Adequacy of Reporting Results of School Surveys and Nonresponse Effects: A Review of the Literature and a Case Study

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To ensure accurate interpretation of research findings, researchers should report details about their research design, data collection method, and response rates when presenting findings from survey research. A review of 100 peer-reviewed articles reporting the results of survey research on K–12 schools with principals as the designated respondents revealed that such information is often not reported. Few studies examined or even acknowledged the potentially biasing effects of nonresponse. A mail survey of 470 schools in California, which yielded a response rate of 33% (157/470), is used as a case study to evaluate the pattern of nonresponse and its effects on univariate and multivariate statistics. Consistent with prior research, nonresponse produced bias in univariate estimates, but associations between variables were robust and not affected.

Keywords: data analysis; nonresponse; survey research; writing

In survey research it is important to design the study with the intent of collecting and reporting information about nonresponse and its effects on survey results. Nonresponse may affect the validity of the findings, especially their external validity, or the extent to which they can be generalized to the population of interest. Surprisingly, recent reviews of survey literature in the social sciences report that it is not the norm to mention nonresponse error as a threat to external validity or to perform nonresponse analyses (Dooley & Lindner, 2003; Lindner, Murphy, & Briers, 2001; Werner, Praxedes, & Kim, 2007). For example, Dooley and Lindner found that about 60% of reviewed articles did not mention nonresponse error as a possible threat to the external validity of the study; 83% made no attempt to analyze nonresponse error. The issues of survey nonresponse, reporting of survey methods and response rates, and evaluation of the potentially biasing effect of nonresponse are relevant for education research, given that surveys are widely used in the field.

Education researchers often use questionnaire surveys to collect data. Questionnaires can be administered by mail, Internet, or telephone or in person. The unit of analysis can be individuals, such as students, teachers, and administrators, or organizations, such as schools or school districts. Data can be collected either from entire populations or from samples of the population of interest.

Many factors combine to determine the quality of a study. These include characteristics of the questionnaire or data collection instrument, characteristics of the proposed population or sample, appropriateness of the analytical procedures, the extent to which the persons or organizations targeted for the study actually participate, and the extent to which the information they provide is complete. To evaluate findings reported in peer-reviewed journals and other venues, readers look for complete descriptions of the proposed and actual methods used in collecting the data. Authors should provide enough information to enable readers to interpret the findings correctly and, if they choose, to replicate the study.

This article has three objectives: (1) to briefly review methods commonly used to evaluate whether nonresponse affects survey data and the implications of the data; (2) to report the results of our literature review of peer-reviewed studies involving surveys of K–12 school principals, in which we examined the extent to which survey methods and results were comprehensively and accurately reported; and (3) to present a case study of a survey of principals of California schools to demonstrate how nonresponse patterns and their potentially biasing effects can be assessed.

Evaluating Nonresponse Bias

Nonresponse can be either random or nonrandom. Both kinds can affect the internal and external validity of study findings, but nonrandom nonresponse is of greater concern than random nonresponse. The bias created by nonresponse is a function of both the level of nonresponse and the extent to which nonrespondents are different from respondents. If, for example, a questionnaire is mailed to all school principals in a particular state for research on the predictors of academic achievement, a 30% response rate with schools of varying levels of academic achievement responding (i.e., random nonresponse in terms of academic achievement) may cause less bias in the research-relevant outcomes than a 50% response rate with only the schools ranking in the top 50% in the state in academic achievement responding (i.e., nonrandom nonresponse in terms of academic achievement). If nonresponse were random in both cases, the magnitude of nonresponse bias affecting research outcomes might be quite similar. Hence increasing response rates in survey research may not necessarily reduce bias or produce vastly different study results (Curtin, Presser, & Singer, 2000; Groves, 2006;

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Groves, Presser, & Dipko, 2004; Keeter, Miller, Kohut, Groves, & Presser, 2000; Teitler, Reichman, & Sprachman, 2003). Rather, it is important to always evaluate both the response rate and the pattern of nonresponse and to determine how they might affect the interpretation of results.

There are a number of ways to evaluate patterns of nonresponse. At a minimum, response rates should be reported and their implications for the interpretation of results should be discussed. Four methods of assessing patterns of nonresponse are commonly used.

Univariate comparisons between respondents and nonrespondents. The first approach is to perform univariate comparisons between respondents and nonrespondents on independent variables that are known a priori from sources other than the survey (e.g., demographic data available in a public database). This method is referred to as the archival approach (Rogelberg & Luong, 1998). It offers some insight into how respondents and nonrespondents compare. It does not provide information, however, about whether or how *dependent* variables differ between respondents and nonrespondents. The implicit assumption is that differences in independent variables are associated with differences in dependent variables. Variations of this approach include comparing independent variables between early and late respondents, between high- and low-effort respondents (where *effort* is defined as the amount of follow-up needed to obtain a response), or between respondents and the population of interest.

Multivariate regression analyses. A second method is to conduct multivariate regression analyses to identify predictors of survey response. As in the first method, data available from other sources about respondents and nonrespondents are used to analyze survey response. This is a more statistically sophisticated method of identifying differences between respondents and nonrespondents in terms of independent variables. The results of this kind of analysis can be used to develop data weights that will adjust for the differential probability of survey response. The inverse of the probability of response can be assigned as weights, such that schools with characteristics correlated with nonresponse receive larger weights than schools with characteristics correlated with response. This method minimizes nonresponse bias, conditional on covariates known for both respondents and nonrespondents (McGuigan, Ellickson, Hays, & Bell, 1997).

Regression models can be extended to include analysis of the predictors of both survey response and research-relevant outcomes. This can be achieved by using data available from other sources to first analyze the likelihood of survey response and then, using survey data obtained from the respondents, analyze the predictors of research outcomes. The strength of this approach is that it is a more direct assessment of the effects of nonresponse, because the variables that are associated with both survey response and outcome measures are those that are most likely to bias the data (Johnson, Holbrook, Ik Cho, & Bossarte, 2006). For example, if women are more likely than men to respond to a survey on child care practices, and if they are also more likely than men to be the primary caregiver, then the distribution of primary caregivers will be biased (i.e., overrepresented) in the survey data. If, on the other hand, the outcome of interest was the respondent's birth order, the fact that women were more likely to respond to the survey would not necessarily bias the outcome because gender is not associated with birth order. The limitation of this method of evaluating potential bias is that it assumes that associations between independent and dependent variables observed among respondents apply in the same way to nonrespondents. This assumption cannot be directly tested because dependent variables are not observed for nonrespondents.

Wave analyses. A third approach is to assume that late respondents and respondents who required more follow-up effort (i.e., high-effort respondents) share characteristics with nonrespondents, and to compare them with early or low-effort respondents on dependent variables (e.g., Curtin et al., 2000). This method is called *wave analysis* (Rogelberg & Luong, 1998). It provides insight into differences between respondents and nonrespondents in terms of research-relevant dependent measures, assuming that late/high-effort respondents are reasonable proxies for nonrespondents.

Random follow-up interviews. A fourth strategy is to conduct follow-up telephone interviews with a random sample of nonrespondents, typically using an abridged version of the original survey questionnaire. This is the most direct method of comparing respondents with nonrespondents on substantive variables. However, this approach can be used only if a survey is not completely anonymous, such that respondents and nonrespondents can be identified; and it assumes that nonrespondents to the original survey will agree to be interviewed by telephone. It also assumes that those who respond to the follow-up survey are representative of all nonrespondents to the original survey.

Survey researchers routinely use only a single method, if any, to evaluate the effects of nonresponse bias in their data. The choice of statistical method can affect the magnitude of the bias estimate because each method relies on a different assumption (e.g., that late/high-effort respondents are reasonable proxies for nonrespondents). To obtain a robust and balanced assessment, the researcher should use more than one method to examine the effects of survey response and evaluate the validity of underlying assumptions.

Review of How Survey Results Are Reported in Education Research

The purpose of our literature review was to assess the extent to which survey respondents, sampling procedures, and data characteristics are reported adequately in scientific articles that report the results of survey research involving K–12 school principals.

Method

We conducted literature searches in multiple article databases: Education Full Text, ERIC, JSTOR, Social Sciences Citation Index, Social Services Abstracts, and PsycINFO. For logistical reasons, we restricted the review to survey research in which the unit of analysis was the school and the designated respondents were school principals. The initial screening criteria were that, to be included in the review, an article needed to (a) be original work;

(b) include the keywords "school," "principal," and "survey" in the abstract (or in the "topic" in the case of the Social Sciences Citation Index); (c) have been published between 2000 and 2007; and (d) be written in English. The additional restriction of searching only in "educational journals" was added when searching in JSTOR. A total of 570 articles met these criteria.

We then reviewed each article individually to exclude duplicate articles (n = 160), research conducted outside the United States (n = 130), nonempirical articles or surveys conducted with 10 or fewer school principals (n = 137), and articles published in journals that were not peer reviewed (n = 43). We determined whether a journal was peer reviewed, or refereed, on the basis of its classification in *Ulrich's Periodicals Directory* (Serials Solutions, 2008). If a journal was not listed in the directory, we examined the journal's review policies to make the determination. After screening we were left with a total of 100 articles to be reviewed. The articles were selected from 68 journals, representing a very broad range among established journals in the education field. For example, journals from which three or more articles were selected included *Education; Educational Administration Quarterly; ERS Spectrum; Journal of School Health; NASSP Bulletin;* and *Rural Educator*.

We reviewed each article's full text to identify characteristics of the study. Some articles reported on multiple surveys, not all of which were performed with school principals; in those cases, only the sections pertaining to the school principals' survey were reviewed. We reviewed each article to determine whether the study used primary or secondary survey data, whether it was a population or sample survey, how the study was designed, and which mode of survey administration was used. Next, we coded each article to indicate whether it reported the survey's population characteristics, population size, response rate, pattern and effects of nonresponse, and use of weights to adjust for response probability. Studies that surveyed a sample rather than the entire population were coded for whether the sampling method and sample size were reported. Studies that used probability samples were coded for whether the article described the use of weights to adjust for selection probability. Frequency distributions were calculated for each of these variables.

Results

The characteristics of the surveys reported in the reviewed articles are summarized in Table 1. Of all the reviewed studies (N = 100), more than 90% involved primary data analysis and used a cross-sectional design. Two thirds (62%) of the studies surveyed a sample of the population of interest, 30% surveyed the entire population, and 8% did not report whether the population or a sample had been surveyed. Of the studies that surveyed a sample (n = 62), 63% (39/62) used a probability sample; others used convenience or purposive samples or did not specify the nature of their samples. By far, the most common mode of survey administration across all studies was by postal mail (61%). Twenty-one percent of the studies, however, did not describe the mode of administration. Several of these studies stated that survey questionnaires were "sent" to study participants but did not describe how they were sent.

Results of the literature review are presented in Table 2. Although 92% of all studies described the population of interest, only half (49%) of them reported the size of the population or the sampling frame. Of the studies that involved sampling (n = 62), 77% (48/62) described the sampling method and 87% (54/62)

Table 1
Descriptive Characteristics of Reviewed Literature (N = 100)

Study Attribute	Classification of Attribute	%
Data source	Primary data	93
	Secondary data	7
Study design	Cross-sectional	95
	Longitudinal[a]	4
	Not reported	1
Surveyed group	Population	30
	Sample	62
	Probability sample	39
	Nonprobability sample, or sample not described	23
	Not reported	8
Administration mode	Postal mail	61
	Telephone	6
	Internet or e-mail	3
	Other[b]	2
	Combination of modes[c]	7
	Not reported	21

[a]Longitudinal designs include panel studies and repeated cross-sectional studies.
[b]Other administration modes include on-site, paper-based administration and in-person interviews.
[c]The most common combination was postal mail and Internet.

Table 2
Frequency Distributions of Information Reported About Survey Method and Data in Reviewed Articles (N = 100)

Type of Information Reported	%
Described survey population	92
Reported population size	49
Described sampling method (n = 62)[a]	77
Reported sample size (n = 62)[a]	87
Reported response rate	87
Discussed the issue of nonresponse bias	24
Analyzed nonresponse pattern/bias (n = 99)[b]	19
Applied weights to adjust for selection probability (n = 39)[c]	26
Applied weights to adjust for response probability[d]	3

[a]Reporting the sampling method and sample size applies only to studies that involved sample surveys (n = 62).
[b]Analyzing the pattern and biasing effects of nonresponse applies only to studies that did not have a 100% response rate (n = 99).
[c]Weighting data to adjust for selection probability applies only to studies that used probability samples (n = 39).
[d]Weighting data to adjust for response probability applies only to studies that did not have a 100% response rate, a sufficiently large sample, and data from other sources to determine the correlates of response. These cases could not be clearly identified on the basis of the information available in the reviewed studies; thus the percentages shown are based on the total sample of reviewed studies (N = 100).

reported the sample size. Of the seven studies that used secondary data, six involved samples. Of those six studies, only one described the sampling method and only two reported the sample size. The others referred the reader to another publication for details about the sample or did not provide any further information.

Of all the reviewed studies (N = 100), 87% reported the response rate. Of the studies that used secondary data, only one reported the response rate. With the exception of one study that reported a 100% response rate, the reviewed studies invariably reported nonresponse; yet an overwhelming majority of the studies (76%) failed to mention the possibility that nonresponse might have affected their data.

Only 19% (19/99) of the studies with nonresponse indicated that the pattern or effects of nonresponse had been analyzed. Fourteen of these studies (74%) performed univariate comparisons of demographic variables between respondents and nonrespondents, between low- and high-effort respondents, or between respondents and the population of interest. This method is the first of the four methods of evaluating nonresponse bias described earlier. Two studies partially used the second method; they performed a multivariate analysis of the predictors of nonresponse but did not examine whether those predictors were also associated with research outcomes. These two studies were conducted by the same principal researcher using the same data. Two other studies used the third method, comparing independent and dependent variables between early and late respondents. They used different data, but the articles were written by the same first author. The last study reported that a nonresponse pattern analysis was performed but did not describe the method. None of the reviewed studies used the fourth method, that of obtaining survey data from nonrespondents through follow-up telephone interviews and comparing them with respondent data.

Some of the variables that typically were examined as possible correlates of nonresponse were school size, grade level, urbanicity, geographical location, and the designated respondent's gender. Nine studies (including two pairs of studies that used the same data) found statistically significant differences between respondents and nonrespondents or between early and late respondents. They found that larger schools, schools with fewer White students, urban schools, and principals (as compared with counselors) were less likely to respond. The effect of respondent gender on survey response was inconsistent across studies.

Only two studies analyzed factors associated with both survey response and dependent variables, comparing data from early and late respondents, and only one of them identified a potentially biasing variable: school size. Larger schools were more likely to have high-effort respondents and more likely to have sun-protection policies (e.g., requiring sunscreen and protective clothing during outdoor activities), which was one of the study's main outcomes.

Data weighting is one way to adjust for the differential probability of selection into a sample, the differential probability of responding to a survey, or both, thereby reducing bias due to the sampling design and nonresponse patterns. This is especially important when the purpose of the analysis is to estimate univariate distributions of variables in the population. The benefit of weighting data to adjust for selection probability applies only to studies that use probability samples, where the probability of selection is known for each sample unit. Of the reviewed studies that involved probability samples (n = 39), 26% (10/39) weighted their data to adjust for the differential probability of selection into the sample (Table 2).

Only three studies applied weights that adjusted for the differential probability of survey response. One of these studies did not present the results of the nonresponse analysis that served as the basis for creating the weight variable. The fact that only a small number of studies (n = 19) analyzed the correlates of nonresponse partly explains why so few studies weighted their data to adjust for response probability.

Case Study of Survey Nonresponse Evaluation

We now turn to a case study that demonstrates the use of three methods to assess the pattern and effects of survey nonresponse. The data come from a statewide, cross-sectional school survey where the school was the unit of analysis and the principal was the designated respondent (Kano & Bourque, 2007).

Method

The case study involved a mail survey of public schools in California on the topic of emergency preparedness. A self-administered questionnaire was sent by postal mail to the principal of each school selected into the sample. The principals were asked about their schools' policies and practices related to emergency preparedness. The survey was sponsored by a university-based research center, and participation was voluntary.

Sample

Schools were sampled using a two-stage sampling design. The primary sample units were school districts; the secondary sample units were schools within districts. The sampling frames for school districts and schools were constructed using public school directories available from the Education Data Partnership website (available at http://www.ed-data.k12.ca.us/). The sampling frame for school districts consisted of all public elementary, unified, and high school districts that were open for enrollment during the 2003–2004 school year (n = 977). The sampling frame was stratified into seven categories of population density, ranging from "Large City" to "Rural—Outside of Metropolitan Statistical Area" as defined by the U.S. Census Bureau. A total of 200 school districts were sampled. The number of districts selected per stratum was proportional to the number of schools in each stratum, so that more districts were sampled from strata that had greater numbers of schools. Within each stratum, school districts were sampled with probability proportional to size, with size defined as the number of schools in a district.

The sampling frame for schools included 4,345 public elementary, middle/junior high, and high schools that belonged to the 200 school districts selected for the study. The sampling frame for schools was stratified by school district and, within each district, by school level (i.e., elementary, middle/junior high, high school). One school was randomly selected from each school level within a district so that a maximum of three schools and a minimum of one school were selected from each school district (high school districts often have only one high school). This resulted in a sample of 470 schools.

Survey Administration

A prenotification letter about the study was mailed to school principals. One week later, a questionnaire packet was mailed, which included a cover letter, information about an incentive, a list of resources on school emergency preparedness, the survey questionnaire, and a business reply envelope. The incentive was a lottery prize of emergency preparedness supplies. A reminder postcard was mailed 2 weeks after the questionnaire packet was sent.

Telephone follow-up for nonrespondents was initiated approximately 2 weeks after the mailing of reminder postcards. This follow-up call was intended only to encourage survey response and did not entail interviewing the nonrespondents by telephone. If it became clear that a designated respondent had not received the questionnaire or no longer had it, questionnaire packets were mailed for a second time. Because resources were limited, multiple callback attempts could not be made. If a completed questionnaire was not received within 2 weeks of the telephone call, a questionnaire packet was mailed again. Survey responses were received from September 2005 through February 2006. The research protocol was approved by the institutional review board and deemed exempt from having to obtain signed informed consent.

Evaluation of Nonresponse Bias

The response rate for this survey was 33% (157/470). This is similar to response rates reported for other organizational surveys (e.g., Baldauf, Reisinger, & Moncrief, 1999; Blair & Zinkhan, 2006; Cordes, Henig, Twombly, & Saunders, 1999; Hager, Wilson, Pollak, & Rooney, 2003; Tomaskovic-Devey, Leiter, & Thompson, 1994).

Although the sample had been designed as a probability sample, the obtained sample could not be analyzed as if it were a statistically representative sample of the population, given the low response rate. Therefore, four steps were taken to assess the effects of nonresponse on the generalizability of the research findings. The first three steps correspond respectively to the first, second, and third methods of evaluating potential bias that were described earlier in this article. The fourth method (obtaining survey data from nonrespondents by means of telephone interviews and comparing the data to survey data obtained from the mail survey respondents) was not used because the telephone follow-up conducted in this case study was intended strictly to encourage response to the mail survey and did not entail telephone interviewing of nonrespondents. The last step in the case study analysis involved exploring the validity of an assumption underlying the third method of nonresponse analysis.

Step 1. To begin, the researchers compared characteristics of respondents and nonrespondents on independent variables known from a source other than the survey. Descriptive data on schools and their student populations were obtained from a secondary source (Education Data Partnership, 2004). Differences in means between respondents and nonrespondents were tested using independent two-sample *t* tests, and differences in frequency distributions between the two groups were tested using Pearson chi-square analyses.

Step 2. Next, the researchers performed regression analyses to identify variables that were associated with both the probability of survey response and the study's outcome measures. This step corresponded to the second method of bias assessment described earlier. Binary logistic regression analysis was used to model survey response as a function of school characteristics (Hosmer & Lemeshow, 2000). Data on school characteristics of both respondents and nonrespondents were used for this analysis. The selected variables were enrollment size, percentage of racial/ethnic minority students, pupil–teacher ratio, local population density, and school level. The percentage of students enrolled in English Learner programs and the percentage enrolled in subsidized meal programs were both highly correlated with the percentage of racial/ethnic minority students (the Pearson correlation coefficients were .74 and .71, respectively) and thus were excluded from the regression models to avoid multicollinearity effects. Enrollment size, percentage of racial/ethnic minority students, and pupil–teacher ratio were recoded into categorical variables for use in the binary logistic regression analyses. This was done to facilitate the interpretation of the odds ratios associated with each variable; in other words, one category was used as a reference group. Enrollment size was recoded into a three-category variable (small, medium, and large) with roughly equal numbers of cases in each category; binary dummy variables, with small schools as the reference group, were created to represent this variable in the regression models. Percentage of racial/ethnic minority students and pupil–teacher ratio each were recoded into a dichotomous variable using a median split. Local population density was dichotomized into urban and nonurban using U.S. Census definitions, and school level was dichotomized into primary and secondary schools.

The regression analyses of research-relevant outcomes were performed using only the respondents' data because outcome data were not obtained from nonrespondents. Binary logistic regression was used to model the effects of school characteristics on binary dependent variables, including prior exposure to school violence and earthquakes, adoption of a standardized emergency management protocol, and presence of a school emergency preparedness coordinator. Ordinal logistic regression was used to model the same effects on the ordinal dependent variables: perceived preparedness for school emergencies and perceived adequacy of emergency training for teachers, both of which were measured on 5-point ordinal scales.

Step 3. The researchers then examined whether the level of effort required to obtain a survey response was associated with research outcomes. In addition, they tested the assumption underlying the preceding set of regression analyses, which was that the associations between independent and dependent variables observed among respondents also applied to the unobserved nonrespondents. In this analysis, high-effort respondents served as proxies for nonrespondents. Survey respondents were classified as high-effort if they required both telephone follow-up and a second mailing of the questionnaire. All other respondents were classified as low-effort. The main effect of high-effort response and the interaction effect of high-effort response with a potentially biasing variable (i.e., a variable that is associated with both survey response and research outcomes) on dependent variables were tested in regression models. The tests were performed with

Table 3
Comparisons of the Characteristics of Responding and Nonresponding Schools

School Characteristics	Responding Schools (N = 154) M (SD)	Nonresponding Schools (N = 316) M (SD)	t	p
Total student enrollment	980.7 (714.3)	912.7 (706.3)	0.98	.33
Racial/ethnic minority students (%)	57.1 (27.7)	62.0 (27.4)	–1.84	.07
Students enrolled in English Learner program (%)	18.3 (17.5)	22.0 (18.8)	–2.07	.04
Students enrolled in subsidized meal program (%)	42.5 (26.6)	46.9 (26.4)	–1.68	.09
Pupil–teacher ratio	22.3 (2.6)	21.9 (3.6)	1.17	.24
	%	%	χ^2	p
Local population density				
Urban	22.7	33.5	7.57	.02
Suburban	57.8	53.8		
Rural	19.5	12.7		
School level				
Elementary	33.1	43.0	4.26	.12
Middle	37.0	31.3		
High	29.9	25.6		

Note. M = mean; *SD* = standard deviation; *t* = measure of association derived using independent two-sample *t* test; χ^2 = measure of association derived using Pearson's chi-square test; *p* = level of statistical significance; English Learner program = special program for students who are not yet proficient in English; subsidized meal program = federally assisted, income-based school food program; pupil–teacher ratio = total student enrollment divided by the number of full-time equivalent teachers. Local population density is based on the U.S. Census Bureau's classification of population density of the community in which the school is located. Three responding schools lacked the data necessary to extract them from the pool of nonrespondents; thus those three responding schools remain classified as nonrespondents in this table. Data are from Education Data Partnership (2004).

reduced models, which did not include other covariates, as well as with full models, which included all other covariates. Showing that neither high-effort response nor its interaction term were statistically significant in the models of research-relevant outcomes would lend support to the assumptions that nonrespondents and respondents were similar in terms of research outcomes and that associations between independent and dependent variables observed among the respondents applied similarly to unobserved nonrespondents.

Step 4. Finally, school characteristics of low- and high-effort respondents and nonrespondents were examined to explore the validity of the assumption that high-effort respondents were reasonable proxies for nonrespondents. Differences in means between low- and high-effort respondents and nonrespondents were tested using one-way analysis of variance tests with Bonferroni's post hoc comparisons, and differences in frequency distributions were tested using Pearson chi-square analyses.

An alpha level of .05 was used to determine statistical significance for all statistical tests performed.

Results

Step 1. Comparing Responding and Nonresponding Schools on Independent Variables

The responding schools were different from the nonresponding schools in terms of the average percentage of students enrolled in English Learner programs and the distribution of schools across urban, suburban, and rural areas (Table 3). There were no other statistically significant differences between responding and nonresponding schools in terms of the independent variables.

Step 2. Identifying Potentially Biasing Variables

The logistic regression model predicting survey response (Table 4) showed that schools in urban areas had a statistically significant odds ratio (OR) of 0.60, with a 95% confidence interval (CI): 0.38, 0.94. That is, schools in urban areas had a lower probability of responding to the survey than did schools in nonurban areas, after controlling for other independent variables. No other school characteristic was associated with probability of survey response.

Urban location, which was the only statistically significant correlate of response probability, was also associated with exposure to school violence (Table 4). Schools in urban areas had three times greater odds of exposure to school violence than nonurban schools after controlling for other independent variables (OR = 3.02; 95% CI: 1.11, 8.25). This result indicated that the underrepresentation of urban schools in the obtained sample likely resulted in a downward bias in the univariate distribution of schools that had experienced violence on campus.

There were no statistically significant associations between urbanicity and the other dependent variables in the study (exposure to earthquakes, adoption of a standardized emergency management protocol, presence of a school emergency preparedness coordinator, perceived preparedness for school emergencies, and perceived adequacy of emergency training for teachers). Variables such as enrollment size and percentage of minority students were correlated with some of these dependent variables but not with

Table 4
Logistic Regression Models of the Effects of School Characteristics on Survey Response and Exposure to School Violence

Independent Variables	Survey Response (Coded as 1 = *responded*), *N* = 470		Exposure to School Violence (Coded as 1 = *exposed*), *N* = 150	
	B (*SE*)	*OR*	*B* (*SE*)	*OR*
Midsize school (1 = 600 – 1,099 students)	–0.01 (0.24)	0.99	0.84 (0.45)	2.33
Large school (1 = 1,100 + students)	0.32 (0.30)	1.37	1.34 (0.558)	3.83*
% Minority (1 = greater than 62.5%)	–0.15 (0.20)	0.86	0.43 (0.38)	1.54
Pupil–teacher ratio (1 = greater than 22.1)	–0.16 (0.24)	0.85	–0.48 (0.45)	0.62
Local population density (1 = urban)	–0.51 (0.23)	0.60*	1.11 (0.51)	3.02*
School level (1 = primary)	–0.34 (0.25)	0.71	–0.49 (0.44)	0.61
Intercept	–0.37 (0.26)	0.69	0.01 (0.41)	1.01
Hosmer-Lemeshow goodness-of-fit χ^2	8.79, *df* = 8, *p* = .36		5.12, *df* = 8, *p* = .75	

Note. *B* = unstandardized simultaneous logistic regression coefficient; *SE* = standard error; *OR* = odds ratio, the exponent of *B; df* = degrees of freedom; *p* = level of statistical significance; pupil–teacher ratio = total student enrollment divided by the number of full-time equivalent teachers. Local population density is based on the U.S. Census Bureau's classification of population density of the community in which the school is located. To determine past experience, respondents were asked, "To your knowledge, please tell us if the emergencies or crises listed below [list included school violence and earthquake] have occurred in the last three years, i.e., since September 2002, in or around your school." Data are from Education Data Partnership (2004).
**p* < .05.

survey response (data not shown). Thus urban location was the only biasing variable identified.

Step 3. Examining the Effect of High-Effort Response and Its Interaction With a Potentially Biasing Variable

Neither the main effects of high-effort response nor the interaction effects of high-effort response with urban location was statistically significant on any of the study's outcome measures. These findings were replicated in full regression models that controlled for other independent variables. The analysis results with exposure to school violence as the outcome are shown in Table 5. Similar nonsignificant results with other outcomes are not shown.

The statistically significant effect of urban location and the lack of a significant effect of high-effort response in Models 1 and 3 suggest that high-effort response had no confounding effect on the association between urban location and exposure to school violence. That is to say, there was no statistically significant difference in the odds of exposure to school violence between low-effort and high-effort respondents.

The lack of a significant interaction effect in Models 2 and 4 suggests that high-effort response had no moderating effect on the association between urban location and exposure to school violence. There was no significant difference between low-effort and high-effort respondents in the magnitude of association between urban location and exposure to school violence. The effect of simultaneously adding urban location and the interaction term into the models was statistically significant in Model 2 ($\chi^2 = 6.25$, $df = 2$, $p < .05$) but not significant in Model 4 ($\chi^2 = 5.59$, $df = 2$, $p = .06$). The statistics are not shown in Table 5.

These results corroborated the earlier findings that urban location is associated with both survey response probability and exposure to school violence, and that the positive association between urban location and exposure to school violence applies similarly to both low-effort and high-effort respondents. This suggests that the observed correlation between urbanicity and exposure to school violence is unlikely to be affected by nonresponse.

Step 4. Evaluating the Assumption That High-Effort Respondents Are Reasonable Proxies for Nonrespondents

Statistically significant differences were found in post hoc pairwise comparisons between low-effort responding schools and nonresponding schools in terms of the percentage of minority students, the percentage of students enrolled in English Learner programs, and the percentage of students enrolled in subsidized meal programs (Table 6). High-effort respondents were not statistically different from either low-effort respondents or nonrespondents in terms of any of the school characteristics. In sum, there was no statistical evidence that high-effort respondents were more similar to nonrespondents than they were to low-effort respondents with regard to independent variables.

Discussion

In this article we reviewed how survey methods and findings are reported in peer-reviewed publications where schools are the unit of analysis and principals are the designated respondents, and we demonstrated how nonresponse effects can be evaluated.

Adequacy of Reporting of Survey Research Results

A review of peer-reviewed articles on research involving K–12 school principal surveys showed that the descriptions of key information about survey methods and data are generally inadequate. Basic information, such as survey administration mode, population size, and sampling method (where applicable), were not always reported. Most of the reviewed studies involved primary data; however, in the few cases where secondary data were used, descriptions of the survey method and data were often very

Table 5
Logistic Regression Models of the Main Effects of High-Effort Response and Its Interaction Effects With Urbanicity on Exposure to School Violence

	Reduced Models				Full Models			
	Model 1 $N = 150$		Model 2 $N = 150$		Model 3 $N = 150$		Model 4 $N = 150$	
Variables	***B* (*SE*)**	***OR***	***B* (*SE*)**	***OR***	***B* (*SE*)**	***OR***	***B* (*SE*)**	***OR***
Main effects								
Local population density (1 = urban)	1.13 (0.49)	3.10*	1.06 (0.70)	2.90	1.11 (0.51)	3.02*	0.79 (0.74)	2.20
Respondent type (1 = high-effort)	–0.04 (0.36)	0.96	–0.06 (0.39)	0.94	–0.06 (0.38)	0.94	–0.15 (0.41)	0.86
Interaction effects								
Local population density with respondent type[a]			0.13 (0.98)	1.14			0.58 (1.03)	1.79
Covariates								
Midsize school (1 = 600 – 1,099 students)					0.84 (0.45)	2.31	0.85 (0.45)	2.33
Large school (1 = 1,100 students or more)					1.35 (0.55)	3.85*	1.37 (0.56)	3.95*
% Minority (1 = greater than 62.5%)					0.43 (0.38)	1.53	0.45 (0.38)	1.56
Pupil–teacher ratio (1 = greater than 22.1)					–0.48 (0.45)	0.62	–0.48 (0.45)	0.62
Primary school (1 = yes)					–0.50 (0.44)	0.61	–0.51 (0.44)	0.60
Intercept	0.47 (0.29)	1.59	0.48 (0.30)	1.61	0.05 (0.49)	1.06	0.10 (0.50)	1.10
Hosmer-Lemeshow goodness-of-fit χ^2	0.02, *df* = 2, *p* = .99		0.01, *df* = 2, *p* = 1.00		5.63, *df* = 8, *p* = .69		9.41, *df* = 8, *p* = .31	

Note. *B* = unstandardardized simultaneous logistic regression coefficient; *SE* = standard error; *OR* = odds ratio, the exponent of *B; df* = degrees of freedom; *p* = level of statistical significance. Local population density is based on the U.S. Census Bureau's classification of population density of the community in which the school is located. Survey respondents were classified as "high-effort" type (*n* = 68) if they had required both telephone follow-up and a second mailing of the questionnaire; otherwise, they were classified as "low-effort" (*n* = 86). Pupil–teacher ratio = total student enrollment divided by the number of full-time equivalent teachers. The dependent variable for all models is a binary variable indicating whether the school had experienced school violence in the prior 3 years (1 = yes). Data are from Education Data Partnership (2004).
*$p < .05$.

scant. Reports of these studies frequently referred readers to another source for details on the survey method and data characteristics. Although this is a common practice, authors should provide basic descriptions of the methods and data in their own articles.

Of even greater concern is that 13% of the reviewed studies did not report response rates and 76% did not discuss how nonresponse can affect survey data or the interpretation of findings. These percentages contrast with those found in similar reviews that have been conducted in other research fields, where about 60% of studies did not report response rates (Dooley & Lindner, 2003) and 35% did not discuss how nonresponse can affect survey data or the interpretation of findings (Lindner et al., 2001). Only 19% of the studies we reviewed that encountered nonresponse mentioned evaluating or controlling for it, as compared with 17% (Dooley & Lindner, 2003), 31% (Werner et al., 2007), and 50% (Lindner et al., 2001) of the articles reviewed in other studies. These omissions are especially problematic when the objective of a study is to obtain population estimates of univariate distributions. As the case study demonstrated, univariate distributions are more susceptible to nonresponse bias than are associations between variables (Blair & Zinkhan, 2006; Rogelberg et al., 2003). Given that nonresponse to surveys of schools is a common problem, failure to address the issue of nonresponse is alarming.

Although it is true that authors are often faced with page limitations and cannot describe in full the details of their studies, it must be recognized that the scientific value and integrity of studies are jeopardized when essential information about methods and data are not provided. Many of the key details can be explained succinctly.

Survey Nonresponse in the Case Study

Several factors may have dampened survey response in the case study. The reasons for refusal could not be asked because of the institutional review board's concern that doing so would apply indirect pressure on the designated respondents, seeming to suggest that a justification for refusal was necessary. However, information volunteered by nonrespondents provided some insight into why schools refused participation. It is helpful to view these in light of the theory that the likelihood of response to an organizational survey is a function of the respondent's *authority, capacity,* and *motive* to respond (Tomaskovic-Devey et al., 1994).

The most common reasons for refusal were time and policy constraints. This is consistent with findings from previous research on refusals among designated respondents of organizational surveys (e.g., Baldauf et al., 1999; Tomaskovic-Devey et al., 1994). Another reason for refusal was that a school would respond only to surveys mandated or authorized by a higher authority, such as the district office. This explanation highlights

Table 6

Comparison of School Characteristics for Low-Effort Respondents, High-Effort Respondents, and Nonrespondents

School Characteristics	Responding Schools: Low-Effort (n = 86) M (SD)	Responding Schools: High-Effort (n = 68) M (SD)	Nonresponding Schools (N = 316) M (SD)	F	p
Total student enrollment	976.2 (693.9)	986.4 (744.4)	912.7 (706.3)	0.48	.62
Racial/ethnic minority students (%)	53.8[a] (27.4)	61.2[a, b] (27.6)	62.0[b] (27.4)	3.08	.05
Students enrolled in English Learner program (%)	16.6[a] (17.4)	20.4[a, b] (17.6)	22.0[b] (18.8)	2.99	.05
Students enrolled in subsidized meal program (%)	38.9[a] (25.2)	47.2[a, b] (27.7)	46.9[b] (26.4)	3.28	.04
Pupil-teacher ratio	22.3 (2.7)	22.3 (2.6)	21.9 (3.6)	0.58	.56
	%	%	%	χ^2	p
Local population density					
Urban	20.9	25.0	33.5	8.14	.09
Suburban	60.5	54.4	53.8		
Rural	18.6	20.6	12.7		
School level					
Elementary	30.2	36.8	43.0	8.05	.09
Middle	33.7	41.2	31.3		
High	36.0	22.1	25.6		

Note. Survey respondents were classified as high-effort (n = 68) if they had required both telephone follow-up and a second mailing of the questionnaire; otherwise, they were classified as low-effort (n = 86). M = mean; SD = standard deviation; F = measure of association derived using one-way analysis of variance test; p = level of statistical significance; English Learner program = special program for students who are not yet proficient in English; subsidized meal program = federally assisted, income-based school food program; pupil–teacher ratio = total student enrollment divided by the number of full-time equivalent teachers. Local population density is based on the U.S. Census Bureau's classification of population density of the community in which the school is located. Data are from Education Data Partnership (2004).

[a, b]Different superscripts within rows indicate statistically significant differences detected in pairwise comparisons of group means using Bonferroni's post hoc tests.

the fact that school surveys typically are affected by school or district administrations' rate of cooperation in survey efforts as well as by the response rate among individuals (e.g., principals, teachers) to whom the survey is administered.

In addition, more than two thirds of the responding schools reported that their school did not have an emergency preparedness coordinator (i.e., an expert on the survey topic). Nonresponding schools were perhaps just as likely or even less likely to have someone in their school with the capacity, or relevant knowledge and experience, to respond to the survey. Some of the nonrespondents also mentioned that they did not feel comfortable disclosing information about their school's preparedness for disasters, indicating a lack of motive to respond. Given these additional constraints that may exist in organizational settings, the designers of organizational surveys need to go beyond the techniques developed for surveying individuals and develop sensitivity to organizational factors if they wish to increase survey response rates (Baldauf et al., 1999; Greer, Chuchinprakarn, & Seshadri, 2000; Hager et al., 2003; Jobber, 1986; Jobber & O'Reilly, 1998; Tomaskovic-Devey et al., 1994).

Evaluation of Nonresponse Bias

The response rate achieved in this study is typical of an organizational survey (Baldauf et al., 1999; Cordes et al., 1999; Hager et al., 2003; Tomaskovic-Devey et al., 1994). Nevertheless, it raises concerns about the effects of nonresponse. Nonresponse bias becomes an issue when there are variables associated with both survey response probability and study outcomes, indicating nonrandom nonresponse. The assessment of nonresponse bias in the case study was informed by several analytical procedures.

Correlate of Response Probability

Urban schools were found to have a lower probability of response. This finding is consistent with previous research showing that survey response tends to be lower in urban areas (Dake, Price, Telljohann, & Funk, 2004; Fowler, 1993; Goyder, 1982; Hackmann et al., 2002). Perhaps schools in urban areas are more likely to be in larger school districts where the authority and capacity to respond to questions about school safety and preparedness are more centralized at the district level. Urban schools may also have more competing priorities because of the size and diversity of their student populations.

Effects of Nonresponse

The regression analyses identified urban location as a variable that possibly introduced nonresponse bias into the data. Urban schools, which were less likely to respond to the survey, were also significantly more likely to have experienced school violence in recent years, controlling for other variables. However, no other outcome measures were affected by the high rate of nonresponse from urban schools. Thus the underrepresentation of urban schools in the obtained sample likely caused an underestimation of the frequency of exposure to school violence but did not affect univariate estimates of other dependent variables.

This finding was further supported by findings from the analysis of possible confounding and moderating effects of the level of effort required in obtaining a response. High-effort response was not associated with outcome variables after controlling for the effects of urbanicity and other covariates. The association between urban location and outcome variables remained constant across low- and high-effort respondents and, by extrapolation, nonrespondents. This result supports the assumption that the associations between urban location and research outcomes observed among the respondents also applied to nonrespondents. These results are consistent with the notion that bias in estimates of associations (e.g., between urbanicity and research outcomes) will be smaller than the bias in univariate estimates (e.g., distribution of exposure to school violence). This prediction applies even if a sample is biased with respect to one variable, as long as the relationships are observed across the full range of the related variables (i.e., the sample is not restricted; Blair & Zinkhan, 2006).

The assumption underlying the previous analysis is that high-effort respondents are reasonable proxies for nonrespondents. Data from this study showed no statistically significant differences on independent variables between high-effort respondents and nonrespondents, although there were some differences between low-effort respondents and nonrespondents. This result is not evidence, however, that high-effort respondents are similar to nonrespondents in terms of outcome measures or associations between variables. Given that outcome measures were not observed for nonrespondents, we could not investigate the validity of this assumption any further. Other empirical studies have shown that high-effort respondents can be poor proxies for nonrespondents (e.g., Teitler et al., 2003).

Limitations

The specific set of search and screening criteria used for the literature review may have been too restrictive. Nevertheless, the articles included in the review were obtained from a wide range of peer-reviewed journals, most of them in the field of education. The limitations of the case study include a low response rate and small sample sizes, especially for some subgroups (e.g., low- and high-effort respondents). In addition, the statistical analyses of nonresponse bias were conditional on certain assumptions. The assumption that was least accounted for is that high-effort respondents make reasonable proxies for nonrespondents. The scope of this study was also limited to a discussion of nonresponse bias and how to evaluate potential effects of nonresponse; the study did not address other sources of bias, such as coverage and selection bias, or statistical methods used to handle nonresponse bias or missing data, about which there is much literature (Dooley & Lindner, 2003; Lindner et al., 2001; Little & Vartivarian, 2005; Rogelberg et al., 2003; Rubin, 2004).

Conclusion

It is important for survey researchers to provide accurate and complete information about their survey procedures and to examine the effects of nonresponse on sample quality and the generalizability of results. At the same time, it should be recognized that generalizability of academic research is not entirely dependent on sample quality, because the relationships studied by academics often are fairly resistant to bias (Blair & Zinkhan, 2006; Rogelberg et al., 2003). Moreover, academic research has multiple paths to generalization. That is, generalizability can take the form not only of *probabilistic generalization,* which depends on sample procedures and quality, but also of theory, or *theoretical generalization,* and replication, or *empirical generalization* (Blair & Zinkhan, 2006). Thus researchers should use best practices to minimize sample bias, evaluate sample quality by taking into consideration all possible sources of bias, and recognize the importance of theory and replication as pathways to generalization.

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