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BETTER STORIES AND BETTER CONSTRUCTS: THE CASE FOR RIGOR AND COMPARATIVE LOGIC

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In a recent article (Eisenhardt, 1989), I attempted to describe theory building from case study research. I focused on multiple cases and how to develop theory from them, but I paid scant attention to the important role of single-case research. This response provides an opportunity to begin redressing that imbalance.

One of the central arguments of "Building Theories from Case Study Research" was that multiple cases are a powerful means to create theory because they permit replication and extension among individual cases. Replication simply means that individual cases can be used for independent corroboration of specific propositions. This corroboration helps researchers to perceive patterns more easily and to eliminate chance associations. Extension refers to the use of multiple cases to develop more elaborate theory. Different cases often emphasize complementary aspects of a phenomenon. By piecing together the individual patterns, the researcher can draw a more complete theoretical picture.

A second central argument of the article stressed the importance of methodological rigor. I described the need to identify research questions, develop well-designed instruments such as interview schedules and questionnaires, consider theoretical sampling and controls, and so forth. Of course, I recognized that these would often shift over the course of the study. Nonetheless, unlike the stereotype of case research as free-form, the implicit position of the article was that both theory-building and theory-testing studies have similar, although not identical, demands for methodological rigor. In particular, the importance of creating precise and measurable constructs was emphasized because such constructs are the foundation of powerful theory.

In contrast, the critique by Dyer and Wilkins argues that single cases are superior to multiple cases for creating high-quality theory. The authors refer to the caliber and the quantity of theory emerging from such cases. Further, the critique calls into question the need for methodological rigor, by challenging the appropriateness of a research focus, sampling, controls, and other trappings of rigor. They termed such methods *paradoxical* and

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hybrid. Yet, the authors' rationale is a curious one. It is not based on the inherent advantages of the loosely developed single case. Rather, the primary rationale is that the single case is superior because there are a number of classic case studies, especially prior to 1960, which apparently rest on one or two such cases

In this response, three critical questions are addressed. First, have the classic case studies actually generated substantially more and better theory than multiple-case research? Second, are the classic case studies truly single cases executed without concern for research focus, development of constructs, and so forth? Third, is "better stories versus better constructs" a false dichotomy? My overall conclusion is that the authors of the critique have seriously misread the classic case studies. Good storytelling may make these studies entertaining to read. But, consistent with "Building Theories from Multiple Case Study Research," their theoretical impact comes from rigorous method and multiple-case comparative logic

ARE SINGLE-CASE STUDIES SUPERIOR?

The authors of the critique argue that single-case research produces more and better theory than multiple-case research. To substantiate their position, they cite a number of studies, including Whyte (1943a), Selznick (1949), Gouldner (1954), Lipset, Trow, and Coleman (1956), Dalton (1959), and Kanter (1977), which they claim rest on one or sometimes two cases. What is not cited is the equally impressive list of classic multiple-setting studies These include Chandler's (1962) study of strategic and organizational change in American industry, Janis's (1972) treatise on presidential decision making, Lawrence and Lorsch's (1967) classic comparison of organizational structures and environments, and Kanter's (1983) study of corporate innovation and entrepreneurship

Has one of these lists had greater theoretical impact than the other? Is Whyte's work more important than Chandler's? Is Kanter's 1-company study, *Men and Women of the Corporation*, superior to her 10-company study, *The Changemasters*? Do we learn more from Selznick's study of the TVA than from Janis's accounts of the Marshall Plan, Cuban missile crisis, Bay of Pigs invasion, escalation in Vietnam, and Pearl Harbor? These are difficult, if not impossible, questions to answer However, it is thought provoking to consider whether, for example, Gouldner would have revealed more about bureaucracy if he had replicated and extended the insights of *Patterns of Industrial Bureaucracy* across multiple plants Would Whyte have developed a stronger and more complete theory of informal organization if he had visited more than one neighborhood or examined more than one ethnic group in *Street Corner Society*? Indeed, this line of questioning suggests that perhaps these classic case studies succeeded because of their fortuitous timing in the early stages of the field and the extraordinary skills of their authors, and in spite of the limitations of single-setting research

More important, a debate over numbers obscures an essential point. The concern is not whether two cases are better than one or four better than three. Rather, the appropriate number of cases depends upon how much is known and how much new information is likely to be learned from incremental cases (Thomas, 1990, personal communication).

ARE THE CLASSIC STUDIES LOOSE SINGLE CASES?

The authors of the critique suggest that the classic case studies rest on a single case developed with little concern for methodological rigor. This is a critical misreading of these studies. Although these studies may focus on a single setting such as a corporation, they are not single cases. Rather, many are multiple-case studies, relying on the comparative multiple-case logic of replication and extension for their theoretical insights.

For example, Whyte's (1943a) *Street Corner Society* details the informal social structure of corner gangs of young men. Although Whyte examined a single setting, Boston's North End, the study itself rests on insights and comparisons made across multiple gangs. Indeed, as Whyte wrote (1941. 648): "I made an intensive and detailed study of 5 gangs on the basis of personal observation, intimate acquaintance, and participation in their activities for an extended period of time."

More important, the use of multiple gangs is essential to Whyte's analysis. Many of his insights rest on replication of observations across groups. Some of these observations are repeated across gangs and thus lead to generalizations. For example, in prefacing his discussion of mutual obligations, Whyte wrote (1941: 658): "This system is substantially the same for all the groups on which I have information." He also noted other regularities based upon replication across corner gangs. For example, "Many corner gangs set aside the same night each week for some special activity, such as bowling"; "Most groups have a regular evening meeting-place aside from the corner"; and "The nuclei of most gangs can be traced back to early boyhood" (Whyte, 1943a: 255–256).

Other observations are disconfirmed across gangs and thus lead to the rejection of chance associations and, thereby, to elimination of erroneous conclusions. For example, based on evidence from multiple gangs, Whyte concluded that personality variables are likely to be of little use in characterizing leadership. He observed (1941: 661):

> One can find a great variety of personality traits among corner-boy leaders, just as one can among business or political leaders Some are aggressive in social contacts, and others appear almost retiring. Some are talkative, and others have little to say Few uniformities of this nature are to be found

Gouldner's (1954) *Patterns of Industrial Bureaucracy* captures the evolution of bureaucracy within a midwestern manufacturing plant. However, although Gouldner studied only one plant, his theoretical insights rely on multiple cases. For example, Gouldner developed his patterns of bureaucracy from a three-case comparison of no-smoking, safety, and bidding rules. As Gouldner wrote (1954: 182), "What could be done, however, was to examine several of the programs and rules within the plant and contrast them with *each other*, noting the variations that were thereby revealed."

Gouldner also used multiple cases to replicate insights. For example, the insight that a source of bureaucracy is management's belief that subordinates are failing to perform their role obligations arises from a two-case comparison of Old Doug, the previous plant manager, and Vincent Peele, his replacement. In Gouldner's words (1954: 233) "They (plant employees) overflowed with stories which highlighted the differences between the two managers, the leniency of Doug and the strictness of Peele." Gouldner replicated this insight with a second two case comparison, surface versus mine workers. He explained (1954: 233):

> On the more highly bureaucratized surface, supervisors tended to view their subordinates as unwilling to work and as ready to "goldbrick"; in the less (or mock) bureaucratized mine, supervisors viewed the miners as hard workers. Thus while only one plant was studied, the above hypothesis is supported, as are others, by observations of several discrete units of behavior.

Finally, Gouldner employed multiple cases to develop extensions, which thereby created more elaborate theory. For example, he used the surface and mine workers cases as the basis for hypothesizing that the bureaucratic process depends upon the resistance of those being bureaucratized. He observed (1954: 236):

> For the most part, these hypotheses stem from the contrast between the mine and surface. The miners, for example, tended to adhere to "traditional" values to a greater degree than the surfacemen, who were more easily adjusted to the rational and changing aspects of bureaucratic organization.

Perhaps the most striking example is Dalton's (1959) classic work, *Men Who Manage*. Although Dalton emphasized a single plant, his study relied heavily on the insights gained from three other organizations as well. He wrote (1959: 274):

> Many of the questions and hunches originating in the experience at Milo and Fruhling were cross-fertilized by concurrent contacts at Attica and Rambeau. Since no simultaneous systematic study could be made of all, and as Milo was the most accessible, that firm became the nucleus of inquiries and the continuing point of major effort. However, general questions and interpretations were increasingly influenced by study of the other firms, especially the factories. Common processes and similar recurring situations evoked interlocking questions which led to establishment of the problem areas.

The critique also seriously underestimates the degree to which the classic case studies rest on methodologically rigorous research designs. Consistent with "Building Theories from Case Study Research," the classic case studies rely on theoretical sampling, a priori measures of constructs, specification of research questions, multiple respondents, previous literature, and so forth Although the authors of the critique may find these methods paradoxical and hybrid, the authors of the classic case studies clearly recognized the close methodological relationship between theory-building research and its mirror, theory-testing research. Such methods are simply good science.

A good example is the classic case study by Lipset, Trow, and Coleman (1956) These authors examined democracy within the International Typographical Union (ITU). The objective of the study is clearly stated by the authors. Specifically, they are concerned with illuminating "the processes that help maintain democracy in the great society by studying the processes of democracy in the small society of the ITU" (1956. xi). The choice of the ITU is made for the explicit reason of theoretical sampling. Most unions are oligarchies, but the ITU is unique in that it maintains a democratic mode of governance. As the authors noted (1956: 1):

> There is, however, one trade union—the International Typographical Union (ITU), the organization of the men who set type in the print shops of North America—which does not fit this pattern. It is the only American trade union in which organized parties regularly oppose each other for election to the chief union posts, and in which a two-party system has been institutionalized

Thus, since the ITU apparently violates Michel's Iron Law of Oligarchy, it is an extraordinarily appropriate setting to explore the limits of oligarchy. Indeed, as the authors wrote (1956: 12):

> From the point of view of the further development of social research in the area of organizational structure, and indeed, the general expansion of our understanding of society, these deviant cases—cases which operate in ways not anticipated by theory—supply the most fruitful subjects for study.

The primary data source is a random sample of informants selected from the population of New York printers. The authors employed a two-stage stratified random sampling procedure to select these informants. Lipset and his colleagues also went to great lengths to develop data-collection instruments and measures Exploratory interviews preceded the development of an interview schedule consisting of 93 predominantly closed-end questions. This schedule was designed to obtain quantitative data on constructs such as liberalism/conservatism, ideological sensitivity, and knowledge of union political issues. In addition, the authors anticipated that these interviews would also reveal unexpected relationships and qualitative information. Finally, the authors employed a mailed questionnaire to develop a second panel of data The resulting theoretical framework reflects the skillful blending of quantitative and qualitative information plus extensive comparison with existing literature, particularly concerning oligarchy

Kanter's case study, *Men and Women of the Corporation*, also illustrates rigorous methodology. She articulated a specific research objective: "the ways in which organization structure forms people's sense of themselves and of their possibilities" (1977 3) She employed a variety of methods, which permitted her to mix qualitative and quantitative data and to blend the insights of multiple informants Her methods included: (a) a mail survey of 205 sales workers and managers, including a 34-item measure of commitment, (b) interviews with the first 20 women to enter the sales force at Incsco, (c) a survey of 111 nonexempt employees on attitudes toward promotion, (d) content analysis of 100 performance appraisal forms, (e) transcriptions of group discussions among mixed-gender groups and among husbands and wives, and (f) participant observation of meetings As Kanter explained (1977 397) "I used each source of data, and each informant, as a check against the others In this way, consistent tendencies could be noted. Nothing that I report was totally unique or true of only one person."

Kanter's research design also indicates sensitivity to other methodological concerns She measured constructs developed both a priori and as a result of her research Further, Kanter also relied heavily upon existing literature, "an extensive review of the sociological, social psychological, psychological, and organizational behavior literatures" (1977 298). She described iterations between theory and data Finally, Kanter's work involves careful specification and measurement of constructs such as those describing different proportional representations of kinds of people, including uniform, skewed, tilted, and balanced, and the linkage of those constructs with variables such as power and opportunity to form hypotheses.

IS "BETTER STORIES VERSUS BETTER CONSTRUCTS" A FALSE DICHOTOMY?

The critique poses a dichotomy between better stories and better constructs As an example of this dichotomy, an article on the politics of strategic decision making (Eisenhardt & Bourgeois, 1988) is compared with Dalton's (1959) *Men Who Manage* Presumably, the article emphasizes constructs, whereas Dalton's work illustrates stories. Unfortunately, comparison of story detail and breadth of research questions between a 30-page journal article and a 300-page book is not helpful As the authors of the critique note, it is not surprising that a book would offer richer insight than an article. A more reasonable comparison would pit Dalton's book against a series of articles (e.g , Bourgeois and I have written several related articles), or, better yet, a multiple-case classic (e g , Chandler, 1962, Janis, 1972). But more important, the comparison does a disservice to Dalton's work. That research does an extraordinary job of combining both good stories and good constructs.

The problem of the critique lies in its misspecification of the dichotomy. The trade-off between stories and constructs is not principally related to whether the study is single- or multiple-case, as the critique would have us believe. Rather, the trade-off arises from page limits and editorial constraints. Research that must fit into the page limit of a journal article is necessarily limited in scope and story detail. Moreover, journal editors usually prefer that such research be written around theory, not stories. Thus, researchers may understand the context, but only have space to describe the theory and not the context of social dynamics that generated that theory. In contrast, the authors of books have much greater editorial freedom and space to present both better stories and better constructs, regardless of whether their studies are single- or multiple-case Therefore, for example, Whyte's journal articles such as "Corner Boys. A Study of Clique Behavior" (1941) and "A Slum Sex Code" (1943b) are much more similar in scope and story detail to *Politics of Strategic Decision Making* (Eisenhardt & Bourgeois, 1988) than to his book, *Street Corner Society* (1943a).

Second, although the critique argues that better stories yield better theories, the authors fail to mention the rather ironic point that the striking impact of stories is usually considered to be an example of cognitive bias. Individuals inappropriately overweigh the information value of a story as compared with more abstract data. So, contextually rich stories lure people into thinking that they know more than they do (Nisbett & Ross, 1980). This hardly seems to be a scientific advantage. And, it certainly seems unrelated to building great theory. If anything, this overvaluing of a story suggests that the resulting theory may well be particularly distorted and inaccurate.

CONCLUSION

The critique has important flaws. First, the claim that the classic case studies cited have had much greater theoretical impact than multiple-setting studies is difficult, if not impossible, to substantiate. Second and more important, many classic case studies (a) are fundamentally multiple-case studies; (b) employ the comparative multiple-case logic of replication and extension to develop theoretical insight; and (c) rest on rigorous methods, including specification of research issues, sampling, measurement of constructs, and controls Thus, the classic case studies are quite consistent with more recent case studies and with the approach described in "Building Theories from Case Study Research." Finally, the authors err in attributing the theoretical contributions of the classic case studies to good storytelling. In fact, nowhere do the authors tell us how great contextual description is translated into great theory. Rather, storytelling is a wonderful and necessary first step as well as a terrific way to persuade and entertain readers. So, yes, rich background context is important. But, the theoretical insights of case studies arise from methodological rigor and multiple-case comparative logic. My concern is not that the critique is wrong, but rather, that if we take the advice too seriously, then we will end up writing interesting stories, but creating little in the way of generalizable theory.

Of course, there are differences between many of the recent multiple-case journal articles and the classic case studies. Given page constraints and editorial demands, the former do often emphasize theory, whereas the latter have the space and editorial freedom to convey rich construct descriptions, characterizations of the setting, and stories. The latter also sometimes emphasize certain cases over others. But, as my colleague and I have recently discovered while conducting our own single-setting research (Eisenhardt & Brown, 1990), the similarities between single- and multiple-setting research are vastly more important than the differences. For both, good storytelling is an essential first step, but good theory is fundamentally the result of rigorous methodology and comparative, multiple-case logic. This is as evident in the classic case studies as it is in contemporary multiple-case research.

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