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Stakeholders, Social Responsibility, and Performance: Empirical Evidence and Theoretical Perspectives

Jeffrey S. Harrison
University of Richmond, harrison@richmond.edu

R. Edward Freeman

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STAKEHOLDERS, SOCIAL RESPONSIBILITY, AND PERFORMANCE: EMPIRICAL EVIDENCE AND THEORETICAL PERSPECTIVES

JEFFREY S. HARRISON
University of Central Florida

R. EDWARD FREEMAN
University of Virginia

The management of competing stakeholder interests has emerged as a significant topic in the management literature. Related issues are the relationship between stakeholder management and the perception that a firm is socially responsible, and the performance implications of both stakeholder management and social responsibility. Theory and models surrounding these issues are abundant, but empirical research is in an early stage. This research forum reports six excellent efforts to tackle fundamental ideas about stakeholders, social responsibility, and performance.

Although social issues have been debated for centuries, only recently have they joined the mainstream management literature as a legitimate area of inquiry. Consequently, the past decade or so has seen a proliferation of ideas about the proper role of business organizations in society. Interest in the topic has been promoted by increased sensitivity to ethical issues among individuals and organizations, especially in the more economically advanced nations. Issues such as damage to the environment, improper treatment of workers, and faulty production leading to consumer inconvenience or danger are highlighted in the media. Government regulation has proliferated. Investors and investment fund managers have begun to make investment decisions on the basis of social responsibility as well as pure economics. Consumers have become increasingly sensitive to the social performance of the companies from which they buy.

Heightened ethical sensitivity, increasing competition, and a hyperactive media have combined to create a very difficult management situation. On the one hand, managers must devise strategies that will make their organizations competitive in the world economy. That is, they must provide high returns for their shareholders. On the other hand, however, some strategies lead to actions that various stakeholders find offensive. Effective stakeholder management can help managers resolve these types of ethical dilemmas (Freeman, 1984; Freeman & Gilbert, 1987; Harrison & St. John, 1996). From this perspective, managing competing stakeholder interests is a primary management function (Ansoff, 1984). In addition, stakeholder theory has potential as an integrating theme for the business and society discipline (Donaldson & Preston, 1995; Jones, 1995; Wood & Jones, 1995).

This special research forum, "Stakeholders, Social Responsibility, and Performance," was created to highlight research regarding the relationship between socially responsible organizational behavior and various types of performance. We were pleased to receive 49 submissions from all over the world. We learned a great deal from the manuscripts themselves and from the review and decision processes. In this introductory article, we describe the six studies that are published in this special research forum and comment on their contributions to the literature. We then provide a few remarks on some empirical issues that seem particularly relevant to business and society researchers. Finally, we evaluate where the field is today—what researchers know and what they need to know if the field is going to advance.

IN THIS SPECIAL RESEARCH FORUM

One issue of central concern to the ongoing social responsibility debate is whether organizations pursue the satisfaction of stakeholder interests for economic reasons or simply because doing so has intrinsic merit (Donaldson & Preston, 1995). In "Does Stakeholder Orientation Matter? The Relationship between Stakeholder Management Models and Firm Financial Performance," Shawn Berman, Andrew Wicks, Suresh Kotha, and Thomas Jones develop testable models around these two competing perspectives. Their first model, which they call *strategic stakeholder management,* reflects an instrumental approach, suggesting that concern for stakeholders is motivated by the perception that it can improve financial performance. Their second model, which they refer to as *intrinsic stakeholder commitment,* rests on the assumptions that firms have a normative (moral) commitment to advance stakeholder interests and that this commitment shapes firm strategy and influences financial performance. Berman and his colleagues offer a longitudinal test of these two competing perspectives in an attempt to determine which model is most accurately reflected by the data. We hoped that the intrinsic model would receive at least some support; however, the empirical tests support only the instrumental approach.

Very little research has been done to help identify which stakeholders really count to managers. Mitchell, Agle, and Wood (1997) developed a theoretical model that advances the idea that stakeholders become salient to managers to the extent that those managers perceive the stakeholders as possessing three attributes—power, legitimacy, and urgency. In "Who Matters to CEOs? An Investigation of Stakeholder Attributes and Salience, Corporate Performance, and CEO Values," Bradley Agle, Ronald Mitchell, and Jeffrey Sonnenfeld test Mitchell and colleagues' (1997) model. Using primary data collected from CEOs combined with social and financial performance data, they discover that the three stakeholder attributes do indeed significantly increase stakeholder salience.

Another critical issue in stakeholder theory is whether managers can successfully balance the competing demands of various stakeholder groups. Stuart Ogden and Robert Watson examine the ability of U.K. water companies to balance shareholder and customer interests in "Corporate Performance and Stakeholder Management: Balancing Shareholder and Customer Interests in the U.K. Privatized Water Industry." They use government information on customer service levels as well as firm accounting and market performance data to examine this issue. They find that increasing customer service levels have a negative influence on profitability in the short term because of the costs associated with improving customer service. However, the increases in customer service levels are linked to increases in market value, a reflection of investors' ability to ascertain the long-term benefits from high levels of customer service.

In "Integrated and Decoupled Corporate Social Performance: Management Commitments, External Pressures, and Corporate Ethics Practices," Gary Weaver, Linda Treviño, and Philip Cochran focus on how external pressures and top management commitment influence the nature of corporate ethics programs adopted by large corporations. The present study builds on prior work in which they demonstrated how the scope and control orientations of ethics programs are influenced by these same factors (Weaver, Treviño, & Cochran, 1999). In the present study, they find that external pressures are most likely to lead to ethics programs that are easily decoupled from organizational processes; policy communications like memos and newsletters would be a feature of such easily decoupled programs. Well-integrated practices such as ethics-oriented performance appraisals were found in companies in which top management was highly committed to ethics. Weaver and coauthors' findings are an excellent demonstration of the importance of managerial commitment to the creation of a meaningful ethics program.

The special forum also includes two excellent research notes on topics that are important to the business and society literature. In "Stakeholders and Corporate Boards: Institutional Influences on Board Composition and Structure," Patrice Luoma and Jerry Goodstein investigate factors that influence the proportion of board seats filled by nonshareholder stakeholders such as suppliers, employees, and public officials. They discover that only about 14 percent of board seats are filled by such stakeholders; however, modest growth in representation is found for the period of their study. Companies that are larger or participate in industries with higher degrees of regulation tend to have greater proportions of nonshareholder stakeholders on their boards.

Finally, in "The Effects of Corporate Governance and Institutional Ownership Types on Corporate Social Performance," Richard Johnson and Daniel Greening use a structural equation modeling technique to test an integrated model of the effects of institutional investors and various governance devices on corporate social performance (CSP). They find that organizations with higher equity ownership by pension funds and organizations with higher levels of outside director representation tend to have higher corporate social performance on a number of dimensions. In addition, higher levels of top management team equity are related to higher levels of product quality. Also of interest, organizations that perform well on the "people" dimension of social performance (relations with employees, women and minorities, and communities) tend to have higher financial performance as well.

Taken together, these articles represent new knowledge about business and society that is both broad and deep. Also, the methods employed cover a wide range of techniques. In the next section, we comment on a few methodological issues.

EMPIRICAL ISSUES

Because the business and society field is relatively young as a discipline, empirical tools are only beginning to be developed. We saw a wide variety of methods in the 49 papers that were submitted. Hypothesis-testing techniques ranged in technical sophistication from simple *t*-tests to structural equation modeling. We received a number of event studies and several case studies. The 6 papers that made it successfully through the review process are indicative of the wide range of methods we found in the initial pool. Technical sophistication did not appear to be a factor with regard to the final accept/reject decisions. However, reviewers were concerned that the techniques used were carefully executed, empirically valid, and appropriate for the theories being tested.

We could comment on many empirical issues, but we focus our attention on two highly critical areas. We begin with a few remarks on existing databases and data collection in general. Then we comment on two particular methods that seemed to create the most problems for authors with regard to the review process: case methods and event studies.

Databases

The *Fortune* reputation survey is a popular source of information on social performance (Griffin & Mahon, 1997). The *Fortune* rankings are based on the opinions of senior executives, directors, and analysts who are asked to rate the ten largest companies in their own industries on eight different aspects of reputation, one of which is social performance. The social performance scale is highly correlated with overall corporate reputation, which suggests that a halo effect may exist (Fryxell & Wang, 1994). Since ill deeds tend to be more widely reported in the media than good deeds, the executives and analysts who perform the ratings are likely to be heavily influenced by negative outcomes or—if nothing is reported in the media for a particular company—may attribute financial success to social factors whether or not such a relationship exists. Perhaps the biggest weakness of the *Fortune* rankings is that they provide only one summary measure of social performance. The *Fortune* data may be more appropriate for use in research on organizational reputation within the strategic management literature.

The Kinder, Lydenberg, Domini, and Company (KLD) index is another popular source of data on social performance (e.g., Graves & Waddock, 1994; Griffin & Mahon, 1997). Social performance on factors such as community impact, diversity, employee relationships, environmental impact, and product safety are assessed in this index. Numerous articles were submitted to the forum that made use of the KLD database, including three of the six articles that appear in this issue. One of the advantages of the KLD ratings is that they are based on the extensive research of independent analysts employed by the firm Kinder, Lydenberg, Domini, and Company, the investment advisor for a socially oriented mutual fund. KLD uses the ratings as a basis for investment decisions and advice. In this issue, Berman and colleagues provide detailed information about the KLD database in the body of their article and in an appendix. You may want to read their article first if you do not already have an appreciation of what this database contains. We believe that KLD data, in spite of their limited coverage of only 650 large companies, will continue to be used by researchers in the field for many years to come. Researchers are now investigating the effects of studying individual variables within this database as opposed to combining them into a single measure of social performance. This trend is demonstrated in all three of the articles in this issue that make use of KLD data.

Although the KLD database is an excellent source of data, and other published data on social performance also exist, we are concerned that too much dependence on the relatively small amount of currently available data may stifle creative thinking in the field by limiting theoretical development to ideas that can be readily tested. Consequently, we were delighted to find excellent examples of primary data collection in many of the studies submitted to the forum. For example, Weaver, Treviño, and Cochran called 1,000 industrial and service companies to identify the person who would most likely be familiar with each firm's ethics practices. Armed with a mailing list of 990 firms, they surveyed these officers, using up to two follow-up mailings. In another excellent example, Agle, Mitchell, and Sonnenfeld combined KLD variables with primary data collected directly from the CEOs of KLD companies. Although the size of their sample was not optimal ($N = 80$), their research questions regarding CEO values and perceptions could not have been answered through the analysis of archival data (they did, of course, test for nonresponse bias).

We also suggest looking for data sources associated with large-scale changes such as restructurings, bankruptcies, and new government regulations. An example of this technique is found in the article by Ogden and Watson. They took advantage of privatization of the water industry in the United

Kingdom to conduct a very interesting study. The U.K. government took a careful approach to releasing control of this industry, collecting data about customer service that might ordinarily not have been collected. Odgen and Watson used these data, in combination with financial information, to investigate whether these firms were able to simultaneously enhance the interests of their new shareholders and satisfy customers.

Finally, we believe that small-sample, case-based studies can be a source of rich data. We will now comment on case research and event studies as they apply to the business and society discipline.

Case Research

Case research is an excellent method for theory building. Since the field of business and society is young and since no widely accepted integrating framework exists (Jones, 1995), case research is especially critical. Several interesting case studies were submitted to the special research forum. Only one of them is published herein, and it includes multiple methods. The Ogden and Watson article contains a detailed narrative of changes that took place in the U.K. water industry and the effects these changes had on the ten newly created firms in the industry. In this sense the article reports a case study, but it also reports an assortment of quantitative tests. The acceptance rate for case-based research was similar to the overall acceptance rate for this special forum; however, we would still like to share some of the insights we gained as we moved through the review process for the cases that were submitted.

The purpose of the *Academy of Management Journal (AMJ)* is to publish rigorous empirical research that advances knowledge in the field of management. Consequently, reviewers take special care to ensure the rigor of the studies published in *AMJ*. Case studies *can* qualify as rigorous empirical research, but our reviewers were fairly critical of most of the cases that were submitted to the forum. It seemed to us that one of the great differentiators in the minds of the reviewers was whether a study was designed with purpose or "just happened." For example, researchers may become actively involved in an organization as consultants or while collecting data for another study. In the process, they observe something and decide to write a case study around it. Reviewers were not sympathetic to this kind of case research.

Eisenhardt (1989) and Yin (1994) provided excellent guidelines for what might be termed "case research with a purpose." Both Eisenhardt and Yin strongly advocated entering the case study process with specific research questions and a deliberate case study design. Yin (1994) suggested that the kinds of questions that are best addressed by case research include how and why questions. Elements of the design include selecting cases, crafting instruments and protocols, and establishing a procedure for analyzing the data (Eisenhardt, 1989).

Typically, the quality of empirical research in the social sciences is measured in terms of construct validity, reliability, internal validity, and external validity. Yin (1994) made several suggestions for conducting case analyses that will pass these commonly accepted tests. With regard to construct validity (establishing correct measures for the constructs under consideration), Yin suggested using multiple sources of evidence, establishing a chain of evidence, and having key informants review a draft of the case study report. Reliability means that a study could be repeated by another researcher and yield the same results. Satisfying this requirement means that the initial researcher needs to document, in great detail, the case study protocol, and he or she may also want to establish a case study database.

Internal validity, the establishment of a causal relationship, is relevant only for explanatory or causal studies. To satisfy this condition, Yin advised researchers to do pattern matching, explanation building, or time series analysis. Finally, the requirement upon which most case studies are judged to be deficient is external validity, or generalizability. In response to critics who have argued that the results of case studies are not generalizable, Yin stated, "Such critics are implicitly contrasting the situation to survey research, in which a 'sample' (if selected correctly) readily generalizes to a large universe. *This analogy to samples and universes is incorrect when dealing with case studies.* This is because survey research relies on *statistical* generalization, whereas case studies (as with experiments) rely on *analytical* generalization. In analytical generalization, the investigator is striving to generalize a particular set of results to some broader theory" (1994: 36; emphasis in original). Nevertheless, Yin suggested that one way to deal with criticism of this sort is to use a replication logic with multiple cases.

Event Studies

Several event studies were submitted to the special forum. None of these studies survived the review process; however, some of the tests Ogden and Watson used in their case-based study include shareholder returns, the typical dependent variable in event studies. McWilliams and Siegel (1997), in their article on the appropriate use of event study methodologies, suggested that event studies are probably not valid for most social responsibility research. We would like to explore this idea in a little more depth.

Event studies are based on the hypothesis that the stock market is efficient in its ability to absorb information that is relevant to the value of a security and that stock prices will immediately adjust to such information when it is released to the public (Fama, 1991; Fama, Fisher, Jensen, & Roll, 1969). Methodologically, event studies are simple: detect an event of strategic importance that is reported regularly in widely accessible business periodicals such as the *Wall Street Journal,* collect the names of companies that participated in the event and the dates when the information was first released to the public, and conduct a test to see how the stock market reacted to the release of this information. An event study's results should be similar to what would be found in a survey of financial market participants about how a particular event will influence future returns.

Event studies are based on the assumption that investors, in the aggregate, understand the extent to which an event will influence returns. Unfortunately, investors, as humans, are limited in their ability to absorb, process, and interpret all of the available information. Consequently, they are likely to simplify their evaluative decisions through the use of uncomplicated decision tools (Duhaime, 1985; Schwenk, 1985; Tversky & Kahneman, 1974). Factors that are widely understood, simple, and intuitively appealing may influence short-term returns whether or not they influence longer-term outcomes. Factors that are less well known, complicated to apply, or counterintuitive may have an impact on actual performance over the long term, but they are unlikely to influence short-term stock returns.

These ideas raise an important issue regarding the application of event study methodologies to business and society research. If we researchers, as experts in the field, do not fully understand how an event such as the announcement of withdrawal of business assets from South Africa or inclusion on a list of socially responsible companies can increase future profits, how can we expect the market to know these things? We speculate that consumers will be more likely to buy products from the companies participating in such events or that civil suits or environmental fines will decrease. We may also hypothesize that these events are mere signals of a bigger picture, indicating firms that are responsive to a wide range of stakeholders. However, we should recognize that investors are no more capable of sorting out these issues than we are. If an event is complicated or poorly understood, we should not rely solely on an event study to determine its financial implications. Since most of the events that social responsibility researchers study are complicated with regard to their effects on financial performance, event studies have limited application. They are, however, worthwhile supplements to other empirical methods used in the field. In essence, event studies can confirm other findings in a manner that is similar to confirmation through a survey of investor opinions.

STAKEHOLDER THEORY AND FUTURE DIRECTIONS FOR RESEARCH

The last 20 years have seen stakeholder theory come to cover a large and multifaceted area of management research. Beginning with an obscure reference in Igor Ansoff's book on corporate strategy in 1965 (Freeman, 1984) and growing to its current position in the popular press, the stakeholder idea has become a mainstay of management theory. For all of the analytical power stakeholder theory offers and its narrative refocusing on a broad set of stakeholder relationships rather than a narrow set of purely economic relationships, there is relatively little agreement on the scope of this theory. Donaldson and Preston (1995), Mitchell, Agle, and Wood (1997), and Jones and Wicks (1999) all summarize the state of current knowledge and point out a host of different, though related, theoretical issues and a variety of future research tasks and programs. We do not rehearse those arguments here but instead concentrate on some broader issues that are relevant to continued progress in conducting mostly empirical research in this area.

Three main theoretical issues are discernible from the work in this forum that points toward future research. The first and most obvious is the very premise of this special research forum and is a widely shared assumption in the business and society field—the idea that it is meaningful to separate economic performance from social performance. Many researchers have tried to find measures of social performance and to then study whether or not the companies that perform well on these measures also perform well on more traditional economic measures. This is the overall strategy Berman, Wicks, Kotha, and Jones (this issue) used. The theoretical problem is that surely "economic effects" are also social, and surely "social effects" are also economic. Dividing the world into economic and social ultimately is quite arbitrary. Indeed, one of the original ideas behind the stakeholder management approach was to try to find a way to integrate the economic and the social. Thus, researchers need to find more robust ways of measuring stakeholder effects, measures that may point us beyond the economic and social typology.

The second theoretical issue follows directly from the first. The stakeholder model, as it has been used, is not terribly sophisticated. By examining large stakeholder groups such as customers, employees, suppliers, investors, and the like, researchers ignore many differences within stakeholder groups. We need fine-grained ideas about each stakeholder group. Perhaps there are meaningful typologies of customers, suppliers, employees, and investors that might yield interesting results. The first step in theory building here would be to create rich and rigorous cases that could lead us to see the overall stakeholder relationship as a multifaceted, multiobjective, complex phenomenon. Extant sources of data such as the KLD database could perhaps be mined here, but more likely we need new sources of data to do the required theory-building work. Such a fine-grained understanding of stakeholder relationships should point to some joint research projects with colleagues in the other business disciplines who are, in some sense, stakeholder experts. Rich collaboration with marketing scholars could surely yield a finer-grained understanding of the customer relationship within an overall stakeholder framework. Similar points can be made with respect to colleagues in operations management, human resources, and finance and accounting.

Finally, this special research forum subtly calls into question two of the foundational assumptions of research in management theory. The first assumption is that normative research can be separated from descriptive research. The second, similar, assumption is that theoretical and empirical research are distinct (or even that theory and practice are distinct). Calling attention to stakeholders rather than stockholders alone and giving ethics a prominent place in the management literature raise questions as to whether research can be so easily divided. These very terms suggest that the way we choose to frame issues of organizational research has normative and descriptive consequences and is at once both theoretical and empirical (Freeman, 1999; Wicks & Freeman, 1998). Calling these assumptions into question does not undermine the logic and rigor of the methods used in these studies; rather, it creates more openness and more diversity with respect to both theory and method. By paying more attention to constructs like stakeholders and ethics and by building and testing rigorous and relevant theories using these ideas, scholars conducting organizational studies will have a bright future.

Before concluding this introduction, we would like to draw readers' attention to a recently published document that is likely to stimulate discussion in the field for many years to come. Lee Preston, Leonard Brooks, and Thomas Donaldson have recently released onto the World Wide Web what they refer to as the "principles of stakeholder management." The principles represent several years of work and hundreds of comments from a worldwide network of colleagues interested in research on the topic. By the time of this printing, the document may already have been published in hard copy by the Clarkson Centre for Business Ethics at the University of Toronto; however, it is also available at http://mgmt.utoronto.ca/~stake/.

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Jeffrey S. Harrison is a professor of management at the University of Central Florida. His Ph.D. in business administration is from the University of Utah. His research interests include mergers and acquisitions, diversification, interorganizational relationships, and business ethics.

R. Edward Freeman is the Olsson Professor of Business Administration and director of the Olsson Center for Applied Ethics at the Darden School, University of Virginia. He has a Ph.D. in philosophy from Washington University. He is the author of many books and articles on stakeholder theory, social responsibility, business ethics, and related topics.