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Validating Instruments in MIS Research¹

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Abstract

Calls for new directions in MIS research bring with them a call for renewed methodological rigor. This article offers an operating paradigm for renewal along dimensions previously unstressed. The basic contention is that confirmatory empirical findings will be strengthened when instrument validation precedes both internal and statistical conclusion validity and that, in many situations, MIS researchers need to validate their research instruments. This contention is supported by a survey of instrumentation as reported in sample IS journals over the last several years.

A demonstration exercise of instrument validation follows as an illustration of some of the basic principles of validation. The validated instrument was designed to gather data on the impact of computer security administration on the incidence of computer abuse in the U.S.A.

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ACM Categories: H.0, J.0

Introduction

Instrument validation has been inadequately addressed in MIS research. Only a few researchers have devoted serious attention to measurement issues over the last decade (e.g., Bailey and Pearson, 1983; Goodhue, 1988; Ives, et al., 1983; Ricketts and Jenkins, 1985), and while the desirability of verifying findings through internal validity checks has been argued by Jarvenpaa, et al. (1984), the primary and prior value of instrument validation has yet to be widely recognized.

There are undoubtedly a number of reasons for this lack of attention to instrumentation. Because of rapid changes in technology, MIS researchers often feel that research issues must be handled with dispatch (Hamilton and Ives, 1982b). Then, too, exploratory, qualitative, and non-empirical methodologies that have dominated MIS research (Farhoomand, 1987; Hamilton and Ives, 1982b) may not require the same level of validation.²

Exactly why, then, should MIS researchers pay closer attention to instrumentation? The question is a fair one and deserves a full answer. In the first place, concerns about instrumentation are intimately connected with concerns about rigor in MIS methodology in general (McFarlan, 1984). McFarlan and other key researchers in the field (e.g., Hamilton and Ives, 1982a; 1982b) believe there is a pressing need to define meaningful MIS research traditions to guide and shape the research effort, lest the field fragment and never achieve its own considerable potential. The sense is that MIS researchers need to concentrate their energies on profitable lines of research — streams of opportunity — in both the near and the far term (Jenkins, 1983; 1985; McFarlan, 1984). This should be carried out, whenever possible, in a systematic and programmatic manner (Hunter, et al., 1983; Jenkins,

² Cf. Campbell (1975), however, Benbasat, et al. (1987), also deal with this question.

1983, 1985; Mackenzie and House, 1979), using, where appropriate (McFarlan, 1986), advanced statistical and scientific methods (King, 1985).

Besides bringing more rigor in general to the scientific endeavor, greater attention to instrumentation promotes cooperative research efforts (Hunter, et al., 1983) in permitting confirmatory, follow-up research to utilize a tested instrument. By allowing other researchers in the stream to use this tested instrument across heterogeneous settings and times, greater attention to instrumentation also supports triangulation of results (Cook and Campbell, 1979). With validated instruments, researchers can measure the same research constructs in the same way, granting improved measurement of independent and dependent variables and, in the long run, helping to relieve the confounding that plagues many streams of MIS literature (cf. Ives and Olson, 1984).

Attention to instrumentation issues also brings greater clarity to the formulation and interpretation of research questions. In the process of validating an instrument, the researcher is engaged, in a very real sense, in a reality check. He or she finds out in relatively short order how well conceptualization of problems and solutions matches with actual experience of practitioners. And, in the final analysis, this constant comparison of theory and practice in the process of validating instruments results in more "theoretically meaningful" variables and variable relationships (Bagozzi, 1980).

Finally, lack of validated measures in confirmatory research raises the specter that no single finding in the study can be trusted. In many cases this uncertainty will prove to be inaccurate, but, in the absence of measurement validation, it lingers.

This call for renewed scientific rigor should not be interpreted in any way as a preference of quantitative to qualitative techniques, or confirmatory³ to exploratory research. In what has been termed the "active realist" view of science (Cook and Campbell, 1979), each has a place in uncovering the underlying meaning of phe-

nomena, as many methodologists have pointed out (Glaser and Strauss, 1967; Lincoln and Gupta, 1985). This article focuses on the methodologies and validation techniques most often found on the confirmatory side of the research cycle, as shown in Figure 1. The key point is that confirmatory research calls for rigorous instrument validation as well as quantitative analysis to establish greater confidence in its findings.

Linking MIS Research and Validation Processes

In order to understand precisely what instrument validation is and how it functions, it is placed in the context of why researchers — MIS researchers in particular — measure variables in the first place and how attention to measurement issues — the validation process — can undergird bases of evidence and inference. In the scientific tradition, social science researchers attempt to understand real-world phenomena through expressed relationships between research constructs (Blalock, 1969). These constructs are not in themselves directly observable, but are believed to be latent in the phenomenon under investigation (Bagozzi, 1979). In sociology, for example, casual constructs like "home environment" are thought to influence outcome constructs like "success in later life." Neither construct can be observed directly, but behaviorally relevant measures can be operationalized to represent or serve as surrogates for these constructs (Campbell, 1960).

In MIS, research constructs in the user involvement and system success literature offer a case in point. In this research stream, user involvement is believed to be a "necessary condition" (Ives and Olson, 1984, p. 586) for system success. As with other constructs in the behavioral sciences (Campbell, 1960), system success, or "the extent to which users believe the information system available to them meets their information requirements" (Ives and Olson, 1984, p. 586), is unobservable and unmeasurable; it is, in short, a conceptualization or a mental construct. In order to come to some understanding about system effectiveness, therefore, researchers in this area have constructed a surrogate set of behaviorally relevant measures, termed User Information Satisfaction (UIS), for the system success outcome construct (Bailey and Pearson, 1983; Ives, et al., 1983; Ricketts and Jenkins, 1985).

³ Strictly speaking, and from the point of view of Strong Inference, no scientific explanation is ever confirmed (Blalock, 1969; Popper, 1968); incorrect explanations are simply eliminated from consideration. The terminology is adopted here for the sake of convenience, however.

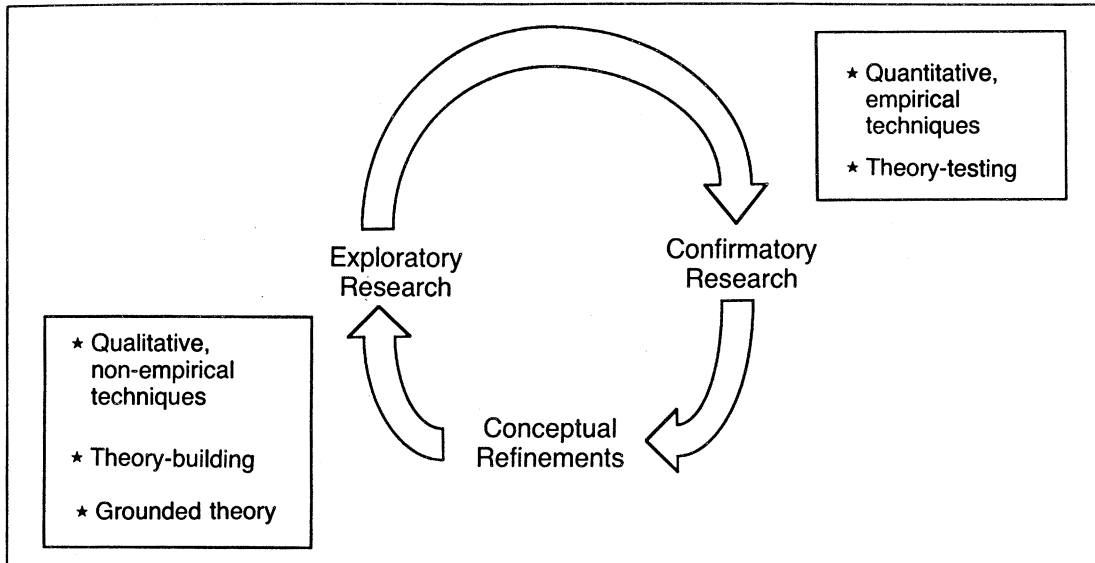


Figure 1. The Scientific Research Cycle
(Based on Mackenzie and House, 1979 and McGrath, 1979)

The place of theory in confirmatory research

Given this relationship between unobserved and observed variables, what is the role of theory in the process? Blalock (1969), Bagozzi (1980), and others argue that theories are invaluable in confirmatory research because they pre-specify the makeup and structure of the constructs and seed the ground for researchers who wish to conduct further studies in the theoretical stream. This groundwork is especially important in supporting programs of research (Jenkins, 1985).

By confining constructs and measures to a smaller *a priori* domain (Churchill, 1979; Hanushek and Jackson, 1977) and thereby reducing the threat of misspecification, use of theory also greatly strengthens findings. Moreover, selection of an initial set of items for a draft instrument from the theoretical and even non-theoretical literature simplifies instrument development. When fully validated instruments are available, replication of a study in heterogenous settings is likewise facilitated (Cook and Campbell, 1979).

For MIS research, there is much to be gained by basing instruments on reference discipline theories. Constructs, relationships between constructs, and operations are often already well-specified in the reference disciplines and avail-

able for application to MIS. This point has been effectively made by Dickson, et al. (1980). A specific need for strong reference discipline theory in support of the user involvement thesis has been put forth by Ives and Olson (1984).

Difficulties in accurately measuring constructs

Measurement of research constructs is neither simple nor straightforward. Instrumentation that the UIS researcher devises to translate the UIS construct (as perceived by user respondents) into data, for instance, may be significantly affected by choice of method itself (as in interviews versus paper-and-pencil instruments) and components of the chosen method (as in item selection and item phrasing (Ives, et al., 1983)). Bias toward outcomes the researcher is expecting — in this case a positive relationship between user involvement and system success — can subtly or overtly infuse the instrument. Inaccuracies in measurement can also be reflected in the instrument when items are ambiguously phrased, length of the instrument taxes respondents' concentration (Ives, et al., 1983), or motivation for answering carefully is not induced. Knowledge about the process of system development, therefore, is only bought with assumptions about the "goodness" of the technique of

measurement (Cook and Campbell, 1979; Coombs, 1964).

In a perfectly valid UIS instrument, data measurements completely and accurately reflect the unobservable research construct, system success. Given real-world limitations, however, some inaccurate measurements inevitably intrude on the translation process. The primary question for MIS researchers is the extent to which these translation difficulties affect findings; in short, we need to have a sense for how good our instruments really are. Fortunately, instrumentation techniques are available that allow MIS researchers to alternately construct and validate a draft instrument that will ultimately result in an acceptable research instrument.

Instrument validation

In the MIS research process, instrument validation should precede other core empirical validities (Cook and Campbell, 1979),⁴ which are set forth according to the kinds of questions they answer in Figure 2. Researchers and those who will utilize confirmatory research findings first need to demonstrate that developed instruments are measuring what they are supposed to be measuring. Most univariate and multivariate statistical tests, including those commonly used to test internal validity and statistical conclusion validity, are based on the assumption that error terms between observations are uncorrelated (Hair, et al., 1979; Hanushek and Jackson, 1977; Lindman, 1974; Reichardt, 1979). If subjects answer in some way that is more a function of the instrument than the true score, this assumption is violated. Since the applicable statistical tests are generally not robust to violations of this assumption (Lindman, 1974), parameter estimates are likely to be unstable. Findings, in fact, will have little credibility at all.

An instrument can be deemed invalid on grounds of the *content* of the measurement items. An instrument valid in content is one that has drawn representative questions from a universal pool (Cronbach, 1971; Kerlinger, 1964). With representative content, the instrument will

⁴ External validity, which deals with persons, settings, and times to which findings can be generalized, does precede instrument validation in planning a research project. For the sake of brevity and because this validity can easily be discussed separately, external validity is not discussed here.

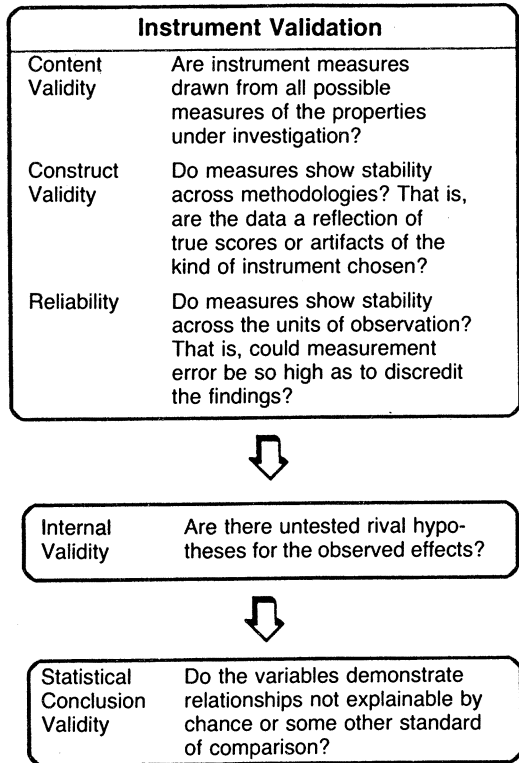


Figure 2. Questions Answered by the Validities

be more expressive of the true mean than one that has drawn idiosyncratic questions from the set of all possible items. Bias generated by an unrepresentative instrument will carry over into uncertainty of results. A content-valid instrument is difficult to create and perhaps even more difficult to verify because the universe of possible content is virtually infinite. Cronbach (1971) suggests a review process whereby experts in the field familiar with the content universe evaluate versions of the instrument again and again until a form of consensus is reached.

Construct validity is in essence an operational issue. It asks whether the measures chosen are true constructs describing the event or merely artifacts of the methodology itself (Campbell and Fiske, 1959; Cronbach, 1971). If constructs are valid in this sense, one can expect relatively high correlations between measures of the same construct using different methods and low correlations between measures of constructs that are expected to differ (Campbell and Fiske, 1959). The construct validity of an instrument can be assessed through multitrait-multimethod

(MTMM) techniques (Campbell and Fiske, 1959) or techniques such as confirmatory or principal components factor analysis (Long, 1983; Nunnally, 1967).⁵ Measures (termed "traits" in this style of analysis) are said to demonstrate *convergent validity* when the correlation of the same trait and varying methods is significantly different from zero and converges enough to warrant "further examination" (Campbell and Fiske, 1959, p. 82). Evidence that it is also higher than correlations of that trait and different traits using both the same and different methods shows that the measure has *discriminant validity*.⁶

Since this systematic bias, or common method variance, is by definition unobservable, as are the constructs themselves (Nunnally, 1967), good examples of what can now be termed construct validity are difficult to come by. Nevertheless, patterns of correlated errors can occur. For example, take the case where large numbers of students unthinkingly check off entire columns in a course evaluation. If these same students were probed later for comparable information in a circumstance where thoughtfulness and fairness prevailed, there would likely be little agreement across methods, raising doubt about the validity of either instrument.

On the other hand, scores can even be distorted when there is no systematic response within the instrument. A single item can be ambiguous or misunderstood by individuals so that responses on this trait differ among alternative measures of the same trait. The subject has answered in a way that is a function of his or her misunderstanding rather than a variation of the true score. *Reliability*, hence, is essentially an evaluation of measurement accuracy — for example, the extent to which the respondent can answer the same or approximately the same questions the same way each time (Cronbach, 1951). High correlations between alternative measures or large Cronbach alphas are usually signs that the measures are reliable.

⁵ It should be noted that factor analysis has been used to test the factorial composition of a data set (Nunnally, 1967) even when maximally different data collection methods are not used. Most of the validation of User Information Satisfaction instruments has followed this procedural line.

⁶ Concurrent and predictive validity (Cronbach and Meehl, 1955) are generally considered to be subsumed in construct validity (Mitchell, 1985) and, for this reason, will not be discussed here.

Internal validity

Internal validity raises the question of whether the observed effects could have been caused by or correlated with a set of unhypothesized and/or unmeasured variables. In short, are there viable, rival explanations for the findings other than the single explanation offered by the researcher's hypothesis (or hypotheses)? For general social science research, the subject has been treated at length by psychometricians Cook and Campbell (1979) who argue that causation requires ruling out rival hypotheses as well as finding associative variables. In MIS, the critical importance of internal validity has been argued by Jarvenpaa, et al. (1984).

It is crucial to recognize that internal validity in no way establishes that the researcher is working with variables that truly reflect the phenomenon under investigation. Sample groups, too, can be easily misdefined if the instrumentation is invalid. This point is made clearer through a hypothetical case.

Suppose, for the sake of illustration, that a researcher wished to detect the effect of system response time on user satisfaction in a record-keeping application. Suppose also that the researcher did not include in the methodological design a validation of the instrument by which user satisfaction was being measured. Such an instrument could be utilized in field or laboratory experimentation, quasi-experimentation, or survey research. Suppose, moreover, that had the researcher actually tested the instrument, serious flaws in the representativeness of measures (content validity), the meaningfulness of constructs as measured (construct validity), or stability of measures (reliability) would have emerged. At least one of these flaws would almost certainly occur, for instance, if the user satisfaction measure was based entirely on how the user felt about EDP applications as a whole.⁷ In this scenario, extensive experimental and statistical controls placed on these meaningless and poorly measured constructs could seemingly rule out all significant rival hypotheses. Internal validity would thus be assured beyond a reasonable doubt. Findings, however, would be moot because measurement was suspect.

⁷ Cf. Ricketts and Jenkins (1985) on this point.

Statistical conclusion validity

Statistical conclusion validity is an assessment of the mathematical relationships between variables and the likelihood that this mathematical assessment provides a correct picture of the true covariation (Cook and Campbell, 1979). Incorrect conclusions concerning covariation (Type I and Type II error) are violations of statistical conclusion validity, and factors such as the sample size and reliability of measures can affect this validity.

Another factor used in determining the statistical conclusion validity of a study is statistical power. Power is the probability that the null hypothesis has been correctly rejected. Proper rejection is closely associated with sample size so that tests with larger sample sizes are less likely to reject the null hypothesis improperly (Baroudi and Orlikowski, 1989; Cohen, 1969; 1977; Kraemer and Thiemann, 1987). It is also statistically related to alpha, the standard error, or reliability of the sample results, and the effect size, or degree to which the phenomenon has practical significance (Cohen, 1969). Non-significant results from tests with low power, i.e., a probability of less than 80 percent that the null hypothesis has been correctly rejected (Cohen, 1969), are *inconclusive* and do not indicate that the effect is truly not present.

Statistical assumptions made by the technique(s) of choice (e.g., regression, MANCOVA, factor analysis, LISREL) have a bearing on the credibility of the analysis, but conclusions based on these statistics say nothing about the viability of rival hypotheses per se or the meaningfulness of constructs in the first place. Much confirmatory MIS research in the past has utilized only statistical conclusion validity to evaluate results, a situation that can often lead to confounded results because high correlation of cause and effect is only one of the criteria for establishing causality (Cook and Campbell, 1979).

In summary, it is possible to show the overall results of violating order or position in the validation process. Figure 3 highlights the dangers. Evaluating statistical conclusion validity alone establishes that variables covary or have some mathematical association. Without prior validation, it is not possible to rule out the possibility that statistical associations are caused by moderator variables (Sharma, et al., 1981) or misspecifications in the causal model (Blalock, 1969). Preceding statistical conclusion validity with internal validation procedures strengthens findings by allowing the researcher to control effects from moderator variables and rival hypotheses. Even these tests do not establish, however,

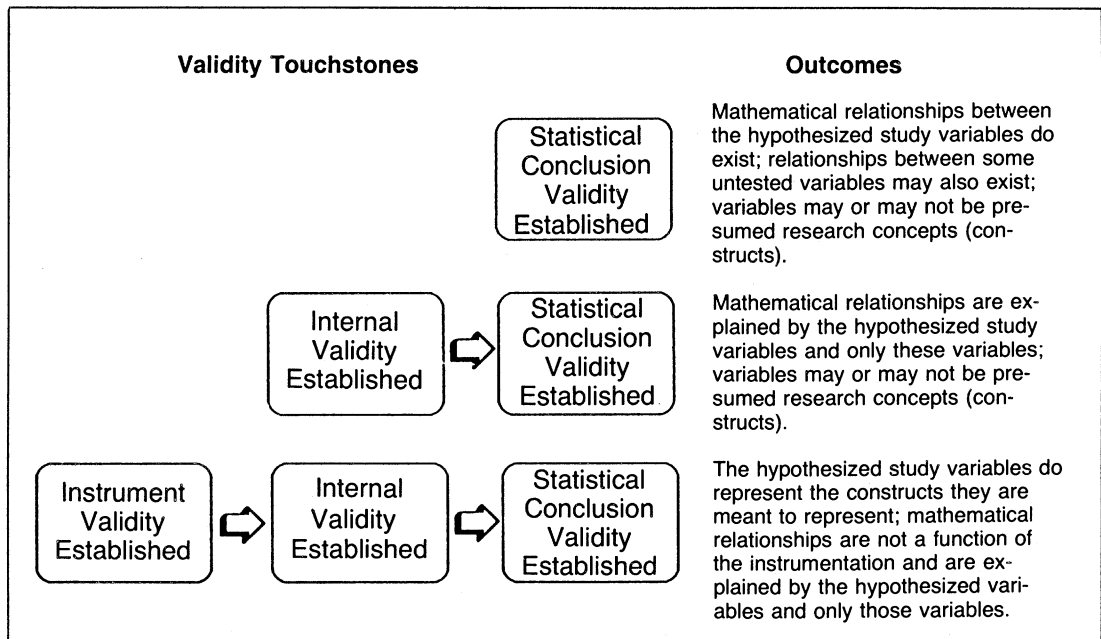


Figure 3. Outcomes From Omitted Validities

that measures chosen for the study are directly tied to the molar mental constructs that answer the research questions (Cook and Campbell, 1979). To accomplish this final step, the instrument itself must be validated.

The Need for Instrument Validation in MIS

Before elaborating a demonstration exercise of instrument validation, it is important to show, as contended, that instruments in the MIS literature are at present, insufficiently validated. To examine this contention, over three years of published MIS empirical research (January 1985-August 1988) have been surveyed. Surveyed journals include *MIS Quarterly*, *Communications of the ACM*, and *Information & Management*. To qualify for the sample, a study had to employ either: (a) correlational or statistical manipulation of variables, or (b) some form of data analysis (even if the data analysis was simply descriptive statistics). Studies utilizing archival data (e.g., citation analysis) or unobtrusive measures (e.g., system accounting measures) were omitted from the sample unless it was clear from the methodological description that key variable relationships being studied could have been submitted to validation procedures.

Background study results

The survey overwhelmingly supports the contention that instrumentation issues are generally ignored in MIS research. With 117 studies in the sample, the survey data indicates that 62 percent of the studies lacked even a single form of instrument validation. Table 1 summarizes other key findings of the survey.

As percentages in Table 1 indicate, MIS researchers rely most frequently (17 percent of the studies) on previously utilized instruments as a primary means of, presumably, validating their instruments. Almost without exception, however, the employment of previously utilized instruments in MIS is problematic from a methodological standpoint. In the first place, many previously utilized instruments were themselves apparently never validated. The only conceivable gain from this procedure, therefore, is to save the time of developing a wholly new instrument. There is no advantage from a valida-

tion standpoint.⁸ In the second place problems arise in the case where researchers are adapting instruments that have been previously validated. In almost all cases, researchers alter these instruments in significant ways before applying them to the IS environment. However well-validated an instrument may have been in its original form, excising selected items from a validated instrument does not result in a validated derivative instrument. In fact, the more the format, order, wording, and procedural setting of the original instrument is changed, the greater the likelihood that the derived instrument will lack validated qualities of the original instrument.

The remainder of the descriptive statistics in Table 1 paint a similarly disturbing picture. Reliability is the most frequently assessed validity, but even in this case, some 83 percent of the studies do not test this minimal level of validation. Reliability is infrequently coupled with tests of construct validity (less than 16 percent of the studies) and assessment of content validity are almost unheard of (5 percent of the studies) in the literature.

Although the nature and extent of validation varied somewhat from journal to journal, a more revealing finding of this background study showed that experimental researchers were much less likely to validate their instruments than non-experimental researchers. Laboratory and field experiments are often pretested and piloted to ensure that the task manipulates the subjects as intended (manipulation checks), but the instruments used to gather data before and after the treatment are seldom validated. Instruments developed for case studies are also unlikely to be validated.

By comparison with reference disciplines like the administrative sciences, MIS researchers were less inclined to validate their instruments, according to the study data. These ratios, moreover, are generally several orders of magnitude in difference. More than 70 percent of the researchers in the administrative sciences report reliability statistics, compared with 17 percent in MIS.

⁸ A weak argument can possibly be made that some degree of nomological validity can be gained from employing previously utilized instruments. This is a very weak argument, however, because nomological validity usually occurs only in a long and well-established stream of research, a situation that does not apply in this case.

Table 1. Survey of Instrument Validation Use in MIS Literature*

Instrumentation Categories	Response		Percentage	
	Yes	No	Yes	No
1. Pretest	15	102	13%	87%
2. Pilot	7	110	6%	94%
3. Pretest or pilot	22	95	19%	81%
4. Previously utilized instrument	20	97	17%	83%
5. Content validity	5	112	4%	96%
6. Construct validity	16	101	14%	86%
7. Reliability	20	97	17%	83%

* A total of 117 cases were surveyed from three journals (43 from *MIS Quarterly*; 28 from *Communications of the ACM*; 46 from *Information & Management*).

They also validate constructs twice as often as MIS researchers (Mitchell, 1985).

A Demonstration Exercise of Instrument Validation in MIS

Conceptual appreciation of the role of instrument validation in the research process is useful. But the role of instrument validation may be best understood by seeing how validation can be applied to an actual MIS research problem — validation of an instrument to measure computer abuse. This process exemplifies the variety of ways instruments might be validated, a process that is especially appropriate to confirmatory empirical research.

The research instrument in question was designed to measure computer abuse through a polling of abuse victims in industry, government, and education.⁹ Computer abuse was operationally defined as misuse of information system assets such as programs, data, hardware, and computer service and was restricted to abuse perpetrated by individuals against organizations (Kling, 1980). There is a growing body of evidence that the problem of computer abuse is serious (ABA, 1984) and that managers are concerned about IS security and control (Brancheau and Wetherbe, 1987; Canning, 1986; Dickson, et al., 1984; Sprague and McNurlin, 1986). Organizations respond to this risk from abuse by attempting to: (a) deter abusers through coun-

termeasures such as strict sanctions against misuse (these programs managed by a security staff), or (b) prevent abusers through countermeasures such as computer security software. The overall project set out to estimate the damage being sustained by information systems from computer abuse and to ascertain which control mechanisms, if any, have been successful in containing losses.

As one of the first empirical studies in the field, the project developed testable propositions from the baseline of the criminological theory of General Deterrence (Nagin, 1978). Causal linkages were postulated between exogenous variables such as deterrent and preventive control measures, and endogenous variables such as loss and severity of impact. Use of theory in this manner strengthens instrument development by permitting the researcher to use prespecified and identified constructs.

Early in the research process, it was determined that the most effective and efficient means of achieving a statistical sample and gathering data was a victimization questionnaire. This type of research instrument has been used extensively in criminology (e.g., the National Crime Survey)¹⁰ and in prior computer abuse studies (AICPA, 1984; Colton, et al., 1982; Kusserow, 1983; Parker, 1981) to explore anti-social acts. Paradigms for exploring anti-social behaviors are well established in criminological studies and, because computer abuse appears to be a prototypical white collar crime, research techniques from this reference discipline were appropriate in this study.

⁹ This research was supported by grants from IRMIS (Institute for Research on the Management of Information Systems) and the Ball Corporation Foundation. It was accomplished under the auspices of the International DPMA (Data Processing Management Association).

¹⁰ Cf. Skogan (1981).

An obvious reference discipline for activities that involve a violation of social codes is criminology, which provides a ready behavioral explanation for why deterrents may be effective controls. General Deterrence theory has well-established research constructs and causal relationships. There is a long-standing tradition of research in this area and concurrence by panels of experts on the explanatory power of the theory (Blumstein, et al., 1978; Cook, 1982). Constructs and measures have been developed to test the theory since the early 1960s, and its application to the computer security environment, therefore, seemed timely.

The thrust of most of the theoretic deterrence literature has been on "disincentives" or sanctions against committing a deviant act. Disincentives are traditionally divided into two related, but independent, conceptual components: (1) certainty of sanction and (2) severity of sanction (Blumstein, et al., 1978). The theory holds that under conditions in which risk of being punished is high and penalties for violation of norms are severe, potential offenders will refrain from illicit behaviors.

In the literature, observable commitment of an enforcement group, such as the police in punishing offenders, typically serves as a surrogate for perception of risk or certainty of sanction (Gibbs, 1975). This assumes that potential offenders perceive risk to be in direct proportion to efforts to monitor and uncover illicit behaviors. In other words, people believe that punishment will be more certain when enforcement agents explicitly or implicitly "police" or make their presence felt to potential offenders. In information systems, this is equivalent to security administrators making their presence felt through monitoring, enforcing, and disturbing information about organizational policies regarding system usage, or what this article refers to as deterrent countermeasures. When punishment is severe, it is assumed that offenders, especially less motivated potential offenders, are dissuaded from anti-social acts (Straub and Widom, 1984). Table 2 presents the pertinent connections between the conceptual terminology used in this article, the constructs most frequently cited in General Deterrence theory, and the actual items as measured in the final instrument, which appears in the Appendix.

Table 2. Concepts, Constructs, and Measures

Concepts	Research Construct	Survey Item	Measure Description
Abuse	Damage	25	- Number of incidents
		39	- Actual dollar loss
		38	- Opportunity dollar loss
		37	- Subjective seriousness index
Deterrents	Disincentives: Certainty	10	- Full-time security staff
		11	- Part-time security staff
		12	- Total security hours/week
		14b	- Data security hours/week
		15	- Total security staff salaries
		22	- Subjective deterrent effect
	13 minus 35 & 13 minus 28 minus 36	- Longevity of security (from inception to incident date)	
	Disincentives: Severity	18	- Information about proper use
19		- Most severe penalty for abuse	
22		- Subjective deterrent effect	
Preventives	Preventives	16	- Use of software access control
		17	- Use of specialized software
Rival Explanations	Environmental-Motivational Factors	30	- Privileged status of offender
		29	- Amount of collusion
		32	- Motivation of offender
		31	- Employee/non-employee status
		24	- Tightness of security
		21	- Visibility of security
		28 minus 35 & 36	- Duration of abuse

Evolving directly from prior instruments, a draft instrument was first constructed to reflect study constructs. Next, instrument validation took place in three well-defined operations. The entire research process was conducted in four phases, as outlined in Table 3.

Phase I: Pretest

In the pretest, the draft instrument was subjected to a qualitative testing of all validities. This phase was designed to facilitate revision, leading to an instrument that could be formally validated.

Personal interviews were conducted with 37 participants in order to locate and correct weaknesses in the questionnaire instrument. The interview schedule was structured so that three full waves of interviews could be conducted. Each version of the instrument reflected improvements suggested by participants up to that point and was continuously re-edited for the next version. The selection of interviewees was designed to get maximum feedback from various expertises, organizational roles, and geographical regions. Initial interviews included divergent areas of expertise: academic experts in methodology and related criminological areas, academic experts in information systems, practitioner experts in information systems and auditing, and law enforcement officials at the state and federal levels. Participants came from a variety of organizations in the public and private sectors including banking, insurance, manufacturing, trade, utilities, transportation, education, and health services. A range of key informants were sought including system managers, operations managers, data security officers, database administrators, and internal auditors.

The in-depth interviews offered insight into the functioning of the entire spectrum of security controls and what respondents themselves indicated were important elements in their deterrent force. Interviews were designed to move progressively from an open-ended general discussion format, to a semi-structured format, and finally to a highly structured item-by-item examination of the draft instrument. Information gathered earlier in the interview did not, thereby, bias responses later when the draft instrument was evaluated. In the beginning of the interview, participants were encouraged to be discursive. They spoke on all aspects of their computer security, including personnel, guidelines and policies, software controls, reports, etc. They were prompted by a simple request for information. Concepts independently introduced by more than three respondents were noted as was the precise language in which these constructs were perceived by the participants (content validity and reliability). In the second semistructured segment, questions from the interviewer directed attention to key matters of security raised by other participants but not raised in the current session. Clarification of constructs and the means of operationalizing selected constructs were also undertaken in this segment (construct validity and reliability).

To eliminate ambiguities and further test validities, participants in the third segment of the interview were asked to evaluate a version of the questionnaire item-by-item. Content validity was stressed in this segment by encouraging participants to single out pointless questions and suggest new areas for inquiry. Most participants chose to dialogue in a running commentary format as they reviewed each question. This facilitated preliminary testing of the other validi-

Table 3. Phases of the Computer Abuse Measurement Project

Phase	Name	Validation Tests Performed	Content Validity	Construct Validity	Reliability
I	Pretest	Qualitative	X	X	X
II	Technical Validation	Cronbach alphas MTMM Analysis		X	X
III	Pilot Test	Cronbach alphas Factor Analysis Qualitative		X	X
IV	Full-Scale Victimization Survey		X		

ties. Because misunderstanding of questions would contribute to measurement error in the instrument, for instance, particular attention was paid to possible discrepancies or variations in answers (reliability).

By the final version, the draft instrument had undergone a dramatic metamorphosis. It had collected data in every class of the relevant variables. To provide sufficient data points to assess reliability, several measures of each significant independent variable as well as each dependent variable were included.

Phase II: Technical validation

In keeping with the project plan, technical instrument validation occurred during the fall of 1984 and the winter of 1985 (Phase II). Its purpose was to validate construct validity and reliability. To triangulate on the postulated constructs (construct validity), extremely dissimilar methods (Campbell and Fiske, 1959) were utilized. The purpose in this case is similar to the purpose of triangulation in research in general (Cook and Campbell, 1979). By bringing very different data-gathering methods to bear on a phenomenon of interest, it is possible, by comparing results, to determine the extent to which instrumentation affects the findings, i.e., how robust the findings truly are.

Specifically, personal interviews were conducted with 44 key security informants. The target population was primarily information system managers, computer security specialists, and internal auditors. Their responses were correlated with questionnaire responses made independently by other members of the organization who had equal access to security information. The instrument also included equivalent, "maximally similar" measures (Campbell, 1960, p. 550) to gauge the extent of the random error (reliability).

In Phases II and III the instrument was quantitatively validated along several methodological dimensions. These dimensions and analytical techniques are summarized in Table 3.

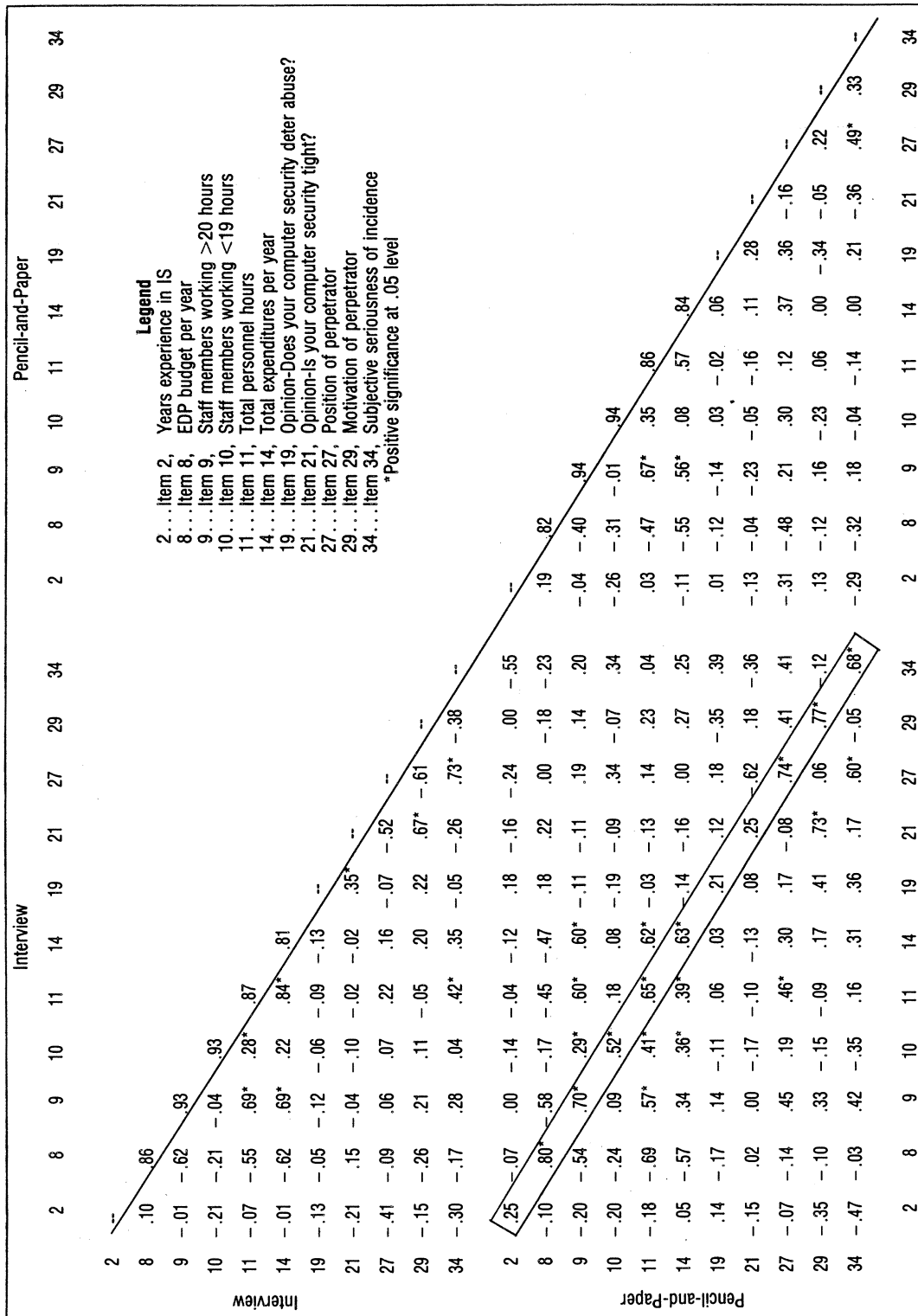
Technical Validation of Construct Validity

Tests of construct validity are generally intended to determine if measures across subjects are similar across methods of measuring those variables. In the Computer Abuse Measurement Project, methods were designed to be "maximally

different," in accordance with the Campbell and Fiske criteria. Triangulation by dissimilar methods is designed to isolate common method variance and assure that the Campbell and Fiske assumption of independence of methods is not violated. A personal interview was conducted with one participant while a pencil-and-paper instrument (the pretested questionnaire) was given to an equally knowledgeable participant in the same organization. During the interactive interviewing process, the researcher verbally presented the questions and recorded responses in a 1-2 hour timeframe. A limited amount of consultation was permitted, but, in general, respondents were encouraged to keep their own counsel. Questionnaire respondents, on the other hand, had no personal contact with the researcher and responded entirely on their own. This instrument was completed at leisure and returned by mail to the researcher. In all instances, the need for independence of answers from the two types of respondents was stressed.

If measures vary little from the pencil-and-paper instrument to the personal interview, they can be said to be independent of the methods used to gather the data and to demonstrate high construct validity. Conversely, high method variance indicates that measures are more a function of the instrumentation than of underlying constructs. As an analytical technique, Campbell and Fiske's MTMM allows patterns of selected, individual measures to be compared through a correlation matrix of measures by methods, as shown in Figure 4.¹¹ Analysis of the MTMM matrix in Figure 4 shows generally low method variance and high convergent/discriminant validity. This can be seen most directly by comparing the values in the validity diagonal — the homotrait-heteromethod diagonal encircled in the lower left matrix partition or block — with values in the same row or column of each trait. Evidence in favor of what is termed "convergent validity" is a relatively high, statistically significant correlation in the validity diagonal. Evidence in favor of "discriminant validity" occurs if that correlation is higher than other values in the same row or column, i.e., $r(i,i) > r(i,j)$ and $r(i,i) > r(j,i)$ where i is not equal to j . The total personnel hours trait (item 11), for example, has a .65 correlation significant at the .05 level and

¹¹ The matrix in Figure 4 is only a partial matrix of all the correlations evaluated. These matrix elements were chosen simply for purposes of demonstration.



Legend

2... Item 2, Years experience in IS
 8... Item 8, EDP budget per year
 9... Item 9, Staff members working >20 hours
 10... Item 10, Staff members working <19 hours
 11... Item 11, Total personnel hours
 14... Item 14, Total expenditures per year
 19... Item 19, Opinion-Does your computer security deter abuse?
 21... Item 21, Opinion-Is your computer security tight?
 27... Item 27, Position of perpetrator
 29... Item 29, Motivation of perpetrator
 34... Item 34, Subjective seriousness of incidence

*Positive significance at .05 level

Figure 4. MTMM Matrix for Victimization Instruments

is greater than other entries in its row and column.

In the MTMM test, the test instrument failed to achieve convergent validity on three out of the 11 items (items 2, 19 and 21) as evidenced by low, non-significant correlations in the validity diagonal ($r = .25, .21, .25$ respectively). Trait 2, as a simple indicator of the number of years of experience of the respondent, however, should demonstrate only random associations, overall, with both itself and other traits in the homomethod or heteromethod blocks. That is, there is no particular reason why two participants from the same organization should have had the same years of experience in information systems, an interpretation borne out by the non-significant validity diagonal value for this trait. By virtue of the validation design, traits 19 and 21 may not be as highly correlated in the validity diagonal as the objective measures (traits 9, 10, 11, and 14), since these are subjective measures from very different sources of information. It is reasonable to expect that two people in the same organization can give very similar objective answers about security efforts (e.g., traits 9, 10, 11, and 14); it is also reasonable to expect them to vary on opinions about the effectiveness of these efforts. And the data unveils this pattern.

Even though the instrument, upon inspection, does pass the first Campbell-Fiske desideratum for convergent validity, there are several violations of discriminant validity. In the interview homomethod block, the correlation $r(14,11)$, for instance, is higher at .84 than the validity diagonal value of .65. Likewise, in the pencil-and-paper homomethod block, $r(11,9)$ exceeds its corresponding validity diagonal value of .65. Interpretations of aberrations such as these can be difficult (Farh, et al., 1984; Marsh and Hocevar, 1983). For one thing, it is well-known that high but spurious correlations will occur by chance alone in large matrices such as the one in Figure 4 (231 elements). Yet, in spite of reasonable or otherwise ingenious explanations that can be offered for off-diagonal high correlations, it may be more straightforward in MTMM analysis to classify departures from the technical criteria simply as violations. As long as violations do not completely overwhelm good fits, the instrument can be said to have acceptable measurement properties.

An analysis of common method variance is the last procedure in evaluating this matrix. Note that

the interview-based monomethod correlation between items 11 and 14 ($r = .84$) is significantly elevated over the parallel heteromethod correlation between items 11 and 14 ($r = .39$). The elevation of .84 over .39 is an index of the degree of common method variance, which appears to be substantial. Basically, .39 is the correlation between items 11 and 14 with the common method variance due to interviewing removed. Similarly, the pencil-and-paper-based monomethod correlation between items 9 and 11, $r = .67$, should be compared against the corresponding heteromethod correlation of .57. Examining other high correlations in the monomethod triangles and comparing them to their heteromethod counterparts suggest that items 9, 11, and 14 do demonstrate common method variance.

Perhaps part of the problem is that subjects are confounded with methods in the design. The people who responded to the interview were a different group than those who filled out the pencil-and-paper questionnaire. Thus, there is likely a systematic effect because of "person," apart from the effect of method per se, that accounts for this pattern of findings.¹² If a person overestimates the number of staff, it is likely he or she will also overestimate total personnel hours. Thus, one might expect a person's estimate of staff to correlate more highly with their own estimate of personnel hours than with someone else's estimate of staff. If this correlation is a true accounting of the real world, then "person" is a source of shared method variance, i.e., errors that are not statistically independent of each other that are common within a method but not across two methods. Unfortunately, with the existing design, it is not possible to disentangle the effects due to people from the effects due to the instrument per se.

Even though this design does fulfill the Campbell-Fiske (1959) criterion for "maximally different

¹² The effect here might have been mitigated if the validation sample had been randomly selected from the population of interest. Random selection of subjects or participants from the population as well as random assignment to treatments (in the case of experimental research) reduces the possibility that systematic effects of "person" (as in "individual differences") are present in the data. Random selection affects external validity whereas random assignment affects internal validity (Cook and Campbell, 1979).

methods," common method variance detracts from the "relative validity" (p. 84) that is generally found throughout the matrix. In this study, if both methods had been administered to each respondent as one solution to evaluating common method variance, a new source of confounding — a test-retest bias (Cook and Campbell, 1979) — would have been introduced.

Technical Validation of Reliability

Essentially, reliability is a statement about the stability of individual measures across replications from the same source of information (within subjects in this case). For example, a respondent to the questionnaire who indicated that his or her overall organization had sales of \$5 billion plus but only 500-749 employees would almost certainly have misinterpreted one or both of the questions. If enough respondents were inconsistent in their answers to these items, for this and other reasons, the items would contain abnormally high measurement error and hence, unreliable measures. Contrariwise, if individual measures are reliable for the most part, the entire instrument can be said to have minimal measurement error. Findings based on a reliable instrument are better supported, and parameter estimates are more efficient.

Coefficients of internal consistency and alternative forms were used to test the instrument. Both are explained in depth in Churchill (1979) and Bailey (1978). Cronbach alphas reported in the diagonal of the MTMM matrix pass the .80 rule-of-thumb test used as a gauge for reliable measures.

Phase III: Pilot test of reliability and construct validity

To further validate the instrument, a pilot survey of randomly selected DPMA (Data Processing Management Association) members was carried out in January of 1985. Judging from the 170 returned questionnaires, the pilot test once again confirmed that measurement problems in the instrument were not seriously disabling.

The instrument was first tested for reliability using Cronbach alphas and Pearson correlations. The variables presented in the MTMM analysis passed the .80 rule-of-thumb test with coefficients of .938, .934, .982, and .941.

Construct validity is assessed in a pilot instrument by establishing the factorial validity (Allen and Yen, 1979) of constructs. This technique, which has been utilized in the UIS (e.g., Ives, et al., 1983) and IS job satisfaction arenas (e.g., Ferratt and Short, 1986), has become popular as a result of the sophisticated statistical capabilities of factor analysis. Factor analysis allows the researcher to answer questions about which measures covary in explaining the highest percentage of the variance in a dataset. The researcher can approach the question with principal components factor analysis or confirmatory factor analysis.¹³ Thus, factorial validity helps to confirm that a certain set of measures do or do not reflect latent constructs.

A principal components factor analysis of the same subset of variables illustrated in the MTMM analysis shows that measures of the computer security deterrent construct (items 9, 10, 11, and 14) all contribute heavily to or "load" on a single factor, as shown in Table 4. Once the first factor is extracted, the analytical technique attempts to find other factors or sets of variables that best explain variance in the dataset. After 4 such extractions (with eigenvalues of at least 1.0), the selected measures load at a .5 cutoff level; together the loadings explain 97 percent of the variance in the dataset. In brief, results of this test support the view that measures of deterrence in the questionnaire are highly interrelated and do constitute a construct.

Table 4. Loadings for Factor Analysis of Pilot Instrument

	Factor 1	Factor 2	Factor 3	Factor 4
Item 9	.972			
Item 10	.943			
Item 11	.939			
Item 14	.863			
Item 2		.956		
Item 19		.910		
Item 21		.885		
Item 27		.847		
Item 34			.961	
Item 29				.897

¹³ One of the most sophisticated measurement model factor analysis tools is LISREL.

Besides their use in validation, pilot tests are also desirable because they provide a testing ground or dry run for final administration of the instrument. In this case, for example, some items were easily identified as problematic by virtue of their lack of variance, or what are frequently called "floor" or "ceiling" effects. Items 8 and 9 were originally scaled in such a way that answers tended to crowd together at the high end of the scale, i.e., the scaling of these items did not allow for discrimination among large organizations.

Other problems that need to be resolved can also surface during pilot testing. A good example in this validation was an item on "Annual cost of computer security insurance." Most respondents in the field interviews assumed that even though their organization did not have specific insurance coverage for violations of computer security, others did. And they also assumed that such costs could be estimated. Given the media attention to security insurance matters in the last several years, these assumptions are quite understandable. The pilot survey data clearly showed that the question was badly misjudged for content; there was virtually no variance at all on this item. Over 95 percent of the respondents left this item blank; some of those that did respond indicated through notations in the margins that they were not sure what the question meant.

The overall assessment of these validation tests was that the instrument had acceptable measurement properties. It meant, in essence, that much greater confidence could be placed in later use of the instruments.

Phase IV: Full scale victimization survey

The full scale implementation of the survey instrument resulted in 1,211 responses and 259 separate incidents of computer abuse. As a result of the work undertaken prior to this phase of the research, it was felt that stronger inferences could be made about causal relationships among variables and about theory.

Guidelines for Validating Instruments in MIS Research

Clearly, numerous contingencies affect the collection and evaluation of research data. In a tech-

nology-driven field such as MIS, windows of opportunity for gathering data appear and disappear so rapidly that researchers often feel they cannot afford the time required to validate their data collection instruments. Researchers engaging in initial studies or exploratory studies such as case studies may feel that validated instruments are not critical. Experimentalists concentrating on legitimate concerns of internal validity (Jarvenpaa, et al., 1984), moreover, may not realize that their pre- and posttreatment data-gathering instruments are, in fact, *sine qua non* means of controlling extraneous sources of variation.

Nevertheless, it is desirable for MIS as a field — experimentalists and case researchers not excepted (Campbell, 1975; Fromkin and Streufert, 1976) — to apply more rigorous scientific standards to data collection procedures and instruments (Mitchell, 1985). As an initial step, a set of minimal guidelines may be considered as follows:

- Researchers should pretest and/or pilot test instruments, attempting to assess as many validities as possible in this process.
- MIS journal editors should encourage or require researchers to prepare an "Instrument Validation" subsection of the Methodology section; this subsection can include, at the least, reliability tests and factorial validity tests of the final administered instrument.

As instrumentation issues become more internalized in the MIS research process, more stringent standards can be adopted as follows:

- Researchers should use previously validated instruments wherever possible, being careful not to make significant alterations in the validated instrument without revalidating instrument content, constructs, and reliability.
- Researchers should undertake technical validation whenever it is critical to ground central constructs in major MIS research streams.

Validating Instruments for Use in MIS Research Streams

There are numerous research streams in MIS that would gain considerable credibility from more carefully articulated constructs and meas-

ures. System success, as probably the central performance variable in MIS, is a prime candidate. It is the effect sought in large scale transactional processing systems (Ives and Olson, 1984), decision support systems (Poole and DeSanctis, 1987), and user-developed applications (Rivard and Huff, 1988). Although instruments measuring certain dependent variables such as UIS have been subjected to validation (e.g., Ives, et al., 1983), there have been few, if any, validations of instruments measuring other components of system success (such as system acceptance or systems usage), or independent variables (such as user involvement) (Ives and Olson, 1984). Varying components of user involvement have been tested, but no validation of the *construct* of user involvement has yet been undertaken. Recent studies (Baronas and Louis, 1988; Tate and Vessey, 1988; Yaverbaum, 1988) have employed altered forms of previously validated instruments (i.e., the Baroudi, Ives, and Olson instrument and Job Diagnostic Survey), but the only tests of the user involvement construct have been reliability statistics.

Basic macro-level constructs in the field, constructs like "information" and "information value," are still in need of validation and further refinement. Epstein and King (1982), Larcker and Lessig (1980), O'Reilly (1982), Swanson (1974; 1986; 1987), and Zmud (1978) all deal with important questions surrounding data, information, information attributes, and information value. But until Goodhue (1988), no technical validation effort (including MTMM analysis) had been undertaken to clarify this stream of research.

There are, in fact, whole streams of research in the field where primary research instruments remain unverified. Research programs exploring the value of software engineering techniques (Vessey and Weber, 1984), life cycle enhancements such as prototyping (Jenkins and Byrer, 1987), and factors affecting successful end-user computing (Brancheau, 1987) are all cases in point. MIS research, moreover, suffers from measurement problems in exploring variable relationships among variables such as information quality, secure systems, user-friendly systems, MIS sophistication, and decision quality.

Conclusion

Instrument validation is a prior and primary process in confirmatory empirical research. Yet, in

spite of growing awareness within the MIS field that methodologies need to be more rigorous, most of the empirical studies continue to use largely unvalidated instruments. Even though MIS researchers frequently adopt instruments that have been used in previous studies, a methodological approach that can significantly undergird the research effort, these advantages are lost in most instances either because the adapted instrument itself has not been validated or because the researcher has made major alterations in the validated instrument without retesting it.

It is important for MIS researchers to recognize that valid statistical conclusions by no means ensure that a causal relationship between variables exists. It is also important to realize that, in spite of the need to warranty internal validity, this validation does not test whether the research instrument is measuring what the researcher intended to measure. Measurement problems in MIS can only be resolved through instrument validation.

This article argues that instrument validation at any level can be of considerable help to MIS researchers in substantiating their findings. Specific guidelines for improvements include pre and pilot testing, formal validation procedures, and, finally, close imitation of previously validated instruments. As demonstrated in the case of the Computer Abuse Measurement Project, instrument pretests can be useful in *qualitatively* establishing the reliability, construct validity, and content validity of measures. Formal validation, either MTMM analysis or factor analysis, offers statistical evidence that the instrument itself is not seriously interfering with the gathering of accurate data. Pilot tests can permit testing of reliability and construct validity, identify and help correct scaling problems, and serve as dry runs for final administration of the instrument.

Improving empirical MIS research is a two part process. First, we must recognize that we have methodological problems that need to be addressed, and second, and even more important, we must take action by incorporating instrument validation into our research efforts. Serious attention to the issues of validation can move the field toward meaningfully replicated studies and refined concepts and away from intractable constructs and flawed measures.

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About the Author

Detmar W. Straub is assistant professor of management information systems at the University of Minnesota where he teaches courses in systems and pursues research at the Curtis L. Carlson School of Management. He joined the Minnesota faculty in September, 1986 after completing a dissertation at Indiana University. Professor Straub has published a number of stud-

ies in the computer security management arena, but his research interests also extend into information technology forecasting, measurement of key IS concepts, and innovation and diffusion theory testing. His professional associations and responsibilities include: associate director, MIS Research Center, University of Minnesota; associate publisher of the *MIS Quarterly*; editorial board memberships; and consulting with the defense and transportation industries.

APPENDIX

Section I. Computer Abuse Questionnaire

Personal Information

1. YOUR POSITION:

- President/Owner/Director/Chairman/Partner
- Vice President/General Manager
- Vice President of EDP
- Director/Manager/Head/Chief of EDP/MIS
- Director/Manager of Programming
- Director/Manager of Systems & Procedures
- Director/Manager of Communications
- Director/Manager of EDP Operations
- Director/Manager of Data Administration
- Director/Manager of Personal Computers
- Director/Manager of Information Center
- Data Administrator or Data Base Administrator
- Data/Computer Security Officer
- Senior Systems Analyst
- Systems/Information Analyst
- Chief/Lead/Senior Applications Programmer
- Applications Programmer
- Chief/Lead/Senior Systems Programmer
- Systems Programmer
- Chief/Lead/Senior Operator
- Machine or Computer Operator

- Vice President of Finance
- Controller
- Director/Manager Internal Auditing or EDP Auditing
- Director/Manager of Plant/Building Security
- EDP Auditor
- Internal Auditor
- Consultant
- Educator
- User of EDP
- Other (please specify): _____

2. YOUR IMMEDIATE SUPERVISOR'S POSITION:

- President/Owner/Director/Chairman/Partner
- Vice President/General Manager
- Vice President of EDP
- Director/Manager/Head/Chief of EDP/MIS
- Director/Manager of Programming
- Director/Manager of Systems & Procedures
- Director/Manager of Communications
- Director/Manager of EDP Operations
- Director/Manager of Data Administration
- Director/Manager of Personal Computers
- Director/Manager of Information Center
- Data/Computer Security Officer
- Senior Systems Analyst
- Chief/Lead/Senior Applications Programmer
- Chief/Lead/Senior Systems Programmer
- Chief/Lead/Senior Machine or Computer Operator

- Vice President of Finance
- Controller
- Director/Manager Internal Auditing or EDP Auditing
- Director/Manager of Plant/Building Security

- Other (please specify): _____

3. NUMBER OF TOTAL YEARS EXPERIENCE IN/WITH INFORMATION SYSTEMS?

- More than 14 years
- 11 to 14 years
- 7 to 10 years
- 3 to 6 years
- Less than 3 years
- Not sure

Organizational Information

4. Approximate ASSETS and annual REVENUES of your organization:

ASSETS		REVENUES	
At all Locations	At this Location	At all Locations	At this Location
<input type="checkbox"/>	<input type="checkbox"/>	Over 5 Billion	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	1 Billion-5 Billion	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	250 Million-1 Billion	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	100 Million-250 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	50 Million-100 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	10 Million-50 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	5 Million-10 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	2 Million-5 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	1 Million-2 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	Under 1 Million	<input type="checkbox"/>
<input type="checkbox"/>	<input type="checkbox"/>	Not sure	<input type="checkbox"/>

5. NUMBER OF EMPLOYEES of your organization:

	At all Locations	At this Location
10,000 or more	<input type="checkbox"/>	<input type="checkbox"/>
5,000-9,999	<input type="checkbox"/>	<input type="checkbox"/>
2,500-4,999	<input type="checkbox"/>	<input type="checkbox"/>
1,000-2,499	<input type="checkbox"/>	<input type="checkbox"/>
750-999	<input type="checkbox"/>	<input type="checkbox"/>
500-749	<input type="checkbox"/>	<input type="checkbox"/>
250-499	<input type="checkbox"/>	<input type="checkbox"/>
100-249	<input type="checkbox"/>	<input type="checkbox"/>
6-99	<input type="checkbox"/>	<input type="checkbox"/>
Fewer than 6	<input type="checkbox"/>	<input type="checkbox"/>
Not sure	<input type="checkbox"/>	<input type="checkbox"/>

6. PRIMARY END PRODUCT OR SERVICE of your organization at this location:

- Manufacturing and Processing
- Chemical or Pharmaceutical
- Government: Federal, State, Municipal including Military
- Educational: Colleges, Universities, and other Educational Institutions
- Computer and Data Processing Services including Software Services, Service Bureaus, Time-Sharing and Consultants
- Finance: Banking, Insurance, Real Estate, Securities, and Credit
- Trade: Wholesale and Retail
- Medical and Legal Services
- Petroleum
- Transportation Services: Land, Sea, and Air
- Utilities: Communications, Electric, Gas, and Sanitary Services
- Construction, Mining, and Agriculture
- Other (please specify): _____

Are you located at Corporate Headquarters: Yes No

Validating Research Instruments

7. CITY (at this location)? _____ STATE? _____
8. TOTAL NUMBER OF EDP (Electronic Data Processing) EMPLOYEES at this location (excluding data input personnel):
- | | |
|--|--|
| <input type="checkbox"/> More than 300 | <input type="checkbox"/> 50-99 |
| <input type="checkbox"/> 250-300 | <input type="checkbox"/> 10-49 |
| <input type="checkbox"/> 200-249 | <input type="checkbox"/> Fewer than 10 |
| <input type="checkbox"/> 150-199 | <input type="checkbox"/> Not sure |
| <input type="checkbox"/> 100-149 | |
9. Approximate EDP BUDGET per year of your organization at this location:
- | | |
|--|--|
| <input type="checkbox"/> Over \$20 Million | <input type="checkbox"/> \$2-\$4 Million |
| <input type="checkbox"/> \$10-\$20 Million | <input type="checkbox"/> \$1-\$2 Million |
| <input type="checkbox"/> \$8-\$10 Million | <input type="checkbox"/> Under \$1 Million |
| <input type="checkbox"/> \$6-\$8 Million | <input type="checkbox"/> Not sure |
| <input type="checkbox"/> \$4-\$6 Million | |

Computer Security, Internal Audit, and Abuse Incident Information

A Computer Security function in an organization is any purposeful activity that has the objective of protecting assets such as hardware, programs, data, and computer service from loss or misuse. Examples of personnel engaged in computer security functions include: data security and systems assurance officers. For this questionnaire, computer security and EDP audit functions will be considered separately.

- | | Computer Security | EDP Audit |
|---|---------------------------|---------------------------|
| 10. How many staff members are working 20 hours per week or more in these functions at this location? | _____ (number of persons) | _____ (number of persons) |
| 11. How many staff members are working 19 hours per week or less in these functions at this location? | _____ (number of persons) | _____ (number of persons) |
| 12. What are the total personnel hours per week dedicated to these functions? | _____ (total hours/wk) | _____ (total hours/wk) |
| 13. When were these functions initiated? | ____/____ (month/yr) | ____/____ (month/yr) |

If your answer to the Computer Security part of question 12 was zero, please go directly to question 25. Otherwise, continue.

14. Of these total computer security personnel hours per week (question 12), how many are dedicated to each of the following?
- A. Physical security administration, disaster recovery, and contingency planning _____ (hours/week)
- B. Data security administration _____ (hours/week)
- C. User and coordinator training _____ (hours/week)
- D. Other _____ (hours/week)
(please specify): _____
15. EXPENDITURES per year for computer security at this location:
- Annual computer security personnel salaries \$ _____
- Do you have insurance (separate policy or rider) specifically for computer security losses?
 Yes No Not sure
- If yes, what is the annual cost of such insurance . . . \$ _____
16. SECURITY SOFTWARE SYSTEMS available and actively in use on the mainframe(s) [or minicomputer(s)] at this location:
- | | | |
|--|------------------------------|---------------------------|
| | Number of available systems? | Number of systems in use? |
| Operating system access control facilities. . . | _____ | _____ |
| DBMS security access control facilities | _____ | _____ |
| Fourth Generation software access control facilities | _____ | _____ |

17. Other than those security software systems you listed in question 16, how many SPECIALIZED SECURITY SOFTWARE SYSTEMS are actively in use? (Examples: ACFII, RACF)
- _____ (number of specialized security software systems actively in use)
- Of these, how many were purchased from a vendor? _____
(number purchased from a vendor)
- ... and how many were developed in-house? _____
(number developed in-house)

18. Through what INFORMATIONAL SOURCES are computer system users made aware OF THE APPROPRIATE AND INAPPROPRIATE USES OF THE COMPUTER SYSTEM?
(Choose as many as applicable)
- Distributed EDP Guidelines
 - Administrative program to classify information by sensitivity
 - Periodic departmental memos and notes
 - Distributed statements of professional ethics
 - Computer Security Violations Reports
 - Organizational meetings
 - Computer Security Awareness Training sessions
 - Informal discussions
 - Other (please specify): _____
19. Which types of DISCIPLINARY ACTION do these informational sources mention (question 18) as consequences of purposeful computer abuse?
(Choose as many as applicable)
- Reprimand
 - Probation or suspension
 - Firing
 - Criminal prosecution
 - Civil prosecution
 - Other (please specify): _____

In questions 20-24, please indicate your reactions to the following statements:

- | | Strongly Agree | Agree | Not Sure | Disagree | Strongly Disagree |
|--|--------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| 20. The current computer security effort was in reaction in large part to actual or suspected past incidents of computer abuse at this location. | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |
| 21. The activities of computer security administrators are well known to users at this location. | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |
| 22. The presence and activities of computer security administrators deter anyone who might abuse the computer system at this location. | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |
| 23. Relative to our type of industry computer security is very effective at this location. | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |
| 24. The overall security philosophy at this location is to provide very tight security without hindering productivity. | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> | <input type="checkbox"/> |

25. How many SEPARATE UNAUTHORIZED AND DELIBERATE INCIDENTS OF COMPUTER ABUSE has your organization at this location experienced in the 3 year period, Jan. 1, 1983-Jan. 1, 1986?
_____ (number of incidents)

(Please fill out a separate "Computer Abuse Incident Report" [Blue-colored Section II] for each incident.)

26. How many incidents do you have reason to suspect other than those numbered above in this same 3 year period, Jan. 1, 1983-Jan. 1, 1986?
_____ (number of suspected incidents)
27. Please briefly describe the basis (bases) for these suspicions.
- _____
- _____
- _____

Section II.
Computer Abuse Incident Report
(covering the 3 year period, Jan. 1, 1983-Jan. 1, 1986)

Instructions: Please fill out a separate report for each incident of computer abuse that has occurred in the 3 year period, Jan. 1, 1983-Jan. 1, 1986

28. WHEN WAS THIS INCIDENT DISCOVERED?

Month/year ____ / ____

29. HOW MANY PEOPLE WERE INVOLVED in committing the computer abuse in this incident?

_____ (number of perpetrators)

30. POSITION(S) OF OFFENDER(S):

	Main Offender	Second Offender
Top executive	<input type="checkbox"/>	<input type="checkbox"/>
Security officer	<input type="checkbox"/>	<input type="checkbox"/>
Auditor	<input type="checkbox"/>	<input type="checkbox"/>
Controller	<input type="checkbox"/>	<input type="checkbox"/>
Manager, supervisor	<input type="checkbox"/>	<input type="checkbox"/>
Systems Programmer	<input type="checkbox"/>	<input type="checkbox"/>
Data entry staff	<input type="checkbox"/>	<input type="checkbox"/>
Applications Programmer	<input type="checkbox"/>	<input type="checkbox"/>
Systems analyst	<input type="checkbox"/>	<input type="checkbox"/>
Machine or computer operator	<input type="checkbox"/>	<input type="checkbox"/>
Other EDP staff	<input type="checkbox"/>	<input type="checkbox"/>
Accountant	<input type="checkbox"/>	<input type="checkbox"/>
Clerical personnel	<input type="checkbox"/>	<input type="checkbox"/>
Student	<input type="checkbox"/>	<input type="checkbox"/>
Consultant	<input type="checkbox"/>	<input type="checkbox"/>
Not sure	<input type="checkbox"/>	<input type="checkbox"/>
Other	<input type="checkbox"/>	<input type="checkbox"/>

(please specify): (Main) _____

(Second) _____

31. STATUS(ES) OF OFFENDER(S) when incident occurred:

	Main Offender	Second Offender
Employee	<input type="checkbox"/>	<input type="checkbox"/>
Ex-employee	<input type="checkbox"/>	<input type="checkbox"/>
Non-employee	<input type="checkbox"/>	<input type="checkbox"/>
Not sure	<input type="checkbox"/>	<input type="checkbox"/>
Other	<input type="checkbox"/>	<input type="checkbox"/>

(please specify): (Main) _____

(Second) _____

32. MOTIVATION(S) OF OFFENDER(S):

	Main Offender	Second Offender
Ignorance of proper professional conduct. ...	<input type="checkbox"/>	<input type="checkbox"/>
Personal gain	<input type="checkbox"/>	<input type="checkbox"/>
Misguided playfulness	<input type="checkbox"/>	<input type="checkbox"/>
Maliciousness or revenge	<input type="checkbox"/>	<input type="checkbox"/>
Not sure	<input type="checkbox"/>	<input type="checkbox"/>
Other	<input type="checkbox"/>	<input type="checkbox"/>

(please specify): (Main) _____

(Second) _____

33. MAJOR ASSET AFFECTED or involved:
(Choose as many as applicable)

- Unauthorized use of computer service
- Disruption of computer service
- Data
- Hardware
- Programs

34. Was this a one-time incident or had it been going on for a period of time?

- (Choose one only)*
- one-time event
 - going on for a period of time
 - not sure

35. If a one-time incident, WHEN DID IT OCCUR?

Month _____ Year _____

36. If the incident had been going on for a period of time how long was that?

_____ years _____ months

37. In your judgment, how serious a breach of security was this incident?

- (Choose one only)*
- Extremely serious
 - Serious
 - Of minimal importance
 - Not sure
 - Of negligible importance

38. Estimated \$ LOSS through LOST OPPORTUNITIES (if measurable): *(Example: \$3,000 in lost business because of data corruption)*

\$ _____
 (estimated \$ loss through lost opportunities)

39. Estimated \$ LOSS through THEFT and/or RECOVERY COSTS from abuse: *(Example: \$12,000 electronically embezzled plus \$1,000 in salary to recover from data corruption + \$2,000 in legal fees = \$15,000)*

\$ _____
 (estimated \$ loss through theft and/or recovery costs)

40. This incident was discovered...

- (Choose as many as applicable)*
- by accident by a system user
 - by accident by a systems staff member or an internal/EDP auditor
 - through a computer security investigation other than an audit
 - by an internal/EDP audit
 - through normal systems controls, like software or procedural controls
 - by an external audit
 - not sure
 - other (please specify): _____

41. This incident was reported to...

- (Choose as many as applicable)*
- someone inside the local organization
 - someone outside the local organization
 - not sure

42. If this incident was reported to someone outside the local organization, who was that?

- (Choose as many as applicable)*
- someone at divisional or corporate headquarters
 - the media
 - the police
 - other authorities
 - not sure

43. Please briefly describe the incident and what finally happened to the perpetrator(s).
