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FURTHER REMARKS ON RETROSPECTIVE ACCOUNTS IN ORGANIZATIONAL AND STRATEGIC MANAGEMENT RESEARCH

BRIAN R. GOLDEN
University of Western Ontario

Miller, Cardinal, and Glick (1997) challenged the conclusions in Golden (1992b), which examined the use of retrospective data in strategic management research. Further, the authors suggest that my findings have led many other researchers to avoid the use of retrospective data. The present note suggests recent researchers have not entirely avoided retrospective data but have perhaps added precautions when using such data. I also suggest that Miller and colleagues did not fully represent important aspects of my research and inappropriately compare my study with others.

In earlier research (Golden, 1992b), I attempted to address two questions in the study of strategic and organizational change: (1) To what extent do CEOs accurately recall and report their firms' past strategies? and (2) Under what conditions are retrospective accounts the most and the least accurate? These questions were viewed as important since retrospective data, frequently the only data available to researchers interested in past strategy, are frequently used, and compelling theory-based arguments in the psychology and organizations literatures suggest that these data may be subject to significant biases. In general, my earlier study concluded that "the use of retrospective accounts in management research needs to be seriously questioned" (Golden, 1992b: 857). Further, it began to identify the specific conditions under which the use of retrospective data may be the most and the least appropriate. These findings and conclusions have recently been challenged by Miller, Cardinal, and Glick (1997), who attempted to show that my study has perhaps been "overinterpreted" by others. In pursuit of their goal, however, I believe they have not accurately characterized certain aspects of my research and how it has been interpreted. Thus, I suggest in the following pages that despite the contribution their paper makes, several (though not all) of their criticisms of my earlier study are questionable. My discussion begins with a brief summary of my earlier conclusions and how they seem to have impacted subsequent research. A response to several concerns raised by Miller and colleagues follows, and I conclude with comments about future research that may rely on retrospective accounts.

THE IMPACT OF "THE PAST IS THE PAST—OR IS IT?"

Berger and Luckman suggested that a sociology of knowledge must address not only the empirical reality of knowledge in human societies, but



also "the processes by which any body of 'knowledge' comes to be socially established as reality" (Berger & Luckman, 1966: 3). With this idea in mind, I must acknowledge the possibility that my intended message in Golden (1992b) was not interpreted by readers completely as I had hoped it would be. This seems to be a fundamental concern of Miller, Cardinal, and Glick (1997), who suggested that the "reality" of my earlier study may have been interpreted as a call for universal prohibition of retrospective data. In particular, Miller and colleagues suggested that I characterized retrospective reports as dangerous and to be avoided or treated with extreme caution. Although I did take the strong position that these data are subject to substantial error under certain conditions, my purpose was to investigate when retrospective data would likely be the most and the least valid.

Specifically, Golden (1992b) examined the correspondence between two measures of strategy obtained by the CEOs of 259 hospitals. The time 1 measure categorized the hospitals' strategies using the familiar Miles and Snow (1978) typology. Hambrick (1981), Snow and Hrebiniak (1980), and Zajac and Shortell (1989) all adopted this measure, and Shortell and Zajac (1990) validated it in a study published in this journal. Two years after the initial strategy measure was obtained (i.e., at time 2), the CEOs who had reported their hospitals' strategies two years earlier (at time 1) were asked to categorize those past strategies. In theory, the "retrospective measure" obtained at time 2 could only be considered valid if it was reliable. And this measure could only be considered reliable if there was generally strong correspondence between the time 1 and time 2 categorizations, which were measures of the same organizational phenomenon (the time 1 strategies). From my analysis, I concluded that "retrospective errors may be pervasive; nearly 60 percent of the retrospective accounts studied here did not agree with validated reports elicited only two years earlier" (Golden, 1992b: 852).

In a second stage of my analysis, I investigated whether the retrospective errors occurred systematically or randomly. Drawing on a well-established literature on the presentation of self (Feldman & March, 1981; Salancik & Meindl, 1984), people's subconscious attempts to maintain self-esteem (Nisbett & Wilson, 1977), and hindsight biases (Fischhoff & Beyth, 1975), I examined the theoretically predictable correlates of disagreements between the time 1 and time 2 accounts. Several of my theory-based predictions were supported, thus suggesting that the disagreements between the time 1 and time 2 accounts were due to systematic retrospective errors. Ironically, my findings were generally consistent with subsequently published remarks of two of the three authors of Miller et al. (1997). They too recognized the potential weakness of retrospective data. Specifically, Glick and his colleagues, in their discussion of open-ended retrospective accounts, noted this:

A second disadvantage of asking open-ended questions about recent changes is that the responses may be associated with errors of recall, e.g., informants may selectively neglect some

events that are important or focus on trends that are actually unimportant but temporarily conspicuous to the informant. . . . Errors of recall can result from strong cognitive processes such as rationalization, self-presentation, simplification, attribution, or simple lapses of memory (Glick, Huber, Miller, Doty, & Sutcliffe, 1990: 302).

In my recent attempt to understand the socially constructed reality that has emerged around my earlier research, I examined each of the approximately 20 manuscripts that cited Golden (1992b) in the years 1993 to 1995, inclusive. My goal was to determine whether my conclusions had been interpreted as an unequivocal criticism of retrospective data. To my relief, my conclusions seem to have been interpreted as I intended them to be; in general, and as a consequence of my findings, others seem to have taken added precautions when using retrospective data. Although a full review of these publications is beyond the scope of the present discussion, several of them illustrate the influence of my 1992 work. For example, Hambrick and Abrahamson (1995) avoided the potential for self-serving retrospective biases by obtaining data from industry experts. Phan and Hill (1995) recognized the possibility of retrospective errors in their study and adapted their research by accessing data from multiple informants. Brockner and his co-authors (Brockner, Konovsky, Cooper-Schneider, Folger, Martin, & Bies, 1995) suggested, in view of my findings, that their research would have been subject to measurement errors attributable to self-serving biases and recall errors had it not been for their additional precautions. In order to strengthen their confidence in their retrospective data, these authors collected other, nonretrospective data as well. And in a final illustration, Chen, Farth, and MacMillan (1993) altered the structure of their data collection in order to minimize the likelihood that the CEOs in their survey would provide self-serving retrospective accounts.

My review of the published works that have cited Golden (1992b) has led me to an interesting, though tentative, conclusion. Each author or set of authors, having cited my findings and cautions about the use of retrospective data, nevertheless presented research using retrospective data. However, all of these authors took precautions against retrospective biases and errors (e.g., using multiple informants, informants who did not have personal stakes in the phenomena under study, and multiple data sources and types) in order to validate their retrospective measures. As a consequence, these authors ruled out some potential alternative explanations for their findings (e.g., self-serving presentations) and thus made more compelling cases for the integrity of their studies. Of course, it is also possible that other researchers have not been able to implement such extra precautions. One may then ask how much research has not been conducted (or published) because of the cautions articulated in Golden (1992b). The answer to this question is largely unknowable. However, it seems that if researchers chose not to conduct research because of their judgments that their retrospective data would be flawed or could not be validated, then they may have chosen wisely. And if

reviewers and editors chose not to publish a particular study because of such concerns, it would seem that the design of the research raised these concerns—though possible weaknesses were perhaps made more salient by the publication of my findings.

A RESPONSE TO MILLER, CARDINAL, AND GLICK

Miller and colleagues (1997), in their critique of my research, raised issues that necessitate a direct response. These issues concern what these authors referred to as my use of an “overly pessimistic statistic” to assess accuracy, use of a weak strategy measure, and failure to separate unreliability (i.e., measurement error) from systematic informant fallibility. I address these interrelated issues in this section.

A primary concern of Miller et al. (1997) is that the contemporaneous strategy measure used in Golden (1992b)—the measure of time 1 strategy elicited at time 1—was of questionable reliability and validity. This is a critical issue since, if the contemporaneous measure was of dubious quality, one cannot conclude that the inconsistency between the time 1 measure and the retrospective time 2 measure was largely (although not entirely) due to retrospective errors. To make this argument, I relied on the validation efforts of Shortell and Zajac (1990). Using archival data to validate their measure of the Miles and Snow (1978) strategy types, they found support for 24 of their 26 tests of discriminant validity. Each of their predictions was based on the theoretical propositions Miles and Snow (1978) suggested. Since I drew my sample of hospitals from the Shortell and Zajac data set, and since I used the same measure as those authors, I suggested that the disagreements between the time 1 and time 2 measures were largely due to retrospective errors. In addition, I conducted validation tests of the retrospective measure (see footnote 2, Golden [1992b]). As predicted, validity could not be established for the retrospective measure but, as in Shortell and Zajac (1990), it could be established for the contemporaneous measure.

Miller and colleagues did not adequately describe the efforts Shortell and Zajac took to establish the validity of the Miles and Snow measure. First, Miller et al. indicated that “24 out of 25 convergent validity coefficients were significant or approaching significance” (1997: 192).¹ This characterization is misleading since the reader cannot determine how many of the 26 tests were statistically significant at commonly acceptable levels. In fact, 22 of the 26 predictions were supported at the .05 level or better, and 3 of the 26 were supported at the .10 level; all were in the predicted direction. Miller, Cardinal, and Glick also did not indicate the breadth of Shortell and Zajac’s validation efforts. Specifically, they did not report that Shortell and Zajac relied not only on perceptual, self-report data, but also on multifaceted archival data examining product development activities, market research intensity, and administrative activities, among other possible reflections of the

¹ Miller et al. erroneously referred to 25 coefficients, but Shortell and Zajac refer to 26.

Miles and Snow types. It should also be noted that this measure and these data have been used in other peer-reviewed, published manuscripts (cf. Golden, 1992a; Veliyath & Shortell, 1993; Zajac & Shortell, 1989). The contemporaneous strategy measure in these studies was statistically associated with the predicted independent or dependent variables. If this measure and these data were unacceptably "weak," as Miller et al. suggested they were, then it is difficult to explain support for the theory-based predictions of these authors. Lastly, Miller and colleagues reported the questionable test-retest reliability results reported in Shortell and Zajac. However, Miller et al. failed to indicate that the intertemporal reliability tests of Shortell and Zajac were not intended to be conclusive. In contrast to Shortell and Zajac's extensive validation efforts, which were conducted with a sample of over 400 CEOs, their preliminary test-retest analysis for intertemporal reliability was based on phone interviews with only 19 CEOs—approximately 4 percent of the full sample. Thus, readers must be cautious about interpreting Miller and colleagues' conclusion that this test-retest result indicates weak reliability.

In an effort to directly critique my findings, Miller, Cardinal, and Glick (1997) also raised the possibility that a sizable proportion of the disagreements between the time 1 and 2 measures I observed may have been due to chance rather than to systematic error. Certainly this may partially explain my findings. In order to address this possibility, Miller and colleagues conducted a form of sensitivity analysis. In doing so, they calculated the agreement coefficient for the time 1 and 2 measures to be .42. These data implied that I had concluded insufficient agreement. After making appropriate adjustments for the possibility of chance agreement (using the four-category strategy typology of prospector, analyzer, defender, and reactor), Miller et al. calculated a new agreement coefficient of .48—a value still below conventional standards of agreement. Even after the reactor category was omitted from their reanalysis, their agreement coefficient only increased to .53; had I earlier omitted this strategy category, the comparable figure for my study would have been .47. Although Miller et al. may have rightly concluded that adjusting for chance agreement improves one's confidence in these retrospective reports, the improvement does not appear sufficient to conclude that most of the mismatches were the result of chance.

Further, and of greatest concern, Miller and colleagues (1997) attempted to challenge my conclusions indirectly by using questionable comparisons to other studies. Central to their criticisms was their examination of the unpublished doctoral dissertation of Fox (1992).² Fox collected both retrospective and nonretrospective data using a strategy measure developed by Glick et al. (1990). Reinterpreting Fox's data, Miller and coauthors reported that CEO rater reliability in her study was "remarkably similar for both retrospective and nonretrospective reports. . . . Contrary to arguments about retrospective error, CEO rater reliability was not even slightly lower for

² I was unsuccessful in reaching Fox in an effort to receive a copy of her dissertation.

retrospective reports than for nonretrospective reports (Miller et al., 1997: 197–198). From their reanalysis of Fox's (1992) data, these authors concluded that "this finding clearly suggests that most of the error in the Golden study was caused not by faulty retrospective thinking but by the measure itself" (Miller et al., 1997: 198).

Fox's (1992) methodology and sample differ substantially from that in Golden (1992b), and thus, the Fox study cannot inform us about my earlier research. First, my study examined the retrospective reports of CEOs and elicited data about their own organizations. Fox (1992) collected data from 31 financial and banking experts in a small metropolitan community. Only 4 of these 31 raters were bank employees (1 CEO and 3 senior officers); 3 raters were professors at a local university. The choice of these raters makes comparisons to Golden (1992b) inappropriate. Specifically, I argued in the 1992 publication that self-serving biases and efforts to present a positive self-image would lead CEOs to distort accounts of past strategy contingent on, among other things, past firm performance—for instance, CEOs of poorly performing firms would be most likely to distort accounts of past strategy. Since only 4 of Fox's 31 raters might be considered to have had roles in a bank's strategy, her sample does not allow for comparisons with Golden (1992b).

Second, Fox (1992) relied on a strategy measure that differed substantially from mine, which was a single, validated, seven-item scale. As in previous published research, the four Miles and Snow (1978) strategy types were arrayed along a continuum, with defender at one end and prospector at the other. In contrast, Fox's (1992) measure was based on four 7-point scales that indicated the extent to which each of the banks exhibited characteristics of each of the Miles and Snow (1978) four strategic types. Without questioning the validity of this technique, I suggest that the differences between the Fox (1992) and Golden (1992b) strategy measures are so great as to make them incomparable.

Third, Miller, Cardinal, and Glick (1997) relied on the questionable logic that since *both* of Fox's (1992) measures exhibited equally low reliability, it is therefore appropriate to conclude that the low reliability of the retrospective measure was not due to systematic biases or more generally, to the time elapsed between measurements. However, in contrast to Golden (1992b), in which the validity of the contemporaneous strategy measure had been validated, Fox (1992) did not establish the validity of the contemporaneous strategy measure. Overall, then, the reanalysis of the Fox data and the comparison with my study could only have been appropriate if three conditions had been met: (1) if Fox had employed a strategy measure similar to mine (it was substantially different); (2) if similar informants were relied upon (Fox's informants were substantially different, and would not have been likely to make the kinds of retrospective errors I predicted for my informants); and (3) if either of Fox's two measures exhibited acceptable validity according to conventional standards (neither did, as Miller et al. indicated).

In addition to examining Fox's (1992) dissertation, Miller and coauthors made several other questionable comparisons between my study and others. In an effort to justify these comparisons, they indicated that "these studies used the same *basic measure* of strategy as Golden (1992) and raters who seem to have been knowledgeable about the focal organizations, and they assessed current, not retrospective, strategy" (Miller et al., 1997: 194; emphasis added). However, their characterization of these other studies is inaccurate; in fact, numerous substantive differences between my study and those they cited make comparisons inappropriate. For instance, unlike my study, Meyer (1979) collected strategy data from industry experts, not hospital CEOs. The problem with comparing expert informants to CEO informants was discussed above. In short, I would not expect industry experts to be subject to the retrospective biases I predicted in Golden (1992b), and thus, this cross-study comparison is misleading.

The comparison with Coleman (1978) is also inappropriate. Coleman obtained an interrater agreement measure of only 39 percent. However, his respondents also differed substantially from those used in my study. In Golden (1992b), I relied on each CEO to report his or her own firm's strategy; in contrast, Coleman elicited CEOs' ratings of *other firms*. For instance, he examined the level of agreement between CEO A's rating of hospital C's strategy and CEO B's rating of hospital C's strategy. Because it was not established that CEO A and CEO B ought to be able to reliably and validly report the strategy of hospital C, and because Coleman's measure differs dramatically from that in Golden (1992b), comparisons with my findings are not justified.

The comparison of Hambrick (1981) with Golden (1992b) is similarly misleading. Hambrick, who conducted his research in three industries, reported a concordance coefficient of .76 for the hospital industry—one of the industries initially studied by Miles and Snow (1978) when they formulated their strategy typology. Miller and colleagues did not report this concordance coefficient. Instead, they reported a concordance coefficient of .65, which is the average concordance coefficient obtained from the three industry studies. Miller et al. failed to indicate, however, that the concordance coefficient for the college "industry" reached only .49 and that Hambrick himself noted that it might not be appropriate to apply the Miles and Snow typology to colleges. Thus, it seems that had they contrasted Hambrick's results for only the hospital industry to those of my sample organizations, which were also in the hospital industry, Miller and colleagues (1997) might have come to a more positive characterization of my strategy measure. Finally, a fourth study Miller et al. used for comparison purposes employed a 20-item strategy measure (Conant, Mokwa, & Varadarajan, 1990). Not only is it problematic to draw comparisons between single- and multi-item measures, but also, Conant and his colleagues elicited their data from marketing directors, not CEOs. Hambrick (1981) earlier found that CEOs tended to provide the most valid accounts of their firms' strategies.

FINAL COMMENTS

Miller, Cardinal, and Glick (1997) sought to critique Golden (1992b) in part by way of indirect comparisons with other studies. A more direct approach, and an attractive model for the organizational sciences, can be seen in Latham, Erez, and Locke (1988), which documents conflicts between the conclusions of Latham's (1992) research on goal setting and those of Erez (e.g., Erez & Arad, 1986). Rather than searching for flaws in each other's research, Latham and Erez pursued the constructivism approach to theory building suggested by McGuire (1980). With Locke serving as a mediator, Latham and Erez conducted a series of experiments explicitly designed to reconcile their conflicting findings and conclusions. In his commentary about the process, Locke noted the substantial effects on findings and conclusions of subtle procedural and design differences among past goal-setting studies (Latham, 1992). As a consequence of their approach, Latham and colleagues were able to better identify boundary conditions of goal-setting theory and, more generally, to provide the field with a "relatively sane way of resolving a scientific dispute" (Latham, 1992: 153). In light of these observations and my remarks above, the approach presented by Latham et al. (1988) appears to offer a most constructive and convincing mode of debate in the organizational sciences.

Future research that indicates under what conditions retrospective data can be used should be designed. In addition, future researchers may also benefit from research such as the work by Mantwill, Kohnken, and Ascher-mann (1995), who discussed "investigative interviewing" techniques developed in the field of eyewitness psychology. The premise of this form of "cognitive interview" is that recall problems are not necessarily caused by loss of information, but rather, are often caused by inaccessibility, which can be overcome. Bergh (1993) also provided an important discussion of predictable time-induced effects in management research. And Mitchell and Thompson (1994) presented a model for improving recall of past "events" by focusing on "aspects"—for instance, weather, fish, and food are aspects of a fishing event. An analogue in strategic management research would be examination of aspects, such as downsizing, outsourcing, resource allocations, marketing programs, and mergers and acquisitions, that comprise the strategy of a firm.

In conclusion, it is important to note that Miller, Cardinal, and Glick (1997) is limited in scope to questioning Golden's (1992b) conclusions and does not provide positive support for the use of retrospective data. And interestingly, Golden (1992b) and Miller et al. (1997) ultimately converge, both making the same point—that, under certain specifiable conditions, retrospective data may be used in management research. To this end, Miller and colleagues offered several prescriptions about the use of data and their necessary validation, and these prescriptions are highly consistent with those presented by me and by others (cf. Huber & Power, 1985). These prescriptions are important, and given the substantial evidence in psychology

and management concerning the *potential* for retrospective biases and errors, they are hardly controversial. Thus, on the basis of my research as well as the recent findings of Miller and colleagues (1997), I maintain my suggestion that future researchers be critical of retrospective data. However, *if* significant efforts are made to minimize retrospective biases and error *and* these data can be validated, retrospective data may well provide unique access to past organizational events.

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Brian R. Golden is an assistant professor of management at the Richard Ivey School of Business, the University of Western Ontario. He received his Ph.D. in organization behavior from Northwestern University. His research interests include managerial cognition, the implementation of strategic change, and interunit knowledge transfer and coordination in diversified firms.