made more legitimate through the kind of formalization that typically occurs in organizations. One of the most basic features of most formal organizations are job titles that convey legitimacy (which Zucker refers to as an institutionalized 'office'). In Zucker's study she selected one subject, at random, to sit in a special chair and be designated Chief Light Operator. She found that adding this aspect of legitimacy caused norms to persist even longer and with less variance.

Note that we regard Zucker's study as a prime example of Big-O, not because of its methodology, but because it integrates a number of topics that satisfy the Organizational Centrality Test: We would understand a great deal about organizations if we understood everything there was to know about *norms*, *coordination*, *legitimacy*, and *organizational change* (or in this case, the absence of change because of institutional forces). Although Zucker's study might fail some definitions of Contextualized-B because it involves a lab experiment, it does not fail the definition of Big-O. Big-O is silent about methodology, but it requires us to ask which topics are central to task of organizing, and to choose methods that allow us to address those topics.

Consider other examples of research that satisfy the definition of Big-O – for example, Sutton and Hargadon's (1996) study of brainstorming at IDEO, the famous Silicon Valley design firm that designed the Apple mouse. Brainstorming is a good Big-B topic, and there is a large research literature that verifies the following result: groups of people who brainstorm together frequently produce fewer ideas and lower quality ideas than a set of individuals who each brainstorm alone. Instead of studying brainstorming in a lab, Hargadon and Sutton study brainstorming in an interesting organization where brainstorming is central to its work; thus their paper certainly meets the definition of Contextualized-B. However, their insights move the paper beyond merely Contextualized-B by providing insights into how brainstorming affects the way that IDEO organizes itself, the domain of Big-O. For example, they argue that group brainstorming not only solves the immediate problem of a particular client, it also rehearses the organization's memory of potential solutions that may be useful in future projects (promoting organization *communication* and *learning*), it creates a status auction that allows employees to appraise their colleague's abilities and insights (providing insight about where the capabilities of the firm are located, thus reinforcing the social *network* and increasing *trust* among potential future teammates), it also socializes employees into important aspects of firm culture like the importance of allowing ideas to flow freely (thus exercising *norms* that reinforce *cooperation*).

Note that if Hargadon and Sutton had merely studied brainstorming in context (e.g. by comparing the number of ideas generated in a brainstorming session at IDEO to the number generated in previous lab studies), their study would be of limited utility because IDEO is a unique firm and things that are true at IDEO might not generalize to other firms or contexts. In our view, when a contextualized study provides useful insights into organizations (as does this one), it is valuable not because it studies behavior in a real context, but because it focuses on behavior that is essential to organizing, and by showing us how behavior is organized in a somewhat *unique* context it allows us to reach *general* insights about organizing.

For another example of this point, consider Weick and Robert's (1993) paper about how personnel on Navy aircraft carriers use 'heedful interrelating' to coordinate their actions in an environment that is unusually demanding. To describe the context of their study, they ask us to imagine shrinking San Francisco Airport down to one short runway, 'make planes take off and land at the same time, at half the present time interval, rock the runway from side to side . . . then turn off the radar to avoid detection, impose strict controls on radios, put an enemy in the air . . . wet the whole thing down with sea water and oil and man it with 20-year-olds' (Weick and Roberts, 1993, p. 357). Here, the context is so novel that it might make the results less general. However, Weick and Roberts use this novel context to provide general insights into organizing by showing how the Navy sets up *norms* about *communication* and standard operating procedures that allow a dispersed group of personnel to coordinate on a task

that requires an extraordinary degree of *interdependence*. Because their paper provides insight into how the Navy 'heedfully' organizes itself in this delicate task, it meets the definition of Big-O.

As a final example of Big-O research, consider recent work on 'transactive memory' (Wegner *et al.*, 1991; Cohen and Bacdayan, 1994; Moreland *et al.*, 1996), which shows that groups of people who work with each other regularly can overcome their own memory limitations by distributing memory across the group. These studies show how a group can acquire collective knowledge that does not reside in any individual. Much of the research on this topic has been done in the lab (and some has even surveyed married couples as the 'group' to be analysed), thus it tends to fail definitions of Contextualized-B. On the other hand, we can think of few topics that hold as much promise for Big-O OB researchers. Transactive memory researchers are studying a phenomenon that is a pure example of organizing – somehow, individuals implicitly organize their behavior so that appropriate tasks can be accomplished despite the fact that no individual or subset of individuals has the big picture. This is clearly a Big-O phenomenon.

The papers above represent the promise of Big-O research. Indeed, if we understood everything there was to know about transactive memory or heedful interrelating or the normative force of institutionalized offices, we would satisfy the Organizational Centrality Test – we would understand a great deal about organizations. Interestingly, many topics that satisfy the Organizational Centrality Test also satisfy the Core Competence Test either fundamentally or in practice (i.e., in situations where topics might fall under other fields but have been neglected by those fields). For example, *interdependence* is a topic in which, fundamentally, organizational researchers tend to have special expertise among social scientists (Thompson, 1967; Staudenmayer, 1997 – unpublished PhD Dissertation). In practice, *trust* is a topic that should be of interest to researchers in many fields such as sociology and political science, yet organizational researchers are leading research efforts on the topic (*Academy of Management Review*, 1998; Kramer and Tyler, 1996).

While we think that our definition of Big-O helps organize many of the topics towards the top of the list, not all of the under-researched topics meet our definition of Big-O. For example, *family* and *cross-cultural* topics raise interesting behavioral issues that the participants on our survey felt are not sufficiently researched.

We see at least two additional benefits of studying topics that fit the definition of Big-O and that pass the Organizational Centrality Test. First, such topics are more likely to be cross-level, and a number of reviewers of the field have requested more cross-level research (Cummings, 1982; Staw, 1984; Rousseau, 1984; Schneider, 1985; House and Singh, 1987; Ilgen and Klein, 1988; O'Reilly, 1990). It is unlikely that we will understand topics like *organizational change* or *interdependence* without understanding how these processes take place among individuals, groups, and the organization as a whole. Second, such topics are less likely to lend themselves to simple studies that simply correlate a laundry-list of behavioral variables. If we view the field as simply a collection of Bs that occur in the context of O, we naturally tend to do empirical studies that ask which Bs occur together. To the extent this has been the case, it helps to explain the frequently cited observation that OB research often tends to focus more on empirical studies rather than theory (Mitchell, 1979; Staw, 1984; Ilgen and Klein, 1989; O'Reilly, 1990). In contrast, if we view the field as focusing on the task of organizing, then we may force ourselves to develop better theory about how organizing happens.

Conclusion

By presenting the results of the survey and by proposing a definition to identify promising new research topics, we have tried to encourage us, as a field, to focus more on our outstanding opportunities.

By proposing the Big-O definition of OB, we hope to summarize and clarify what kinds of topics are likely to address important and under-addressed questions. If in Table 2, we contrast the top 10 topics and the bottom 10, the Contextualized-B or Big-B definitions of OB seems to capture less of the distinction between top and bottom 10 topics than does Big-O. Historically, we suspect that the majority of OB research has been Contextualized-B, with relatively less Big-B or Big-O. The results of the survey suggest that a shift toward Big-O topics might be welcome. Researchers should understand that Big-O topics would be well-received by peers who are likely to be reviewing our work. Journal editors should see their role as fostering Big-O research and seizing opportunities to solicit and publish Big-O topics.

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