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á³á Validating Instruments in MIS Research¼* á³á

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A B S T R A C Tœ

Calls for new directions in MIS research bring with them a call for renewed methodological rigor. This paper 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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I. Introduction

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

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 nonempirical methodologies which have dominated MIS research (Hamilton and Ives, 1982b; Farhoomand, 1988) may not require the same level of validation.â   ÿ
X Cf. Campbell, 1975, however. Benbasat et al., 1987, also deal with this question.â

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 raised about rigor in MIS methodology in general (McFarlan, 1984). McFarlan and other key researchers in the field (e.g., Hamilton and Ives, 1982a and 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 ofÜj   Ü research--streams of opportunity--in both the near and the far term (McFarlan, 1984; Jenkins, 1983). This should be carried out, whenever possible, in a systematic and programmatic manner (Jenkins, 1984, 1983; Hunter, et al., 1982; 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., 1984) in permitting confirmatory, followup research to utilize a tested instrument. By permitting other researchers in the stream to use this tested instrument across heterogenous settings and times, it 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 will also bring 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 measuresœ in confirmatory research raises the specter that no single finding in the study can be trustedœ. In many cases thisÜd Ü
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 confirmatoryâ ÿ
X Strictly speaking, and from the point of view of Strong Inference, no scientific explanation is ever confirmed (Popper, 1959; Blalock, 1969); incorrect explanations are simply eliminated from consideration. The terminology is adopted here for the sake of convenience, however.â
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 phenomena, as many methodologists have pointed out (Glaser and Strauss, 1967; Lincoln and Gupta, 1985). This paper focuses on the methodologies and validation techniques most often found on the confirmatory side of the research cycle, as shown in Figure 1, "The Scientific Research Cycle." The key point here is that confirmatory research calls for rigorous instrument validation as well as quantitative analysis to establish greater confidence in its findings. Á Á

[Figure 1 about here]œ

II. Linking MIS Research and Validation Processes œ

Á ÁIn order to understand precisely what instrument validation is and how it
functions, it will be placed in the context of why researchers, and 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,Üd Ü
causal 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).
Á ÁÁ Á 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 (Ricketts and Jenkins, 1985; Ives, Olson, and Baroudi, 1983; Bailey and Pearson, 1983).

″ The Place of Theory in Confirmatory Researchœ•

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 preªspecify 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). Üd ÜŒ 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 may already well specified in the reference disciplines, and available for application to MIS. This point has been effectively made by Dickson, Benbasat, and King (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 Constructsœ•

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, Olson, and Baroudi, 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 theÜd Ü instrument when items are ambiguously phrased, length of the instrument taxes respondents' concentration (Ives, Olson, and Baroudi, 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 (Coombs, 1964; Cook and Campbell, 1979).

In a perfectly valid UIS instrument, data measurements will completely and accurately reflect the unobservable research construct, system success. Given real world limitations, however, some inaccurate measurements will inevitably obtrude on the translation process. The primary question for MIS researchers is to what extent 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• œ

Á Á In the MIS research process, instrument validation should precede other core empirical validities (Cook and Campbell, 1979),â ÿ
X 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 will not be discussed here.â which are set forth
according to the kinds of questions they answer in Figure 2, ""Questions Answered by the Validities." Researchers and those who are going to utilize confirmatory research findings need first 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 (Reichardt, 1979; Hair, et al., 1979; Hanushek and Jackson, 1977; Lindman, 1974).
IfÜd Ü
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.

[Figure 2 about here]œ

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 the universe of questions available to answer selected research questions (Cronbach, 1971; Kerlinger, 1964). With representative content, the instrument will 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 would expect relatively high correlations between measures of the same construct using different methods and low correlations between measures of constructs that are expected to
differÜd Ü
(Campbell and Fiske, 1959). The construct validity of an instrument can be assessed through MTMM (multitrait-multimethod) techniques (Campbell and Fiske, 1959) or techniques such as confirmatory or principal components factor analysis (Nunnally, 1967; Long, 1983).â ÿ
X 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.â 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 converge 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 is evidence that the measure has ”discriminant validity•.â ÿ

X 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.â

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 could occur in a case where large numbers of students unthinkingly check off entire columns in a course evaluation. Were these same students 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 will 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 as a variation of the true score. ”Reliability•, hence, is essentially an evaluation of measurement accuracy, e.g., the extent to which the respondent can answer the sameÜd Ü questions or close approximations the same way each time (Cronbach, 1951). High correlations between alternative measures or large Cronbach alphas are usually signs that the measures are reliable.Á Á

” Internal Validityœ•

Á Á Internal validity asks whether the observed effects could have been caused by or correlated with a set of unhypothesized and/or unmeasured variables. In short, it asks if there are 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, Dickson, and DeSanctis (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 easily be misdefined if the instrumentation is invalid. This point can be 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,Üd Ü
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.â ÿ X Cf. Jenkins and Ricketts, 1985, on this point.â Under 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. The findings, however, would be moot because the edifice was raised on sand.Á Á Á Á

” Statistical Conclusion Validityœ•

Á Á 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 (Cohen, 1969, 1977; Baroudi and Or-likowski, 1986; 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 sig¬nificance (Cohen, 1969). Nonsignificant results from tests with low power, i.e., aÜf Ü probability of less than 80% 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).

By way of summary, it is possible to show the overall results of violating order or position in the validation process. Figure 3, "Outcomes from Omitted Validities," 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, 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.
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[Figure 3 about here]œ

Üf ÜŒ IV. The Need for Instrument Validation in MISœ

Á Á Before elaborating a demonstration exercise of instrument validation, it is important to establish that instruments in the MIS literature are presently, as contended, 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 employed 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 Resultsœ•

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% of the studies lacked even a single form of instrument validation. Figure 4 summarizes other key findings of the survey.

[Figure 4 about here]œ

As percentages in Figure 4 indicate, MIS researchers rely most frequently (17% of the studies) on previously utilized instruments as a primary means of, presumably, validating their instruments. Almost without exception, however,Üd Ü 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 validation standpoint.â ÿ X 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, in that nomological validity usually occurs only in a long and well established stream of research, a situation that does not apply in this case.â The second problem arises 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 point of fact, the more the format, order, wording, and procedural setting of the original instrument is changed, the greater is the likelihood that the derived instrument lacks validated qualities of the original instrument.

The remainder of the descriptive statistics in Figure 4 paint a similarly disturbing picture. Reliability is the most frequently assessed validity, but even in this case, some 83% of the studies do not test this minimal level of validation. Reliability is infrequently coupled with tests of construct validity (less than 16% of the studies) and assessment of content validity are almost unheard of (5% 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 was 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 that are used to gatherÜd Ü
data before and after the treatment are seldom validated. Instruments developed for case studies were 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% of researchers in the administrative sciences report reliability statistics, as compared with 17% in MIS, and twice as many researchers validate constructs in this reference discipline field (Mitchell, 1985).

Á Á V. A Demonstration Exercise of Instrument Validation in MISœ

Á Á 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 will exemplify 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.â ÿ

X 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 International DPMA (Data Processing Management Association).â

Computer abuse was operationally defined as misuse of information system assets such as programs, data, hardware, and computer service, and 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 I/S security and control (Brancheau and Wetherbe, 1987; Canning, 1986; Dickson, et al., 1984; Sprague and McNurlin, 1986). Organizations respond to this risk from abuse byÜd Ü
attempting to: a) deter abusers through countermeasures such as strict sanctions against misuse (these programs being 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 preªspecified 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 generally (e.g., the National Crime Survey)â
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X Cf. Skogan, 1981.â and in
prior computer abuse studies (AICPA, 1984; Colton, 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.

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 aÜd Ü 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 60's, 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 distributing information about organizational policies regarding system usage, or what we have been referring 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). Figure 5, "Concepts, Constructs, and Measures," presents the pertinent connections between the conceptualÜd Ü
terminology we have been using, the constructs most frequently cited in General Deterrence theory, and the actual items as measured in the final instrument, which appears in the Appendix.

[Figure 5 about here]œ

Á ÁÁ Á   Evolving directly from prior instruments, a draft instrument was first
constructed to reflect study constructs. Following this, instrument validation took place in three, well-defined operations and the entire research process in four phases, as outlined in Figure 6, "Phases of the Computer Abuse Measurement Project."

[Figure 6 about here]œ

″ Phase I: Pre©Testœ•

In the pre©test, 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 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 includedÜd Ü
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 semiªstructured 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 being evaluated. In the beginning of the interview, participants were encouraged to be discursive. Participants 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 well as the precise language in which these constructs were perceived by the participants (content validity and reliability). In the second, semi-structured 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 alsoÜd Ü
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 validities. 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, then, the draft instrument had undergone a dramatic metamorphosis. It collected data in every class of the relevant variables. To provide sufficient data points to assess reliability, several measures of each significant independent variable were included as well as several for each dependent variable.

” Phase II: Formal Validationœ•

In keeping with the project plan, formal instrument validation occurred during the Fall of 1984 and the Winter of 1985 (Phase II). Its purpose was to validate, in order of importance: 1) construct validity, 2) reliability, and 3) content validity. 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 theÜd Ü
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 these several methodological dimensions. These dimensions and analytical techniques are summarized in Figure 6, "Phases of the Computer Abuse Measurement Project."
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″ Phase II: Formal Validation of Construct Validityœ•

Á Á Tests of construct validity are generally intended to determine if measures across subjects are similar over 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 pre-tested 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 time frame. A limited amount of consultation was permitted, but, in general, respondents wereÜd Ü
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 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. High method variance, conversely, indicates that measures are more a function of the instrumentation than of underlying constructs. As an analytical technique, Campbell and Fiske's MTMM (multitrait-multimethod) allows patterns of selected, individual measures to be compared through a correlation matrix of measures by methods, as in Figure 7, "MTMM Matrix for Victimization Instruments."â
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X The matrix in Figure 6 is only a partial matrix of all the correlations evaluated. Matrix elements here were chosen simply for purposes of demonstration.
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Analysis of the MTMM matrix in Figure 7 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 is greater than other entries in its row and column.

Üd ÜŒ [Figure 7 about here]œ

In the MTMM test, the test instrument failed to achieve convergent validity on 3 out of the 11 items (items 2, 19 and 21) as evidenced by low, nonsignificant 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 nonsignificant 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 persons in the same organization can give very similar objective answers about security efforts (e.g., traits 9, 10, 11, and 14), and reasonable to expect them to vary on opinions about the effectiveness of these efforts. And the data unveils this pattern.

Whereas the instrument, upon inspection, does pass the Campbell©Fiske first 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 (Marsh and Hocevar, 1983; Farh, et al., 1984).
Üd Ü
For one thing, it is well known that high but spurious correlations will occur by chance alone in large matrices like this one of 231 elements. Yet, in spite of reasonable or otherwise ingenious explanations that can be offered for offªdiagonal high correlations, it may be more straight©forward 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 may 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 heteroªmethod 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 paper and pencil 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 suggests 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. It is likely that there is a systematic effect due to person, apart from the effect

of method ”per se•, that accounts for this pattern of findings.â
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X 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).â 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 estimateÜd Ü
of staff to correlate more highly with their own estimate of personnel hours than with someone else's estimate of staff. If so, 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 criterion for "maximally different methods," common method variance detracts from the "relative validity" (p. 84) that is generally found throughout the matrix. Had both methods been administered to each respondent as one solution to evaluating common method variance, though, a new source of confounding©©a test©retest bias (Cook and Campbell, 1979)©©would have been introduced.

” Phase II: Formal Validation of Reliabilityœ•

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 more efficient.

Coefficients of internal consistency and alternative forms were used toÜd Ü
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• œ

Á Á 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, Olson, and Baroudi, 1983) and I/S 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.â

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X One of the most sophisticated of the measurement model factor analysis tools is LISREL.â Factorial validity, hence, 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, orÜd   Ü
"load" on a single factor, as shown in Figure 8, "Loadings for Factor Analysis of Pilot Instrument." 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 loaded at a .5 cutoff level; together the loadings explain 97% of the variance in the dataset. In brief, results of this test support the view that measures of deterrence in the questionnaire are highly inter©related, and do constitute a construct.

[Figure 8 about here]œ

Á Á 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 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 would. 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.
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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% of the respondents left this item blank; some of those that did respond indicated through notations in the margins that they weren't 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 findings from the 1211 returns in the full-scale survey in Phase IV of the Computer Abuse Measurement Project.Á Á

VI. Guidelines for Validating Instruments in MIS Researchœ

Á Á Clearly, numerous contingencies affect the collection and evaluation of research data. In a technology©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 post treatment 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 (Fromkin and Streufert, 1976; Campbell, 1975) 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:Üd ÜŒÁ Á

- o Researchers should pretest and/or pilot test instrumentsœ, attempting to assess as many validities as possible in this process

- o MIS Journal editors should encourage or require researchers to prepare an "Instrument Validation" sub©section of the Methodology sectionœ; this sub©section can include, at the least, reliability tests and factorial validity tests of the final administered instrument

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

- o Researchers should use previously validated instrumentsœ wherever possible, being careful not to make significant alterations in the validated instrument without revalidating instrument content, constructs, and reliability

- o Researchers should undertake formal validationœ whenever it is critical to ground central constructs in major MIS research streams

VII. Validating Instruments for Use in MIS Research Streamsœ

Á Á There are numerous research streams in MIS that would gain considerable credibility from more carefully articulated constructs and measures. 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, Olson, and Baroudi, 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 (Tait and Vessey, 1988;Üf Ü
Baronas and Louis, 1988; Yavenbaum, 1988) have employed altered forms of previously validated instruments (i.e., the Baroudi, Ives, and Olson instrument and the 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. Swanson (1974, 1986, 1987), Zmud (1978), Larcker and Lessig (1980), O'Reilly (1982), Epstein and King (1982) all deal with important questions surrounding data, information, information attributes, and information value, but until

Goodhue (1988), no formal 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.

Á Á VIII. Conclusion œ

Á Á Instrument validation is a prior and primaryœ process in confirmatory”• empirical research. Yet, in spite of growing awareness within the MIS field that our methodologies need to be more rigorous, most of our empirical studies continue to use largely unvalidated instruments. Even though MIS researchers frequently adopt instruments that have been used in previous studies, aÜd Ü methodological approach which 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 paper 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 pre©tests 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 theÜd Ü
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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Ã 1-ÃFigure 1. The Scientific Research Cycleƒ
(based on McGrath, 1979 and
Mackenzie and House, 1979)

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Ã 1 Ã Figure 2. Questions Answered by the Validitiesœ ƒÜ6 Ü

Figure 3. Outcomes from Omitted Validities

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   á³á                                                                     á³á
   á³á   Cases:  Total = 117   ”MISQ• = 43  ”CACM• = 28  ”Info&Mgmt• = 46
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   á³á                                                                     á³á
   á³á                                                                     á³á
   á³á   1. Pretest        15 Yes   102 No    (13% Yes  87% No)            á³á
   á³á                                                                     á³á
   á³á   2. Pilot           7 Yes   110 No     (6% Yes  94% No)            á³á
   á³á                                                                     á³á
   á³á   3. Pretest or pilot    22 Yes    95 No    (19% Yes  81% No)       á³á
   á³á                                                                     á³á
   á³á   4. Previously utilized     20 Yes    97 No    (17% Yes  83% No)   á³á
   á³á       instrument                                                    á³á
   á³á                                                                     á³á
   á³á   5. Content validity     5 Yes   112 No     (4% Yes  96% No)       á³á
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   á³á   6. Construct validity    16 Yes   101 No    (14% Yes  86% No)     á³á
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   á³á   7. Reliability    20 Yes    97 No    (17% Yes  83% No)            á³á
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                     Figure 4. Summary of Survey Resultsœ

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Concepts	Research Construct	Survey Item	Measure Description
		25	-Number of incidents
Abuse	DAMAGE	39	-Actual dollar loss
		38	-Opportunity dollar loss
		37	-Subjective seriousness index
		10	-Full-time security staff
		11	-Part-time security staff
		12	-Total security hours/week
	DISINCENTIVES:	14b	-Data security hours/week
	CERTAINTY	15	-Total security staff salaries
Deterrents		22	-Subjective deterrent effect
		13-35 &	-Longevity of security (from
		13-28-36	inception to incident date)
	DISINCENTIVES:	18	-Information about proper use
	SEVERITY	19	-Most severe penalty for abuse
		22	-Subjective deterrent effect
Preventives	PREVENTIVES	16	-Use of software access control
		17	-Use of specialized software

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á³á             á³á                 á³á   30   á³á -Privileged status of offender
á³á
á³á             á³á                 á³á   29   á³á -Amount of collusion
á³á
á³á  Rival      á³á ENVIRONMENTAL- á³á   32   á³á -Motivation of offender
á³á
á³á   Explana-  á³á  MOTIVATIONAL  á³á   31   á³á -Employee/non-employee status
á³á
á³á   tions     á³á    FACTORS     á³á   24   á³á -Tightness of security
á³á
á³á             á³á                 á³á   21   á³á -Visibility of security
á³á
á³á             á³á                 á³á 28-35 &á³á -Duration of abuse
á³á
á³á             á³á                 á³á   36   á³á
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Figure 5. Concepts, Constructs, and Measures

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Phase	Name	Validation Tests Performed	Content Validity	Construct Validity	Relia©bility
I	Pre-Test	Qualitative	X	X	X
II	Formal Validation	Cronbach alphas			X
		MTMM Analysis		X	X
III	Pilot Test	Cronbach alphas			X
		Factor Analysis		X	
		Qualitative	X		
IV	Full©scale Victimization Survey				

Ã 1 ÃFigure 6. Phases of the Computer Abuse Measurement Project ƒ

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œ Ã 1 ÃFigure 7. MTMM Matrix for Victimization Instrumentsœƒ

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	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

Figure 8. Loadings for Factor Analysis of Pilot Instrument

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Endnotes

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