

Review

Reviewed Work(s): Corporate Views of the Public Interest by Jeffrey A. Sonnenfeld

Review by: R. Edward Freeman

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and Kilmann, for example, found that “ideal stories” told by managers reveal implicit images and personality types. Martin’s (1980) research argues the case that stories not only make messages more memorable than explicit data-based information but also generate organizational commitment. Deal and Kennedy do provide many excellent references but do little to instruct the reader on the supporting research.

The authors’ popular style sometimes is fun and colorful: “Managers run institutions; heroes create them”; “Values are the soul of culture”; and corporate tribes are identified as tough-guy macho, work-hard/play-hard, bet-your-company and process cultures. Occasionally, extravagant language creates false impressions such as motivation is a “complex science” and heroes are the “stuff and hope of culture”; they can do what Quality Circles and MBO cannot do.

The chapter in the book that looks to organizations of the future seems almost contradictory. That is to say the contention should be questioned that chief executive officers should set the tone; they must “‘put the business in their heart’ and crowd out softer sentiments. It’s a lesson today’s managers should learn as an antidote to the type on business humanness” (p. 195). Yet prescriptions for the future call for “radical decentralized atmoized organization” (p. 193). Such is the case for the far-flung franchises of McDonalds or the independent rangers in the U.S. Forest Service, or even the Catholic Church. Each of these, the writers suggest, is a model of an organization that has built culture with soul, spirit, magic, heart, ethos, mission, and saga.

Corporate Culture is not nearly all I want in a book on this topic, but it will create a lasting impression on the corporate world. Because of such a book, corporate consciousness will be raised to the importance of creating and sustaining a culture uniquely suited to organizational goals. The perturbing side of this, to those who value a collaborative employee-participative thrust, is that the authors too much elevate the “tough image” that has been played by chief executive heroes such as NCR’s John Patterson, who once moved an executive’s desk into the street, soaked it in kerosene, and had it ignited.

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Corporate Views of the Public Interest, by Jeffrey A. Sonnenfeld. Boston, Mass.: Auburn Publishing Co., 1981, 285 pp., cloth.

Reviewed by R. Edward Freeman, School of Management, University of Minnesota, Minneapolis, Minn.

Sonnenfeld purports to offer “a map to guide the corporate executive through the increasingly important but largely uncharted terrain of public affairs” (p. 7). He has really done much more. Sonnenfeld offers a systematic account of “public affairs,” a detailed analysis of how the concept unfolds in the forest products industry, and, most importantly, the beginnings of a theory that relates managerial choice in the public affairs arena to organizational performance.

Corporate Views of the Public Interest is an outgrowth of Sonnenfeld’s doctoral dissertation at Harvard, which won the Academy of Management’s Social Issues Division Dissertation Award in 1981. It should serve as a model of “how to do an industry study” using and testing a conceptual framework in which the emphasis is on theory construction and the presentation of rich hypotheses. Sonnenfeld deals with all kinds of data, and his book is much better for wading into waters in which narrow empiricists would not dare to tread. Chapters 1 through 3 rehearse the arguments for understanding the external environment of organizations, and Sonnenfeld argues for a conception of public affairs that is “proactive,” whereby the organization actively seeks to influence the external events that affect it. The nice thing about Sonnenfeld’s approach is that it is inherently managerial, and in this area in which we know so little, it is better to start with the world as it is: real, live managers with incredibly messy problems.

Chapter 4 presents a “double loop learning” model of organization-environment interaction that Sonnenfeld uses later in Chapters 8 to 13 to explain the behavior of firms in the forest products industry. It is worth noting here that the model is perfectly general and is not necessarily applicable only to the public affairs function. Chapter 5 is on methods and possibly could have been an appendix to make the book more readable. However, it is interesting to note that methods include not only company data and standard external sources, but interviews and questionnaires with key stakeholder groups.

Chapters 6 and 7 present a generic analysis of the forest product industry and paints a picture of incredible complexity from the brute fact that it takes trees a long time to grow to the controversy surrounding forest management, environmental issues, labor practices, pricing policies, and market conditions.

The real meat of the book is in Chapters 8 through 12.¹ Chapter 8 takes functional departments as the basic unit of analysis and measures the quantity and quality of the interactions of these departments with key stakeholder groups. Thus, the general idea is that “public affairs” departments such as public relations, legal, government relations, and so on, will have different interaction patterns from those of general management, engineering, finance, and so on, yielding and inherent bias towards certain stakeholder groups. Chapter 9 takes a cut at these data by company, and Sonnenfeld can clearly group companies in the industry by their interaction patterns with stakeholders. Chapter 10 is unique and seeks to enrich the data analysis of the previous chapters by sharing some clinical observations of the companies, so that by the end the reader has a pretty good feel for Sonnenfeld’s and others’ views of the industry. Chapter 10 examines the executives’ perception of the public affairs environment, and Chapter 11 looks at key stakeholders’ perceptions of the companies.

Here Sonnenfeld found, surprisingly, that executives in departments who spend relatively little interaction time with stakeholders are more tolerant of stakeholder concerns. For example, engineers were more tolerant of environmentalists than were public affairs executives. Human resources executives were more tolerant of financial stakeholders’ concern for short term performance than were financial executives. In general, executives who have major responsibility for a particular stakeholder group were more critical and less tolerant of that group. Sonnenfeld

explains this perversity of “the better I know some group the less I like it”: departments that have relatively little contact with stakeholders tend to appreciate them more as information sources. As expected, from Mitroff, Mason, and Emshoff’s work on assumptions analysis, functional departments differed radically on which issues should have the highest priority for the company. Again, Sonnenfeld is showing how systematic bias is built into a functional structure, and the argument is that such systematic bias is not necessarily optimal in meeting stakeholder expectations about performance. He finds that there are marked differences in perceptions of the issues across companies as well as within companies, thus challenging that view that public affairs is an industry matter and raising questions about the enormous amount of interaction time that executives seem to spend with trade organizations. Indeed, Sonnenfeld found, as have Sturdivant and others, that company executives had very different perceptions of their social responsiveness than did their external stakeholders. What then is one to make of all this?

First, Sonnenfeld has reminded us again of the importance of taking a larger systems view of organizations, and that in doing research on organizations we must include these multiple stakeholders in the process. More generally he causes us to wonder about appropriate performance measures, given the systematic bias with which executives and stakeholders see the firm. It may be that the whole debate on corporate social performance should more properly be construed in stakeholder terms by asking “How does a firm keep score with each stakeholder group?” and “What is the relevance of traditional measures of performance to this stakeholder scorecard?”

More importantly, Sonnenfeld perhaps unintentionally causes us to wonder whether or not “business strategy” and “social or public affairs” can logically be separated? Good strategy does not run afoul of external stakeholders; rather, good strategy satisfies these groups. Hence, the conceptual split between strategic management and social issues (mirrored in the structure of the Academy of Management) is an arbitrary one. Sonnenfeld has begun some research to help bridge this conceptual gap and in doing so has helped us to understand the concept of public affairs in a much more interesting light.

In summary, this is a very good book. It does suffer somewhat in that it reads like a dissertation with the requisite literature review, methods, data, and

conclusions. However, it is worth the effort and is written in quite clear prose. The book is hard to find, and I fear that many will miss it. Researchers on the cutting edge of the overlap of organization theory, strategy, and social issues cannot afford to miss this important contribution.

Concentration, Mergers and Public Policy, by Yale Brozen and George Bittingmeyer. New York: Macmillan Publishing Co., Inc., 1982, 427 pp., \$29.95 cloth.

Reviewed by John R. McIntyre, Technology and Science Policy Program, Georgia Institute of Technology, Atlanta, Ga.

Brozen's and Bittingmeyer's *Concentration, Mergers and Public Policy* seeks basic answers to questions that have been at the heart of the field of industrial organization since the turn of the century such as: Will big business take over all economic activity? Are large enterprises less productive and competitive than smaller concerns? Is the traditional public policy towards large scale business compatible with the problem of productivity, growth, R&D, international competition, and a more efficient use of resources? Prior to the 1930s, prevailing attitudes in the United States viewed the rise of the large corporation as a response to technology changes largely benefitting the consumers and controlled by the forces of the market. During the Great Depression and subsequent period of inflation, a significant attitude shift came about. Markets became perceived as failing; the postwar inflation was blamed on monopoly power in concentrated industries. An assumption was made that concentration more or less automatically led to collusion among leading firms.

The tone of the traditional answers given to the questions raised by Brozen about the impacts of industrial size was set by the 1932 classic book of A. Berle and G. Means, *The Modern Corporation and Private Property*, which predicted that by 1969 all nonfinancial business assets would be owned by 200 corporations. These views led to policies whose aim was to nip incipient concentration by horizontal merger in the bud, by dissolving larger corporations with sizable assets or market shares and generally restricting mergers. By the 1970s, a revisionist group

of economists, to which Brozen belongs and which is largely associated with what is generally described as the Chicago school, challenged these assumptions. They considered that the studies done from 1935 to 1970 on which these assumptions had been based employed inaccurate data, used unrepresentative industry and firm samples, and misinterpreted historical trends and statistical associations so that their conclusions largely reflected the researchers' preconceived opinion that concentration per se was anti-competitive.

The debate has centered around the so-called structure-conduct-performance paradigm, which holds that because industrial structure (concentration/size) is assumed to determine conduct (price, market behavior), it also defines market performance. The post-1970s research rejects this paradigm and the concentration-collusion doctrine. It considers that reverse causation (i.e., performance and conduct determine structure) might be operative and that concentration and collusion are not necessarily associated in any predictable way.

Brozen's book, in a sense, is the culmination of the revisionist stream of research. It is a superb, in-depth study of the ongoing debate that has revolutionized the field of industrial organization. This debate (and its supporting literature) has in fact been carried out in the professional journals but has not yet appeared in economics textbooks, with perhaps the exception of Dominick Armentano's (1982) critical study. Brozen's book has the distinct merit of masterfully surveying the evolution of economic thought as it pertains to industrial organization in the United States without putting the reader through the complicated mathematical, statistical analysis and the technical jargon that pervade the literature. The book thus is accessible to a broader audience but retains its substantive accuracy and has thorough chapter notes. It is surely fated to become as well known and used as Robert Bork's (1978) *The Antitrust Paradox*, which analyzed the unsatisfactory state of antitrust law from the viewpoint of a legal scholar. The book is a logical companion to Bork's work, written from much the same critical and scholarly perspective.

A chapter overview gives a sense of the breadth of the book's coverage. Chapter 1 raises the question of how concentrated American industry really is. Brozen and Bittingmeyer berate the pessimists who