

Still Servants of Power

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Baritz's historical analysis demonstrated that in the first half of the 20th century, groups of social scientists in the service of management emerged, with their offerings being received warmly in general. The author's reactions to an initial reading of Baritz's story in the 1970s and a recent reading in the 1990s are presented. The principal intent of doing so is to stimulate other management researchers to question who they should serve.

This is a short story about my reactions to a book I first read more years ago than I now can remember precisely. Nevertheless, I do recall that my initial reading of Loren Baritz's (1960) *The Servants of Power: A History of the Use of Social Science in American Industry* made me feel uneasy about being associated with those kind of social scientists. Upon a recent reread of the book, I felt somewhat less uneasy but considerably more perplexed about the state of my chosen occupation.

The principal intent of sharing my reactions to *The Servants of Power* is to stimulate others to pause and think about who we, as management researchers, serve. I begin my story below with a very brief summary of Baritz's book.

THE SERVANTS OF POWER ABRIDGED

Baritz opened by explaining how certain forces combined during the first half of the 20th century to create a need in the minds of some American manag-

ers for the expertise of the social scientist (principally, the expertise presumably supplied by psychologists and sociologists but occasionally by anthropologists). These forces included the increasing size and bureaucratization of corporations; the ambiguities in regards to status, function, and role attached to management being separated from ownership; the growth of organized labor; and the gradual shift in the ideology of workers toward thinking of themselves as more than simple cogs in a complex machine. Baritz, drawing on the work of such analysts as Peter Drucker (1946), concluded,

The twentieth-century industrialist has realized, to a greater extent than did his predecessors, that he must understand the living world contained in his factory. The day is gone when businessmen could believe that their power of personality and knowledge of . . . finance would prove adequate weapons in the competitive struggle. As managers realized that men, even more than nuts and bolts, played a determining role in the financial condition of their firms, the search for an understanding of men began. (Baritz, 1960, p. 15)

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Industrial psychologists led their brethren into the world of commerce and industry. According to Baritz, this leadership role rested on the research of James McKeen Cattell, who was influenced by the ideas of Sir Francis Galton concerned with “the problem of human individual differences” (Weld, 1928, p. 270). Specifically, Cattell and his collaborator Farand, in 1896, published a study involving the use of a battery of tests to measure the physical and mental status of a sample of Columbia University students. Later, other psychologists built on this first use of ability testing to develop techniques for screening job applicants to avoid hiring workers who were unsuited for the tasks required. Obviously, these techniques were appealing to managers. Less than 20 years later, Hugo Münsterberg (1913), armed with knowledge of psychological testing, demonstrated an awareness of just how useful psychologists could be to managers by formulating the first methodological outline of industrial psychology. As quoted in Baritz, Münsterberg recognized that this “new science” was “systematically [to] be placed at the service of commerce and industry” (1960, p. 36). He also recognized that this new science owed a debt to Frederick W. Taylor’s “scientific management” (Kanigel, 1997); however, this debt, at least in Münsterberg’s eyes, was not a large one because Taylor was a mere engineer and thus a psychological dilettante. In reality, scientific management gave industrial psychology its purpose.

The financial condition of the firm was the ubiquitous criteria of the success of scientific managers, and to an improvement of this condition they gave their attention. The aim was to help industry achieve the ends it defined for itself in the most efficient way. A similar acceptance of the industrialist’s ethic became characteristic of industrial psychology as it criticized scientific management for failing to make industry efficient enough. (Baritz, 1960, p. 31)

Clearly, industrial psychologists aimed to serve management, and managers were primed to receive their help by the widely publicized successes of Henry Ford in applying enlightened new labor rules (Nevins & Hill, 1954). In part, these rules addressed proper living, the enforcement of which was assigned to a sociological department staffed by 100 investigators who were empowered to visit workers’ homes to ascertain, for example, that the workers’ sex lives were without blemishes. The supposed lesson Ford taught to the American management community was that by devoting time, effort, and a little money to the human

element of its business, production and profits would rise.

World War I provided a significant opportunity for psychologists to show what they could do. The army represented a vast and visible testing ground for the personnel selection techniques that had begun to be implemented in industry. In 1916, the National Research Council was organized and mandated to inventory and mobilize the nation’s scientific resources. The following year, a Committee for Psychology was established by the council, in part, to discover ways of using psychology for military training and selection. In reference to the selection problem, the committee claimed that “officer material” could be located speedily and that men of “mental inferiority” could be eliminated (Baritz, 1960, p. 46). In general, the war focused management’s attention on personnel problems, leading to the increased prevalence of centralized personnel offices. In such an atmosphere, psychologists were turned loose at the end of the war. Many saw economic opportunities. In 1921, James McKean Cattell founded the Psychological Corporation, with a board that included many of the country’s most prominent psychologists (e.g., Hall, Watson, and Yerkes). The founding of the company, according to one of its latter officers, “established *psychological work* as a legitimate means of producing profits” (Baritz, 1960, p. 53).

Baritz devoted considerable attention to a series of studies well-known to management scholars, those conducted at the Hawthorne Works of Western Electric beginning in the mid-1920s. He devoted this attention to these studies and to the central figure identified with them, Elton Mayo, because of the associations between Hawthorne and the rise of what Baritz termed *managerial sociology*. How results of the bank wiring room experiment were interpreted exemplify this relationship; that is, explanations of the workers’ behaviors focused on their self-imposed social organization. These interpretations were critical of the earlier, narrowly focused efforts of industrial psychologists on developing selection tests. Mayo (1931) stated, “The belief that the behavior of an individual within the factory can be predicted before employment upon the basis of a laborious and minute examination by tests of his mechanical and other capacities is mainly, if not wholly, mistaken” (p. 293). Alternatively, the primacy of the group was praised as the major influence on individual behavior. According to Baritz (1960), “from Hawthorne on the problems of selection, training, efficacy, and control would never

seem as simple as they had been to the testing psychologists of the 1920's" (p. 95). Mayo, like the testing psychologists he was critical of, knew who he served. For example, he asserted that social science research, by providing management with the means of discovering the causes of labor "disorder and unrest," would make these problems "relatively easily controlled" (Baritz, 1960, p. 109).

During the Great Depression, the participation of social scientists in controlling labor discord and unrest rose substantially. In part, this rise was attributable to the formation of the Committee for Industrial Organization, which was aimed at organizing the industrial workforce, and to the passing of the National Labor Relations (i.e., Wagner) Act, which specified the rights of employees to engage in union activities. In 1933, a committee appointed by President Hoover reported that managerial attitudes toward the practice of personnel management were becoming more positive and that the principal reason for this change was the desire on the part of management to use personnel activities to curtail union strength. This managerial desire to alleviate the human problems of labor was good for industrial social scientists because they were the experts in such matters. (Particularly important during this period was the social scientist's knowledge of job attitudes.) In the struggle between labor and management, the position of the industrial social scientist was clear. For example, two psychologists (Fisher & Hanna, 1931), in a book titled *The Dissatisfied Worker*, concluded that most industrial unrest was caused by the emotional maladjustment of the worker, with the worker mistakenly attributing his dissatisfaction to his job. Psychologists were not alone in their promanagement orientation. One of the Hawthorne sociologists, T. North Whitehead (1938), argued that unions were social clubs designed to meet the psychological and social needs of their members and not a means of equalizing bargaining power.

As World War II ended the Great Depression, materials and men became scarce in industry. During the war, industrial social scientists came into their own because America's managers seemed to be forced by the necessities of wartime production to turn to the ideas so long preached by social scientists. Chief among the wartime problems were absenteeism and turnover, which management thought could be addressed by social scientists. Elton Mayo, of Hawthorne fame, concurred. He stated that "the only way to achieve control of absenteeism and labor turnover is to study such symptoms in the situations in which

they exist" (Mayo & Lombard, 1944, p. 1). This sort of research dramatically increased the use of attitude surveys in studying morale and job satisfaction. In addition, the military turned again to psychological testing, increasing its use. Moreover, the use of merit ratings rose during the war. Baritz (1960) quotes the speech of the president of the American Association of Applied Psychology to a 1944 Cleveland audience as a means of summarizing the spirit of the period, "These are thrilling times for applied science. . . . War always drives effort in the direction of utility and service. Whatever enterprises cannot be so classified tend to become outlawed for the duration" (p. 143).

The 1950s were boom years for industrial social scientists. This growth was fueled by a popular product, human relations training, either delivered at a company facility or off-site, for example, on a university campus. The content of this training for managers was diffuse, but central topics included motivation, leadership, groups, participation, and communications. The following, albeit cynical, appraisals of the lessons of human relations depict quite clearly their ideological bias.

A 1957 cartoon (Mulligan) in *The New Yorker* shows a friendly executive saying to a white-collar subordinate, "Now, I don't want you to do it my way because I say 'Do it my way.' . . . I want you to do it my way because you *see* it my way" (p. 39).

According to a United Automobile Workers publication, managers "are trooping to special classes at Harvard, where they learn workers are not the least bit mercenary . . . [and] report to the plant each morning for love, affection, and small friendly attentions" ("Deep Therapy," 1949, p. 48).

William H. Whyte (1952), an editor at *Fortune*, said,

Now one no longer need be ashamed of going along with the herd; indeed, with the aid of the new jargon he can be articulately proud of the fact. He is not just conforming, he is using "group skills." He is maintaining "equilibrium." He is "participating." (pp. x-xi)

All in all, the 1950s, despite the disparaging words of a few critics, were good years for industrial social sciences.

Baritz (1960) begins his concluding chapter by recognizing that "by the middle of the twentieth century, industrial social science had become one of the most pregnant of the many devices available to America's managers in their struggle with costs and labor" (pp. 191-192). Management's support of the social sci-

ences was not a free good. In return, social scientists were to “concentrate exclusively on the narrow problems of productivity and industrial loyalty” (p. 195). Thus, “managers made of industrial social science a tool of industrial domination” (p. 195). Depicting this domination is what an executive in the personnel department of a large public utility told an audience of social scientists in 1950 with regard to the requirements of working with business:

a willingness to accept the notion that business perform a useful function in society, and that their methods may be necessary to accomplish this function . . . [and] a willingness to accept the culture and conventions of business as necessary and desirable. (Lawshe, 1951, p. 80)

Baritz (1960) closes the book by observing, “Because so many industrial social scientists have been willing to serve power instead of mind, they have been themselves a case study in manipulation by consent” (p. 210).

INITIAL REACTIONS

I hope the above synopsis was sufficiently true to Baritz’s tale so that readers adequately appreciate the plausibility of industrial social scientists’ being servants of management in the first half of the 20th century, and of the services they delivered being received warmly by the powers that be. This appreciation is essential to understanding my reactions to reading *The Servants of Power* for the first time 20 or so years ago when I was either a doctoral student or a newly minted Ph.D. in management and organization. Also important to understanding my initial reactions is what I saw around me during that period. I had teachers telling me that the satisfaction of workers was worthy of study only because of its potential relationships to important behavioral criteria (i.e., absenteeism, turnover, and job performance; hence, my continuing affection for Nord’s [1977] “Job Satisfaction Reconsidered,” which attempted to rebut such teachings). Moreover, the message I took away from the management journals I read was that the field did not take very seriously the well-being of workers (either economically or psychologically defined) as a desired end in and of itself. The literature overwhelmingly seemed focused on understanding how to make organizations more effective, with effectiveness criteria including indicators of worker well-being only when

such measures were justified in terms of their relationships to outcomes really important to managers. That is, what I read in the journals reinforced my professors’ teachings. Although I did not ignore that my surroundings were telling me that what’s good for the company is good for its workers, it was clear to me that when push came to shove among students of management, the company came first. What troubled me equally was that even the possibility of company-worker trade-offs was virtually never even whispered (also see Bramel & Friend, 1981). In summary, the professional world I saw around me at that time was consistent with much of Baritz’s story. This consistency contributed to the discomfort I experienced initially in reacting to *The Servants of Power*.

The roots of that discomfort were within me in the form of my liberal values (which I now at least continue to espouse). These values dictated a concern for those whom I saw as disadvantaged and stigmatized. I realize such a statement may sound self-serving and/or trite, but it was (and I hope still is) an expression of my beliefs.

My initial reactions to *The Servants of Power* led me to the now quite obvious conclusion that science is not value free (i.e., our values unavoidably shape the research questions we pose [Kaplan, 1964]; also see, e.g., Connell & Nord, 1996; Howard, 1985). As a budding management scholar, I felt uneasy for having chosen a profession in which members’ values dictated a research thrust not openly inclusive of the sort of issues congruent with my values. Let me be clear on this point. I was in no way opposed to research in the service of management (nor am I now). I was troubled by such an exclusive mission.

One can feel discomfort with a presumably scientific discipline in the exclusive service of management for reasons other than because it exhibits a narrow focus that excludes concerns relevant to one’s personal values. For example, as management scholars, we study lower level members of organizations as well as organizational elites, and as participants in our research, these lower level members deserve our ethical obligation. This obligation, for example, according to the “Academy of Management Code of Ethical Conduct,” entails preserving and protecting their “dignity, well-being, and freedom” (Academy of Management, 1997, p. 1470). Most certainly, unless one naively assumes that no conflict of interests ever exists between management and labor, research in the exclusive service of management potentially could endanger the dignity, well-being, or freedom of lower level

organizational members. With regard to the charge of naïveté, some may claim that currently popular management tactics (e.g., self-managed teams) serve to increase the autonomy of workers and thereby reduce the likelihood of conflicts between management and labor. At some superficial level, this probably is true, but in the tradition of Edwards (1979), others see the tactics as transferring pressures to maintain performance from supervisors to peers (e.g., Cappelli, 1995) but recognize that managerial control of the workplace remains fundamentally intact (also see Jacoby, 1985). If, as I see it, the form rather than the substance of management-labor conflicts has changed in recent years, then the status of our research results as being potentially harmful remains. Driving such results are the questions we seek to answer or, as asserted next, the questions we do not seek to answer.

It could be argued that we also are obligated ethically to society at large (e.g., to advance science, to better the human condition, to increase understanding) (Rosenthal & Rosnow, 1991). Again, it would take one extraordinarily naive person to assume that the interests of management encompass all of those in the larger society; that is, our obligations to society at large likely would lead us to pose research questions that would go unasked if our only concern were to service management. The founders of the Academy of Management in 1941 recognized the larger obligation; they stated,

The general objectives of the Academy shall be . . . to foster: (a) A philosophy of management that will make possible an accomplishment of the economic and social objectives of an industrial society with increasing economy and effectiveness. The public's interest must be paramount in any such philosophy. (LeBreton, 1962, p. 330)

I see little evidence that the academy has encouraged management scholars to put the public's interest first or even to contemplate that interest in setting their research agendas. Recently, however, I was pleased to learn that the academy's board is considering a research newsletter on public policy issues. However, if the research that might be reported in such a newsletter was to be exclusively in the service of management, then the academy and its members would become fair game for those critics who recognize that conflicts of interests are evident in society and that power (i.e., managerial power) does not always make right.

Elsewhere, Janet Dukerich and I (Brief & Dukerich, 1991) have written about how the desire to conduct managerially useful research disrupts the scientific enterprise. Our arguments principally were methodological in nature, generally avoiding the sorts of moral concerns raised above. There is no need to repeat those arguments here; however, a brief example should indicate what we were about. Those seeking to do managerially useful research are advised to be concerned with independent variables in the control of management (e.g., Thomas & Tymon, 1982); however, doing so could limit theory development by deemphasizing or excluding those variables that management cannot manipulate but that may be potent with explanatory power. The identification of such problems is not new. In Baritz (1960), for instance, Arthur Kornhauser, a prominent research psychologist who did not see himself as an industrial social scientist, was quoted as saying,

Psychological activities for industry . . . are characterized by the fact that business management constitutes a special interest group which manifest its special viewpoint in respect to research as in other matters . . . certain crucial variables must not be dealt with. (p. 206)

All of the above seems to pertain only to our research and not to our teaching. However, except for what we might glean from our personal experiences and from the pages of *Business Week*, *Fortune*, and the like, it is the interpretations of our research that we teach. So, the concerns previously expressed about the consequences of a managerial bias for our research are relevant to what we do in the classroom. Anthony (1977) makes this point in an interesting way. He states,

Ideology plays a considerable part in the curricula of management courses at universities. . . . The ideological element is not always instantly recognizable for what it is. One reason . . . is that much of the ideological element in management education appears to be concerned with objective, scientific, research-based conceptualization of practical managerial problem-solving. . . . It would be hard to find another field of educational activity in which intelligent, and sometimes educated minds, were so harmoniously disposed. There may be occasional disagreement about educational methods, never about doctrine. (pp. 260-262)

The above is how I interpreted my reactions to first reading *The Servants of Power* some 20 or so years ago. I accepted Baritz's story line that among most of those who studied management, there was a shared ideology that created a distinct promanagement bias (for the consequences of such a shared ideology being unconscious, see Bem & Bem, 1970). Moreover, I believed that this bias, this desire to serve management, largely excluded serious attention to the sorts of research concerns my personal values dictated. Hence, I felt uneasy about my choice of a profession (obviously, however, not uneasy enough to abandon it). Below, I try to show that sticking with my choice was the right thing to do, for over the next 20 years, events unfolded to temper my initial uneasiness.

LATER REACTIONS

A recent reread of *The Servants of Power* stimulated less emotion and more thoughts than did my first read. I felt less uneasy (i.e., guilty) about being a professor of management; however, the thoughts evoked left me perplexed. As explained below, these thoughts rested on my observations that on one hand, our research literature became more inclusive and, correspondingly, critics of its managerial bias became more vocal, but on the other hand, all of this might matter less because managers may no longer be listening to what we have to say.

What most clearly reduced my unease was a change in the research literature that I as well as others perceived. In 1984, Barry Staw observed that absenteeism, turnover, performance, and the like, as the field's traditional research concerns, "can be faulted for being rather narrow" (p. 652). Indeed, this set of variables really had not expanded since the first half of the century, as evidenced by Baritz's analysis. Staw also noted that other variables may be of interest to individual participants in organizations and to the general public and went on to discuss the research pertaining to two of these, job stress and dissent. With regard to the former, he stated, "Within the last 5 years there has been a burgeoning of research interest in job stress and an effort to specify its determinants" (Staw, 1984, p. 652), but regarding the latter, he stated, "Unlike the study of job stress, research on dissent and whistleblowing has not yet been integrated into a unified stream of research or even a recognized concern for the field" (p. 654). The publication of Miceli and

Near's (1992) *Blowing the Whistle* helped signal that in less than a decade, Staw likely would have changed his assessment of how the field had received the study of dissent. The point is that by the late 1970s, our literature was opening up to research that at least could be argued to serve others beyond the management community. Thus, the sort of research my personal values dictated became acceptable to the field, but only just acceptable. That is, it seems to me that research targeted to serve workers and the public at large can be published in the field's principal journals; however, such targets are, at best, of secondary concern to the vast majority of management scholars in the United States. For instance, in our journals, the organizationally relevant attitudes of the survivors of layoffs have received considerable attention (e.g., Brockner, 1988); however, very, very rarely have the victims of layoffs (i.e., those fired) been the object of study (e.g., Winefield, Winefield, Tiggemann & Goldney, 1991). Apparently, how these people cope with having lost their jobs is not widely seen as interesting to management scholars as are the job attitudes of their more fortunate counterparts, layoff survivors. Clearly, a managerial bias still dominates the field. Supportive of this assertion are other examples of what we tend not to study: organizations engaging in deceptive advertising, organizational factors contributing to the prevalence of unsafe products and production processes, the use of bribes—domestically and abroad, greed as a result of organizational socialization processes, the role of religion in business, and the reactions of stigmatized people to hostile workplaces. The list could go on and on.

Today, the field's managerial bias is being challenged openly, sort of from within, but not very effectively (also see Martin, 1994; Pfeffer, 1997). This challenge comes from those who can be labeled *critical theorists* (e.g., Alvesson & Deetz, 1996; Burrell, 1996; Burrell & Morgan, 1979; Clegg, 1989; Frost, 1980; Steffy & Grimes, 1986). For example, according to two such theorists,

The disciplines of management are generally understood to be devoted to the (scientific) improvement of managerial practice. . . . It is assumed that questions directly or indirectly connected to efficiency and effectiveness are central and that knowledge of management is of greatest relevance (only) to managers. . . . Management is considered to be a socially valuable technical function, normally acting in the general interests of workers, employers, customers and citizens alike. In contrast, [critical theory] . . . questions

the wisdom of taking the neutrality or virtue of management as self-evident or unproblematical. (Alvesson & Willmott, 1992, p. 1)

From my perspective, this is terrific stuff; however, critical theorists do not stop with criticizing management scholars for their managerial bias. Following the lead of the Frankfurt School (e.g., Horkheimer & Adorno, 1979), they go on to critique the methods most of these scholars use and the philosophy of science on which those methods rest. Perhaps because students of critical management seem to attack everything management scholarship is about, the field generally has turned a deaf ear to their pleas for change—even though some of those pleas might seem reasonable individually.

Another reason I react differently today to *The Servants of Power* than I did 20 or so years ago is because the managerial bias still evident in the field simply may matter less. Recall that Baritz argued not only that industrial social scientists sought to serve management but also that their services were received warmly by management. Today, evidence from a variety of sources suggests that there is a chill in the air, that management no longer receives our offerings with open arms. If this is so, then the effects of the field's managerial bias, in practice, must be less.

Evidence of a chill in the air readily can be found in Porter and McKibbin's (1988) influential *Management Education and Development: Drift or Thrust into the 21st Century?* They stated,

Our overall impression from our corporate interviews is . . . [that] the business world is, generally speaking . . . *ignoring* the research coming out of business schools. . . . The total perceived impact is, judged by what we learned in some 200 interviews in the business sector, virtually nil. (p. 173)

Further evidence of the chill and the persistent but now perhaps paradoxical managerial bias are recent Academy of Management presidential addresses. For instance, Don Hambrick (1994) called for the following kinds of activities to ensure that the academy "really matters": "Our professional divisions should be encouraged . . . to conduct joint conferences with practitioner organizations in their respective domains" (p. 14), and "the Academy should initiate a new major award: a prize for exceptional scholarly contribution to the practice of management" (p. 15). Note that he did not call for joint meetings with consumers and environmental or labor groups or for a new major

award for exceptional scholarly contribution to enhancing the quality of work life. Rick Mowday (1997) in his presidential address cites a 1996 report of the American Assembly of Collegiate Schools of Business (AACSB) Faculty Leadership Task Force that discusses "the irrelevance of our research" (p. 337). His response to the task force's concerns included embracing Ernst Boyer's (1990) broad definition of scholarship, which Mowday sees as being consistent with management faculty developing "closer links to business" (p. 339). Presumably, these links would foster what Boyer calls the "scholarship of application"; thus, Mowday, recognizing that our research may not be seen as useful by the powers that be, at least implies that our scholarship should include applying our knowledge to benefit management. Another example of the chill in the air is supplied by the growing body of literature indicating that the adoption of new management techniques is not necessarily dependent on managerial scholars' providing guidance but more so on other fashion setters (e.g., the business media and management consultants) (e.g., Abrahamson, 1991, 1996). Finally, the business media has been openly critical of management scholarship. Take the case of John Micklethwait and Adrian Wooldridge (1996), staff editors of *The Economist*, who in their recent book, *The Witch Doctors*, claimed that

management theory, according to the case against it, has four defects: it is constitutionally incapable of self-criticism; its terminology usually confuses rather than educates; it rarely rises above common sense; and it is faddish and bedeviled by contradictions that would not be allowed in more rigorous disciplines. (p. 12)

All in all, it may be the case, as asserted above, that management scholars have come to be seen by their intended audience as something akin to how the sellers of ice cubes are viewed by the Eskimos. What might have happened between the time when Baritz's story ended and now? What explains the apparent fall of management scholarship in the eyes of managers? Clearly, this essay is not the place to address this question seriously, but I cannot resist posing a few responses. It might be that (a) managerial perceptions are distorted and they unknowingly are influenced by management research, (b) the quality (whatever that means) of management research has so deteriorated that its end products now are near worthless, (c) (even though we are academics) our research has become too academic, or (d) the learning curve of the field has

plateaued. Wherever the truth may lie, the good but perplexing news is that the problems associated with the field's managerial bias are lessened by managers not accepting what is served to them. We need not worry about the ethics of how our research findings are applied if our intended audience is not listening to what we have to say.

NOW WHAT?

The Servants of Power demonstrated that management scholars, at least up until the 1950s, aimed to serve the management community and generally got high marks from that constituency for the services they did render. The major problem with all of this, as I saw it, was that other segments of society were not adequately served by our scholarship. However, something changed in the past 40 years. Although the field's managerial bias may have lessened a bit, it remains clearly evident; however, the managerial community no longer seems receptive to our offerings. This presumed lack of receptivity has led a variety of voices inside and outside of the discipline to demand that our research become more applied so that we can better serve our master. My more recent reading of Baritz's book stimulated an alternative response to the criticisms that recently have been hurled at us. Individually and collectively, we ought to pause and ponder who we should be serving through our research and how well we are meeting the realistic needs of those identified groups.

Following the lead of some stakeholder theorists (e.g., Donaldson & Preston, 1995; Freeman, 1994), in identifying the groups we should be serving, we ought to consider those who do or could have moral claims on our research. These claims are attributable to belonging to any group that might benefit from or be harmed by the conduct or results of our research. Clearly, these groups would include managers and, as indicated earlier, workers, consumers, and the public at large. Not to be omitted from this list is the group we perhaps have the greatest obligation to, our students. As noted earlier, directly, but too often indirectly, the substance of what we serve in our classrooms is (or should be) a product of our scholarship. As regards the degree to which we meet the needs of our students through our research, I question how much we really do have to offer beyond helping them to understand how they might contribute to making their future employers more productive, efficient, and profitable.

To what degree does our research shed light on such matters as (a) resisting ethically questionable orders from organizational superiors; (b) directing, in a legitimate way, organizational resources toward personally important social or environmental causes; (c) blending the responsibilities of being an employee, spouse, and parent; or (d) living a religious life in the workplace? Assuming that such matters are important, who, if not us, should address them in the classroom and whose research, if not ours, should be concerned with them?

I sincerely hope that individuals ask themselves who they, as management scholars, should serve and that the answers produced are far from uniform. I suspect many of us would identify similar sets of groups but would differ significantly in terms of the relative import of those groups. Although I may judge those who are largely excluded from certain organizational roles (e.g., Blacks) as a high-priority group to serve, someone else may emphasize serving government policy makers and most undoubtedly will continue to put management first. However, the point is, by asking who we should serve, we are more likely to end up with a more diverse set of masters than is now evident. In addition, it is diversity in how we approach the study of management that we sorely need. Such diversity from a values perspective would ensure, for example, a broader array of questions being posed, thus stretching the boundaries of inquiry and hopefully providing a set of questions whose answers would help satisfy our moral obligations to a considerably wider slice of society. At the discipline level, the Academy of Management, at the very minimum, ought to reaffirm that in the endeavors it supports, the "public's interest must be paramount" (LeBreton, 1962, p. 330). Ideally, this reaffirmation should be coupled with the initiation of an open, organized, ongoing discussion between academy members on such matters as what constitutes the public's interest from the prospective of a management scholar, when might one anticipate the public's interest to collide with those of management, and how the academy can encourage scholarship that speaks to the concerns of both parties in such conflicts. To do less would be to endorse the position that management's interests are the right ones.

I want to close on a methodological note. In assessing how well we are meeting the realistic needs of those groups we seek to serve, we ought to move beyond the evidence supplied by the media or derived exclusively from the self-reports of executives. I, for

one, am not sure that the impact of our research on managers is "virtually nil" (Porter & McKibbin, 1988, p. 173). Let me qualify this assertion. I do not believe that managers read our theories and then proceed to apply them verbatim in their organizations. Such a belief would approach foolishness (Brief & Dukerich, 1991). However, I do believe that as teachers (of both managers and managers-in-training), we do more than legitimize (e.g., Abbott, 1988) our students, we expose them to the products of our scholarship and they, thereby, are led to pose questions about managing their organizations that otherwise would have gone unasked, and asking these questions sometimes yields results that help prevent costly disasters or facilitate desired outcomes. That is, I suspect that the effect of our research on managers is indirect, subtle, and often unconscious. With regard to the latter characteristic, this implies that managers, like the rest of us, often cannot trace the origins of the questions they ask. Thus, in ascertaining how well we are meeting our obligations to managers or any other group we seek to serve, one ought to consider that attempts to obtain clear-cut proof of the direct utility of our research, based on the self-reports of likely users, probably are doomed to fail or, at a minimum, to yield suspect findings. Those who are interested in evaluating how well we serve should approach the problem as they would any other research puzzle, realizing that the most obvious strategy may not be the correct one.

At the end, I feel that I have rambled too much, obscuring my central thesis: Question who you serve. In the past and more recently, Baritz did stimulate for me a variety of thoughts with regard to what I am about as a modern industrial social scientist. I hope that this essay serves to stimulate similar sorts of critical thinking for others.

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