*This article provides a review of recent research on the quality of survey data and serves as an introduction to this special issue of* SMR. *The article reviews standard statistical treatments of survey errors and discusses the importance of developing population models for measurement errors. A model is presented which nests measurement errors within other nonmeasurement survey errors—errors of coverage, sampling, and nonresponse. Promising new developments in research on survey measurement are reviewed and evaluated—applications of rational choice theories of response behavior, applications of cognitive theories of judgment and information processing, and experiments in the meaning of questions. The article concludes with a brief summary of the contributions included in this special issue of* SMR.

# Research on Survey Quality

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In the social sciences there are many different perspectives on the quality of survey data and different languages for describing the nature of survey errors (Groves 1988). From the point of view of the sampling statistician, survey errors are categorized as either sampling errors or nonsampling errors. From the perspective of the survey methodologist, the later category can be further divided into response errors and nonresponse errors. And from the point of view of classical measurement theories in the social sciences, survey measurement errors should be evaluated in terms of their reliability and validity. These perspectives all register a legitimate claim on understanding the nature and sources of survey error and they all introduce worthwhile standards for quantifying and evaluating the relative quality of survey measurement. With the exception of Groves's (1989) recent work (see

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below) there has been little systematic attention to the relationship between the various concepts and criteria used to describe sources of survey error.

Some aspects of survey error are relatively well-understood and appreciated by most survey practitioners. Sampling errors and errors of nonresponse are, for example, commonly understood and the quantification of these errors is widely recognized as an important aspect of survey reporting. For example, one journal, *The Public Opinion Quarterly*, puts such emphasis on the importance of sampling, nonresponse, and question wording that it publishes the following notice to contributors.

> All submissions . . . must contain the following information: (1) A definition of the population from which the sample was drawn and a statement of the selection procedure sufficient to permit at least approximate replication. (2) The response rate and details of its calculation. (If response rate is not appropriate, the number of refusals.) (3) Dates the survey was conducted. (4) Exact wording of all questions used in the analysis.

Of course, in addition to knowing the nature of the sampling procedures, journal editors often require the calculation of standard errors, sometimes even where it is inappropriate, as a means of evaluating the likelihood of sampling error. The point is that there is broad consensus that some types of survey errors should be quantified and reported, specifically sampling errors, question-wording effects, and potential nonresponse errors, but others are often neglected.

Other aspects of survey error are less-well understood, and this is perhaps why there is considerably less agreement about their importance. For example, no social science journal that I am aware of requires a report of the reliability of measurement, although editors sometimes request that estimates of reliability of composite scores be calculated and reported.[1] But, in general, the reliability of measurement is not an accepted standard for describing the quality of survey data. Similarly, measurement validity is a standard against which to evaluate the quality of survey data, but one which is only rarely of concern to survey methodologists. This is true despite the fact that high levels of invalidity may be just as fatal to the quality of one's inferences as a poor sample or a low response rate. Reliability and validity of measurement may be just as important (and perhaps more important) an indicator of survey quality as the more commonly accepted standards reflecting response rates and sampling procedures (Alwin 1989). It should be obvious that no matter how good one's sample and no matter how high one's response rate, the quality of the conclusions drawn from the survey depends heavily on the quality of measurement.[2] This should not be construed to mean that good samples and good response rates are not important, but rather that considerations of data quality not rely exclusively on such criteria.

Despite the lack of consensus on the importance of various criteria for evaluating the quality of survey data, there is little question about the existence of measurement errors, as well as nonresponse, sampling and nonsampling errors. In a recent attempt at a "synthesis" of the predominant perspectives on survey errors, Groves (1989, p. vi) presents the following framework for classifying the various types of survey errors:

*Coverage error.* Error that results from the failure to include some population elements in the sampling frame or population lists.

*Sampling error.* Error that results from the fact that a subset of the population is used to represent the population rather than the population itself.

*Nonresponse error.* Error that results from the failure to obtain data from all population elements selected into the sample.

*Measurement error.* Error that occurs when the recorded or observed value is different from the true value of a variable.

The presence of any of these types of survey errors can influence the accuracy of the inferences made from the sample data, and the potential for such errors in the application of survey methods places a high priority on being able to anticipate their effects. In the worst case the errors may be so great as to invalidate *any* conclusions drawn from the data. In the best case errors are minimized through efforts aimed at their reduction and/or efforts taken to minimize their effects on the conclusions drawn, in which case strong inferences can be made on the basis of the data. Errors are always to some extent present in survey data, and it is important for researchers to realize that it is untenable to confront survey data as if they were free of error (Alwin 1977).

## *AN OVERVIEW OF SURVEY ERRORS*

There are a number of different ways to think about the relationships among the several types of errors listed above. The classical sampling perspective on the classification of survey errors begins with an expression of the mean square error (MSE) for the deviation of the sample estimator for a sample mean (for a given sampling design) from the population mean, that is, MSE $(\bar{y}) = E(\bar{y} - \mu)^2$. This results in the expression:

$$\text{MSE}\,(\bar{y}) = \text{Bias}^2 + \text{Variance}$$

where Bias$^2$ refers to the square of the theoretical quantity $\bar{y} - \mu$, and Variance refers to the variance of the sample mean $\sigma^2_{\bar{y}}$.[3] Groves (1989, pp. 8-12) embeds the four error types listed above into this classical statistical distinction between bias and variance. He regroups coverage, sampling and nonresponse errors into a category of "nonobservational" errors (referring to measurement errors as errors of "observation"), and further classifies sources of measurement errors into those which are due to interviewers, respondent, instrument, and mode of observation (e.g., telephone vs. face-to-face interviewing). Thus his above four-fold classification becomes even more detailed, as can be seen in the following listing (see Groves, 1989, pp. 8-12):

$$\text{MSE}\,(\bar{y}) = \underbrace{\text{Bias}^2}_{\phantom{x}} + \underbrace{\text{Variance}}_{\phantom{x}}$$

| Bias$^2$ | Variance |
|---|---|
| *Nonobservational Errors* | *Nonobservational Errors* |
| Coverage bias | Coverage error variance |
| Sampling bias | Sampling error variance |
| Nonresponse bias | Nonresponse error variance |
| *Observational Errors* | *Observational Errors* |
| Interviewer bias | Interviewer error variance |
| Respondent bias | Respondent error variance |
| Instrument bias | Instrument error variance |
| Mode bias | Mode error variance |

The value of these distinctions is not entirely self-evident—for example, note that measurement errors are "observational," and sampling, coverage and nonresponse errors are "nonobservational."4 It is, on the other hand, useful to distinguish errors that bring about *bias* as against those that produce contributions to *variance*. And it is useful to distinguish various sources of measurement error, namely errors stemming from the mode of administration, the questionnaire, the interviewer and the respondent. Groves (1989) discusses many of these error sources and their assessment at great length, and the reader is referred to that excellent source for further elaboration of this scheme.

Although the classification of survey error sources is important, especially with respect to pinpointing their effects on sample estimates of means and their variances, it is even more important to understand what may be done (if anything) about them. And although the traditional statistical perspective highlights issues connected to *nonmeasurement* errors (that is, issues of population coverage, sampling, and nonresponse), that perspective is less useful for understanding problems of measurement. Those researchers working on problems of measurement errors—errors resulting from factors associated with interviewer behavior, the mode of data collection, the characteristics of the questions or of the respondent—have generally taken one of two approaches to the study of errors. One approach has been to focus on the reduction of errors through the development and testing of techniques that will improve the quality of the data collected (see Cannell, Miller, and Oksenberg 1981). A second approach is to develop models for the behavior of measurement errors which can be used to improve substantive inferences based on fallible data (see, e.g., Alwin and Jackson 1979; Alwin 1989).

## *A MEASUREMENT PERSPECTIVE ON SURVEY ERRORS*

In this section I present an alternative approach to the conceptualization of survey errors, which differs in several key respects from the traditional sampling perspective. Rather than begin with the sampling errors and frame other errors in terms of their contributions to *sampling bias* and *sampling variance* (see Groves 1989), I begin with the consideration of the population model for the measurement of some quantity, y, and move forward from the consideration of the estimation of that model. There are several such measurement models, and although I shall not address the question of which types of models are to be preferred for which types of questions, my general objective is to rescue the consideration of measurement errors from their traditional place as nonsampling errors. Although some of the ideas expressed here rely on classical measurement theories from psychometrics, the considerations go well beyond those foundations. It has been established that, except for the potential it has for contributing to an understanding of reliability of measurement (see the contribution in this issue by Alwin and Krosnick), the classical true-score notions of psychometric theory are quite limited.

The approach I take here relies on the metaphor of a set of nested structures, each inside the next, like a set of Russian *matrioshka* dolls, in which distinct levels of "nestedness" represent different "compoundings" of errors. In order to approximate the metaphor, one ideally might present this scheme in three dimensions, involving spheres rather than circles, but for present purposes the concentric circles shown in Figure 1 should be adequate to present these ideas. At the innermost level is the population model and the population "quantities" of interest to the researcher. Such quantities may be population means, variances, covariances, regression slopes, or any other population characteristic that can be justified in terms of the model for measurement. It is important to begin at this point, not only because of the importance of the distinction between sample and population in a sampling sense, but also because most researchers interested in modeling measurement errors begin (or should begin) by stating a model for the typical element in the population of interest. Thus this is the level at which one's inferences are directed, both in terms of sampling inferences and inferences about one's model for the measurement errors. If one believed there were no measurement errors, a belief which is unfortunately all too common in the social sciences, then survey errors would be due entirely to coverage error, sampling error, or nonresponse error. Or, by contrast, if one could

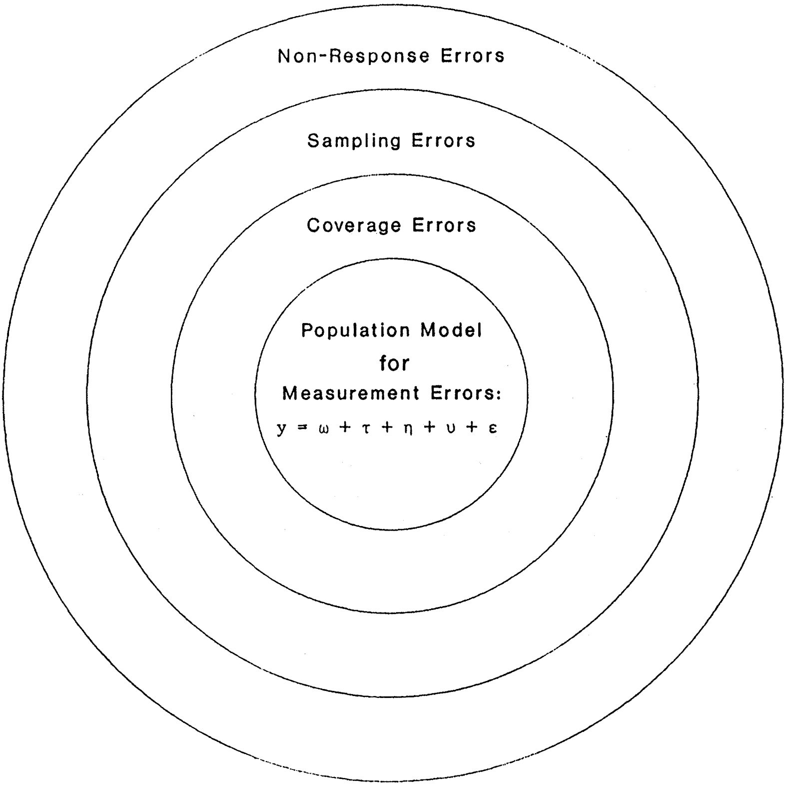


**Figure 1: The Relationship of Sources of Survey Errors**

observe the population directly, without any need to sample, and if one also could observe every such population element, there would only be measurement errors. The point to be made about these error sources is that in reality measurement errors exist primarily in the population model, and as such are nested within nonmeasurement errors. And within the set of nonmeasurement errors, each can be viewed as nested within the outer adjacent source, coverage errors within sampling errors and sampling errors within nonresponse errors.

Thus measurement errors result from processes that are formulated at the level of the theory for the population, affecting population means and population variances (not just sample quantities). Thus if one posits a random error component in the measurement of a variable, y, this component exists in theory in the responses of any element of the population. For example, the classical true-score theory of random measurement errors states its model, $y = \tau + \varepsilon$, for a population of elements (see Lord and Novick 1968). Measurement models that provide more complex renderings of the nature of the measurement errors involved in survey responses, including both random and non-random errors, also state the models at the level of the population (see e.g., Alwin and Jackson 1979; Alwin 1974, 1989).

Similarly, if one posits an "effect" or "error" due to the use of a particular question or question form, the theory and hypotheses regarding the nature of that error is stated *in theory* for the typical element in the population. Or, in the case of split-ballot experimental manipulations, a model is stated for the typical element experiencing a particular experimental condition. The same point can be made about the mode of interviewing, or any other condition during measurement experienced by elements in the population, such as the "fixed" effect of a particular interviewer. The argument is simply that those researchers attempting to deal with measurement errors — either through error-reduction techniques or through error structure modeling — are working with a theory that refers to the population rather than the sample. Further, it is the population characteristics which are of interest to the measurement analyst, and inferences made about the effects of error-reduction techniques on the validity of measurement, or on the model-based estimates of error structures, are inferences made to the population.

It is true that measurement errors enter the picture at the last stage of data collection, after errors of coverage, sampling, and nonresponse have occurred, but it must be remembered that measurement errors are never observed directly. One seeks evidence of measurement error, or measurement error reduction, in the sample estimates of population quantities. That is, one seeks to test implications for the data set forth by the measurement model formulated at the level of the population.

As noted before, if there were no need to sample the population or no need to deal with nonresponse problems because it were possible to observe all population elements, then coverage, sampling, and nonresponse errors would not exist. In such a hypothetical situation, the observations would be produced by whatever set of measurement errors and true variables were at work, and the goal would be to accurately describe the nature and pattern of errors at the level of the population. In fact, this hypothetical situation rarely exists, and the practical realities are such that nonmeasurement errors are introduced because samples must be used and sampled elements cannot always be observed. This, however, does not alter the fact that measurement models are developed at the level of theories of the response process applied to the elements in the population.

The graphic depiction in Figure 1 is also intended to indicate that all nonmeasurement errors may be viewed as compounding the researcher's ability to make appropriate inferences at the population level. These errors also do not enter the picture at the same time or in the same manner, but they all may be viewed as compounding the measurement errors believed to exist at the population level. As depicted in Figure 1, errors are potentially introduced in being able to say something about error structures in the population because of practical circumstances that prevent complete coverage of the population. As noted above, coverage errors result from the inability to develop appropriate sampling frames for the population of interest. In this case, one either lives with the potential errors introduced by lack of complete coverage, or one changes the target of inference to the portion of the population covered. After errors of coverage contaminate one's ability to describe the parameters of the population model, that is, subsequent to the errors introduced by the nature of the sampling frame, then sampling errors enter the picture. As previously noted, sampling errors result from the fact that a subset of the population must be observed rather than the entire population, so again, it should be clear that any particular sample is just one of an infinite set of possibilities, each containing a set of elements to which the measurement model for the population applies. Sampling errors include both variable errors and bias, introduced by the nature of the sampling design.

It is clear, then, that measurement errors in theory occur to all members of the population, not just to those sampled and interviewed. Thus, measurement errors must be conceptualized and models formulated in terms of the typical element in the population. The other survey errors—errors of coverage, sampling, and nonresponse—must be thought of as diminishing one's ability to precisely estimate parameters of the population model for the response. In this regard, Blalock's (1968) notion of *auxiliary theory* may come the closest to the point I am making. Auxiliary theories are particular operationalizations of more general theories, which can be operationalized in a number of alternative ways. Any particular study, with its errors of coverage, sampling and nonresponse, represents just one attempt to estimate the nature of the response process, as represented by the parameters of a population-based model that includes components of measurement error.

In the following section I present a general framework for considering components of survey measurement error. Within that context I define two standards that can be used to evaluate the quality of survey measurement: *reliability* and *validity*. Space does not permit within this context to discuss the design options for estimating these aspects of measurement (but see Alwin 1989), but a framework is presented for thinking about these quantities.

## *COMPONENTS OF SURVEY MEASUREMENT ERROR*

The literature on survey measurement errors suggests that responses to a survey question y at time *t* can be usefully conceptualized as containing a number of components. Elsewhere (Alwin 1989) I have suggested that variation in $y_t$ may be usefully summarized as containing the following five components:

1. The *true* variable being measured, or $\tau_t$.[5]
2. *Measurement bias* or other *invalid* constant measurement properties of questions, possibly due to the influence of question wording, form or context, denoted $\omega_t$.[6]
3. *Invalidity* due to errors of conceptualization and/or operationalization, that is, other stable constructs that are measured by the question, represented by $\eta_t$. For present purposes I represent a single such stable construct, as $\eta_t$, although in truth one should perhaps think in terms of a vector of such variates.[7]

4. Measurement errors that are random, denoted $\varepsilon_t$.
5. Measurement errors that are random with respect to $\tau_t$, but correlated with measurement errors on separate occasions of measurement. Such type of measurement error may be thought of as a stable component of error, one which is correlated over time, represented as $\upsilon_t$.[8]

Thus a given survey question, y, with response categories $y_1, y_2, \ldots y_r$, may be conceived as a function of the following components,

$$y = \omega + \tau + \eta + \upsilon + \varepsilon \tag{1}$$

where the $\omega$ term represents measurement bias, and the remaining terms are sources of variation.[9]

The variance of y may, thus, be written as:

$$\sigma_y^2 = \sigma_\tau^2 + \sigma_\eta^2 + \sigma_\upsilon^2 + \sigma_\varepsilon^2$$

Both $\upsilon$ and $\varepsilon$ are random with respect to all of the other components and are, thus, difficult to separate from one another. Owing to this difficulty, for many purposes one must consider their combined influences, treating them together, that is, $\delta = \upsilon + \varepsilon$, and $\sigma_\delta^2 = \sigma_\upsilon^2 + \sigma_\varepsilon^2$. Further, it is possible that $\tau$ and $\eta$ are correlated, but for present purposes it is assumed that they are not. It is in the nature of $\eta$ that we have difficulty separating it from $\tau$ and $\upsilon$, and thus, we make the arbitrary decision that, to the extent errors of measurement are correlated with $\tau$, they are included (however invalid) with $\tau$, and to the extent they are uncorrelated with $\tau$, they are included in either the $\eta$, $\upsilon$ or $\varepsilon$ terms. In actuality, our ability to separate these components will depend on the nature of the design used to estimate their contribution to response variation.

Reliability of measurement refers to the consistency of measurement, or more precisely, the extent to which the observed variance of survey responses can be said to be true variance rather than random error variance. The degree of reliability is an important aspect of the quality of survey information reported, and as such it should be reported along with estimates of sampling error, response rates, and other quantities used to register the quality of survey data. Reliability is a property of both the measurement instrument — the question in the case of survey measurement—and the population to which it is applied. In terms of the above formulation reliability is defined as the ratio of the true variance to the observed variance, that is, $\sigma_\tau^2/\sigma_y^2$, where $\sigma_y^2$ is defined as above. As I demonstrate elsewhere (Alwin 1989), it is frequently the case that in order to identify sources of variability in survey response due to factors in $\eta$, it is necessary to assume $\eta$ is uncorrelated with $\tau$. In addition, since both $\upsilon$ and $\varepsilon$ are also uncorrelated with $\tau$, and with each other, it is difficult to separate these three components. This is a major threat to the usefulness of classical true-score theory about measurement reliability and an important obstacle to the interpretation of reliability estimates, because variation of factors represented by the $\eta$ component are presumably reliable sources of variation. The use of inappropriate designs to estimate components of error variance will typically over-estimate the random error component, to the extent that it includes factors in $\eta$. The estimation of realiability of measurement is nonetheless important, because errors of measurement have significant consequences for inferences regarding substantive processes (e.g., Achen 1975; Alwin and Thornton 1984; Asher 1974; Bileby, Hauser, and Featherman 1977a, 1977b; Bielby and Hauser 1977; Borus and Nestle 1973; Converse and Markus 1979; Erikson 1979; Hauser, Tsai, and Sewell 1983; Jagodzinski and Kühnel 1987; Marquis and Marquis 1977; Siegel and Hodge 1968).

Heise and Bohrnstedt (1970) introduce the terms *validity* and *invalidity* to refer to the factors in $\tau$ and $\eta$ respectively, and this may have some utility here. In this sense, both valid and invalid variation in y is reliable. Estimates of reliable variance often do not make explicit the components of variation that are thought to be nonrandom errors, and to the extent such nonrandom components or error are inadvertently included in the "error" term, reliability interpretations may be inappropriate (see Alwin 1989).

In some important ways the use of the terms "validity" and "invalidity" to refer to components of reliable variation is unfortunate. Whereas the term validity refers to what one is measuring, reliability refers to how well it is being measured. Therefore, to define validity as a component of reliability is potentially misleading, because reliable measurement does not necessarily imply valid measurement. Although it is well-known that the index of reliability (which is the square root of the reliability) does place an upper bound on criterion validity, that is, the extent to which a measure may correlate with a theoretically defined criterion, it is illogical to reverse the implication. Such confusion about the appropriate use of the term "validity" has led some authors (e.g. Bohrnstedt 1983) to refer to univocal indicators (indicators presumed to measure one and only one thing) as "perfectly valid" because they are tied to one theoretical latent variable. This is an unfortunate confusion of terms, because such a latent variable, however reliably measured, may imperfectly represent the theoretical construct of interest (see Alwin 1989). In any event, I accept the distinction suggested by Heise and Bohrnstedt (1970) as a way of depicting multiple sources of reliable variation, but I also believe that the terms "validity" and "invalidity" should be used with great caution in such a context. In short, it would be a grave error to conclude that, because a high proportion of the reliable variance might be due to what appears to be "trait" variation, this necessitates the conclusion that one's measurement is valid. This would amount to a confusion of validity with reliability.[10] There is plenty of evidence in the survey methods literature of reliable reporting biases which are reflections of invalidity not validity (e.g., Bachman and O'Malley 1981; Cannell et al. 1981; Marquis 1978; Marquis, Duan, Marquis, and Polich 1981; Miller and Groves 1985; Weaver and Swanson 1974).

### *BETWEEN- VERSUS WITHIN-SUBJECTS DESIGNS*

One traditional approach to the study of survey response errors is the use of classical randomized experimental designs. In such research two or more forms (or ballots) are devised which are hypothesized to evoke different responses in a given population. These forms are then randomly assigned to members of the sample and the "effects" of different questions or "methods" are studied by examining differences in the marginal distributions or the covariance properties of those distributions (see e.g., Alwin and Krosnick 1985; Schuman and Presser 1981). This approach is useful for examining the constant errors or biases linked to a particular method or question form, as indicated in the previous discussion of components of survey error, but it provides little assistance in assessing components of response variation, whether random or systematic, unless it is embedded within a within-subjects design. By contrast, the classical psychometric approach to the study of measurement error requires replication of measurement within subjects, so that covariances among measures can be identified (see Alwin and Jackson 1979). It is a simple step, however, to generalize the concerns of both research traditions within the same set of models wherein within-subjects designs are nested within several experimental conditions (see Alwin 1989). For example, let the following be the representation of the above model (see Equation 1) for the *gth* experimental group:

$$y = \omega^g + \tau^g + \eta^g + \upsilon^g + \varepsilon^g \quad (2)$$

From this it may be seen that within this more general framework it is possible to compare components of response variance across subgroups or experimental conditions. McClendon and Alwin (1990) present an approach to the decomposition of population variance into components of survey error and point out how such between-group designs may be implemented within the context of studying components of survey error.

## *RESPONSE ERRORS—RECENT THEORETICAL APPLICATIONS*

Although respondent errors or response errors are just one of several error sources, they may ultimately be the most important aspect of measurement error. I say this in part because there has been little evidence that such things as interviewer effects or mode of administration effects or question effects produce much error, but also because recent developments concerning the quality of survey data emphasize the contributions of respondents to measurement error. In the following I briefly discuss several of these recent theoretical developments.

*APPLICATIONS OF RATIONAL CHOICE THEORY*

One of the most distinctive recent developments in the study of survey errors has been the application of principles of rational choice theory to the survey response process. Esser (1986) argues that survey data should be understood within the framework of the social situation that produced it, and that such an understanding will be enhanced if the investigator realizes that respondent behavior is a joint function of individual tendencies or motives to respond and situational constraints on behavior. Empirical data, Esser (1986) argues, should be understood as the result of situation-oriented rational actions of persons. Respondents are given tasks and are asked to solve them, and response behavior can only be understood if it is interpreted in this context of problem-solving behavior. However, the completion of survey tasks in a manner that produces reliable and valid data, that is, data free of error, depends on the respondent's cost-benefit analysis of various courses of action. Thus respondents are viewed as rational actors who perceive the elements of the situation, evaluate them against the background of their own preferences and perceived consequences—the subjective-expected-utility (SEU) values—of various action alternatives, and a certain action or response is selected according to the maximization of subjective utilities. In order to predict respondent behavior, then, one needs to understand the respondent's goal structure (e.g., providing valid data, or acquiescing to the perceived expectations of the interviewer) as well as the respondent's SEU values associated with response alternatives. Within this framework the selection of response alternatives is made according to the interests of the respondent, and one refers to this selection as rational choice, even if the respondent behavior in no way corresponds to the criteria of objective rationality (Esser 1986, 1990).

One recent application of these ideas relies on Herbert Simon's (1957; Simon and Stedry 1968) concepts of *satisficing* and *optimizing* behavior. The term satisficing refers to expenditures of the minimum amount of effort necessary to generate a satisfactory response to a survey question, in contrast to expenditures of a great deal of effort to generate a maximally valid response, or optimizing (see Tourangeau 1984; Krosnick and Alwin 1988, 1989). In Esser's (1986) framework these concepts may be used to define two broad response strategies, which result from the respon-dent's evaluations of utilities associated with selecting various response alternatives, given certain goals.

There is little work that has gone into understanding respondent behavior as rational choice, although there is clearly a recognition on the part of survey methodologists that an important component in generating maximally valid data is the fostering of a commitment on the part of the respondent to report information as accurately as possible (see Cannell et al. 1981). It is also clear from discussions of respondent burden that high cognitive and motivational demands placed on respondents may result in a reduction in the quality of data. The potential usefulness of these developments can be seen from Krosnick and Alwin's (1989) review of possible applications of the idea that respondents maximize their utilities. We suggest that satisficing seems to be most likely to occur when the costs of optimizing are high, which is a function of three general factors: the inherent difficulty of the task, the respondent's capacities or abilities to perform the task, and the respondent's motivation to perform the task. Consistent with Esser's (1986) theoretical point of view, the greater the task difficulty and the lower the respondent's ability and effort, the greater are the costs of optimizing and the greater is the likelihood that respondents will satisfice.

Although there is no explicit test of these notions to date, there is indirect evidence in the survey methods literature which provides a basis for a "satisficing" interpretation: random responding (Converse 1964, 1970, 1974), the effects of "don't know" filters (Schuman and Presser 1981), the effects of offering middle alternatives (Schuman and Presser 1981), response order effects (Krosnick and Alwin, 1987), acquiescence response bias (McClendon 1991 [this issue]), and non-differentiation in the use of rating scales (Krosnick and Alwin 1988). The conditions under which this wide array of well-documented response errors are precisely those that are known to foster satisficing. But although this is true, it may be too early to conclude that rational choice is the explanation for such response behavior.

*APPLICATIONS OF COGNITIVE THEORY*

Within the past decade survey methodologists have increasingly turned to cognitive psychologists for help in understanding the nature of responses to survey questions (see Jabine, Straf, Tanur, and Tourangeau 1984). This is understandable, given the cognitive demands placed on respondents in producing answers to survey questions, often requiring considerable effort and abilities on the part of respondents. Because of the reliance of survey research on cognitive processes of comprehension, recall, and judgment, there has been recent attention to the application of knowledge of these cognitive processes to the reporting of information in survey measurement.

Most of the information obtained in survey research falls into one of the following categories: (a) factual information about the respondent or about his or her family, household, or network of relationships, regarding respondent behavior or statuses and achievements, (b) respondent's knowledge, beliefs, or awareness of a topic, (c) respondent's judgment about the importance or relative priority of desirable end-states or instrumental means of obtaining such objectives, (d) respondent's attitudes or opinions with respect to particular objects or actors, (e) respondent's subjective evaluation of the state of accomplishment with respect to specific domains (Alwin 1989, pp. 313-14). And, although it is often assumed that the problems are greatest in the measurement of nonfactual content in surveys (e.g., Kalton and Schuman 1982), there is considerable agreement that a better understanding of the cognitive processes involved in producing survey information, across these various domains of content, will result in better data.

Regardless of whether one's focus is on the reduction of errors during the collection of survey data or on the modeling of errors once they have occurred, there is a common interest between these emphases in developing an accurate picture of the response process and factors that impinge on the collection of accurate information in the survey interview. It is generally accepted that errors introduced by the lack of clarity or vagueness in questions can be greatly improved through extensive pretesting, although it is clear from reading survey questionnaires that many of the questions posed by social scientists that even pretesting cannot help salvage some questions (see Cannell et al., 1989). Also, it is generally accepted that errors introduced by the interviewer can be minimized through the provision of additional training in the use of the questionnaire and through the provision of question-by-question instructions. Less is known about the introduction of errors via the particular mode of interviewing, although is commonly believed that measurement errors produced are similar across modes (see Groves and Kahn 1979).

With respect to respondent errors, it is generally agreed that there are several key cognitive processes that are involved in shaping the quality of the survey response. Four critical elements in the response process are as follows: (a) the respondent's comprehension and understanding of the question and the information it requests, (b) the respondent's access to or ability to recall the information requested, (c) the respondent's capacities for formulating a response to the question on the basis of the information at hand, and (d) the respondent's translation of that response into the response categories provided by the survey instrument (see Cannell et al. 1981; Strack and Martin 1987; Bradburn and Danis 1984; Tourangeau 1984, 1987; Tourangeau and Rasinski 1988). These sets of factors are referred to as *comprehension*, *accessibility*, *retrieval*, and *communication*. These factors play a role in affecting the quality of survey data, whether the question seeks information of a factual nature, or whether it asks for reports of subjective states, such as attitudes.

It is possible to conceive of sources of reporting error arising from each of these sets of factors as involving several dynamic elements of the situation, including the respondent's motivation to respond accurately, the respondent's mental abilities, especially with respect to the comprehension and understanding of the question, and the nature of the social relationship, or rapport, established during the survey interview. All of these factors affect the conditions of measurement and the quality of the data gathered.

Recent efforts to apply aspects of cognitive theory to survey methods have advanced some rather strong claims about the benefits to be gained from the cross-fertilization of cognitive psychology and research on survey methods (see e.g., Hippler, Schwarz, and Sudman

1987). And although there are isolated examples where efforts have been undertaken in large-scale surveys to test hypotheses based on cognitive theories (e.g., Krosnick and Alwin 1987; Alwin forthcoming), most applications of the cognitive perspective to research on survey quality are based on small-scale laboratory studies or are primarily ad hoc in nature. Still, it is expected that the cognitive perspective on survey methods will result in a number of salutary consequences for the practice of survey research and the improvement of the quality of survey measurement.

### *EXPERIMENTS IN QUESTION MEANING AND CONTEXT*

Language is essential to communication, and the purpose of the survey interview is to communicate information (see Clark 1985). Thus it should come as no surprise that the meaning attached to the words used in survey questions and the context in which questions are asked have important implications for the study of survey data quality. Considerable research on survey methods has been stimulated by Schuman and Presser's (1981) widely cited documentation of the effects of alterations in questions and question context on response distributions, although this line of research is not about survey errors as such. Much of this work has been characterized by "limited theorizing and many ad hoc experiments," (Hippler et al. 1987, p. 4), but it has had a role in underscoring the importance of language in shaping the quality of survey data. This "question-centric" perspective is articulated by Schuman and Kalton (1985), who state that "the fundamental unit of meaning in a survey is the single question . . . (playing a role) analogous to that of atoms in chemistry . . . (with) words, phrases, and similar constituents of questions . . . regarded as the subatomic particles of the survey process" (p. 642). Although this perhaps overstates the singular importance of the survey question in the response process (see the other theoretical perspectives given above), there is little question that the meaning and interpretation given to the survey question has an important role in shaping the quality of the data. These issues are especially critical to assessing the validity of survey measurement.

On the one hand, survey researchers typically expect that respondents will know the meaning of the question and its constituent parts, and it is further assumed that all respondents will interpret these meanings in the same way. On the other hand, there are many examples from the survey literature wherein investigators have asked respondents to report their perceptions of the meaning of survey questions, which sometimes reveal substantial variation in interpretations of words and phrases (e.g., Belson 1981). It is important to recognize, however, that if a question is unclear in its meaning, or if parts of questions can have more than one meaning, it would hardly be possible to argue that the question measures a single true value. Thus the ability to assess measurement errors may presuppose questions and response categories that are precise in meaning.

## *OVERVIEW OF THIS SPECIAL ISSUE*

In this issue of *Sociological Methods & Research* there are four articles, each of which deals with some aspect of survey data quality. In this section I briefly review each of these.

### *ESTIMATING NONRESPONSE AND RESPONSE BIAS*

The first article, by Nora Cate Schaeffer, Judith A. Seltzer, and Marieka Klawitter, examines nonresponse and response bias in self-report measures of child support payments. This study is representative of a genre of studies known as validation or record-check studies wherein survey reports are compared with official records for purposes of ascertaining the extent of bias.[11] In this case the criterion measure is based on court records of court-ordered child support payments. This measure is then used as a basis for assessing the validity of the self-report measures obtained via telephone interviews. Their detailed analysis of the components of bias in these measures leads to the conclusion that nonresponse bias can be a serious threat to survey-based estimates.

*ACQUIESCENCE AND RECENCY RESPONSE-ORDER EFFECTS*

The second article, by McKee J. McClendon, employs a split-ballot (i.e., between-subjects) design to investigate the effects of acquiescence and response-order on attitudes toward lawyers, items from Srole's Anomia Scale and Rosenberg's Self-Esteem Scale. As McClendon indicates, both the effects of acquiescence and response-order effects can be interpreted in terms of the "satisficing" principle elaborated on previously. He tests a theory developed by Jon Krosnick and me (see Krosnick and Alwin 1987) regarding response-order effects which postulates that in telephone interviews one is much more likely to find *recency* effects for a list of response alternatives read to the respondent, whereas in face-to-face interviews where visual cues are provided response-order effects are much more likely to exhibit the phenomenon of *primacy*-order effects. McClendon's results show that both acquiescence and response-order effects—primarily the recency effects predicted by the Krosnick-Alwin theory—are pervasive.

*THE RANDOMIZED RESPONSE METHOD*

The third article, by U. N. Umesh and Robert A. Peterson focuses on a technique introduced by Warner (1965, 1971) for measuring self-reports on sensitive topics. Many surveys request information considered to be highly sensitive (e.g. alcohol and drug use, certain disease-related symptoms, illegal behaviors, etc.), and although survey researchers adhere to strict standards of protecting the confidentiality of reported information, these assurances are often insufficient to encourage respondents to disclose such information. There are three accepted procedures for obtaining sensitive information in surveys: (a) the use of self-completion procedures in which the respondent completes a self-administered form, (b) the use of informant reports in which the respondent reports on a "network sample" familiar to them, but unidentified for purposes of the survey, or (c) the randomized response method (RRM), in which the respondent is presented with a randomized question of which the interviewer is ignorant. The technique is actually misnamed because it is the question to be answered by the respondent is what is randomized, not the response. In any event, the Umesh and Peterson article reviews 20 years of research in which the RRM has been applied to the measurement of sensitive information. Their review addresses the question of measurement validity by reviewing those studies which produced sample estimates based on direct questioning versus the RRM. Although there are flaws in these validation studies, they do provide a basis for the conclusion that the method deserves continued development.

### *THE RELIABILITY OF SURVEY ATTITUDE MEASUREMENT*

The article in this volume by Jon Krosnick and me addresses the investigation of the reliability of the measurement of attitudes—defined as latent, unobserved predispositions to respond in positive versus negative ways—which social and behavioral scientists often posit to underlie behavior, behavioral intentions and verbal statements (see Ajzen 1990; Alwin 1973). The question of the reliability of attitude measures is, as we point out in greater detail, particularly problematic because researchers (e.g., Kalton and Schuman 1982; Schuman and Kalton 1985) often think attitudes are more difficult to measure than "factual" material, and because it is often possible to discount the "precision" of some attitude questions.

Based on a meta-analysis of single-item reliabilities for 96 survey items, we conclude that reliability is partly a function of both question and respondent characteristics. With some important exceptions questions with more response categories tend to have higher reliabilities, those with more extensive verbal labeling tend to have higher reliabilities, but those offering a "don't know" alternative were not found to be more reliable. Also we found that older respondents and those with less schooling tend to report their attitudes with somewhat less reliability. Our results underscore the observation made earlier in this introductory article that the reliability of measurement is an important ingredient in the evaluation of survey quality, as important as assessing the extent of coverage, sampling and nonresponse errors.

# NOTES

1. There is a tradition within education and psychology which emphasizes the importance of reporting reliabilities of scales (American Psychological Association 1974). These standards, however, are generally not thought of as relevant to the evaluation of survey data.

2. It is noteworthy in this regard that a recent summary of survey methods for social psychologists addressed the problems of sampling and question-wording differences (which are not errors in a strict sense), but paid virtually no attention to problems of measurement errors, unreliability and invalidity (see Schuman and Kalton 1985).

3. See Anderson, Kasper, and Frankel (1979) for the classical sampling framework for the discussion of survey errors.

4. I prefer to use the term "measurement error" rather than "observational error" in order to avoid confusion. In contrast to ethno-methodologists and some laboratory researchers, survey researchers do not "observe" their subjects. Other types of researchers make "observational errors"; survey researchers make "measurement errors."

5. One may define *true scores* either (a) in the manner of psychometric theory, as the expected value of the propensity distribution of the observed variable for a fixed person (Lord and Novick 1968, p. 30), or (b) as the "true" known value, as in the case of "Platonic" true scores (Lord and Novick 1968), which is the approach used in "record check" validity studies (see below). In this model, $\tau$ is defined as in psychometric theory, and I include the possibility of objective "true" scores by absorbing their mean difference into response bias (see Marquis, Marquis, and Polich 1981).

6. *Measurement bias* (also called *response bias*) is defined as the difference between the expected value of the propensity distribution and the "true" mean. In the case of attitudes and other subjective variables that have no objectively defined "true" value, there can be no such thing as a response bias. In the case of such variables any constant errors result from the operation of question wording, question form, or the context in which the question is asked. These errors affect the central tendency or mean of the response distribution. Of course, the model allows for the possibility that some question form, wording, or context effects may vary depending on other variables assessed by y, in which case the $\omega$ term does not adequately represent such effects. In such cases, the effects of question bias are contained in the $\eta$ and $\upsilon$ terms.

7. Some questions may assess the intelligence or education of the respondent instead of what they are intended to measure, or questions may assess the respondent's tendency to acquiesce, or produce the socially desirable response. One theory even suggests that agreeing to some questions may reflect the respondent's need for social approval (Crowne and Marlowe 1960).

8. One such an example of *within-time* random errors that may be correlated over time is memory or the conscious motivation to be consistent with previous responses (see Moser and Kalton 1972, p. 353).

9. For present purposes I have dropped the subscript $t$, which denotes time of measurement. Suffice it to say at this point, that except for $\omega$ and $\varepsilon$, all of the components of survey response may be correlated over time.

10. There is considerable confusion in the survey methods literature about the concept of reliability and its implications for validity of measurement. The index of reliability is defined as the square root of reliability (see Lord and Novick 1968). Unfortunately, there is some

confusion about this in the survey methodology literature. Groves (1989, p. 42), for example, confuses "reliability" with the "index of reliability," defining the latter in the way the psychometric literature defines the former.

11. There are a number of problems in conducting such validation studies (see Marquis 1978, 1984). For an exemplary study of validating economic data, see Duncan and Mathiowetz (1985).

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