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# An Assessment of Laboratory Experiments in Accounting

ROBERT J. SWIERINGA AND KARL E. WEICK\*

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What we call progress is the exchange of one nuisance for another.

—Havelock Ellis  
(1859–1939)

A reading of back issues of the major academic journals in accounting suggests an increasing number of laboratory experiments. Since 1970, the results of over 100 experiments have been published in *Accounting, Organizations and Society*, *The Accounting Review*, and the *Journal of Accounting Research*. These experiments have focused on the behavior of subjects in response to a wide variety of settings and stimuli.

Our intent here is to provide an assessment of laboratory experiments in accounting. We do not intend to provide a review of these experiments, since several reviews of experimental research in accounting are available. Dyckman, Gibbins, and Swieringa [1978] review and evaluate experimental and survey research in financial accounting, Gibbins [1977] reviews behavioral research in auditing, and Ashton [1982], Libby [1981], and Libby and Lewis [1977; 1982] review human information processing research in accounting.

Also, a review of the diverse accounting experiments would have to be very general. For example, Berelson used to summarize Berelson and Steiner [1964], which contained 1,045 established propositions about human behavior, with three propositions: (1) some do, some don't; (2) the differences aren't very great; and (3) it's more complicated than that. These propositions also may provide more than a casual summary of results from laboratory experiments in accounting.

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Our assessment proceeds as follows. In the first section, we provide a brief summary of laboratory experiments in accounting. A survey of experiments published in three academic accounting journals since 1970 reveals some trends in both the design and content of these experiments and provides the basis for a description and an assessment of major types of experiments. These experiments also provide some information about how experimenters tend to view the role and effects of accounting information.

In the second section, we reconsider the nature of experimentation. Laboratory experiments in accounting have become more restrictive in scope and design. This trend can legitimately be viewed as progress given earlier reviews and evaluations of experimental research (e.g., Gonedes and Dopuch [1974] and Dyckman, Gibbins, and Swieringa [1978]). However, this trend may also suggest that laboratory experiments are limited vehicles for observation. We discuss several trade-offs and subtleties of experiments for the purpose of encouraging experimenters to consider a more differentiated view of experiments and how to learn from them.

Finally, with some reluctance, we discuss what many regard as the most overwrought topic in experimentation, the issue of realism. We do so because accounting experimenters worry about realism. We distinguish between two types of realism: *mundane realism*—whether laboratory events are similar to real-world events— and *experimental realism*—whether laboratory events are believed, attended to, and taken seriously. Since all experimenters strive for experimental realism, the concern is about mundane realism. Should accounting experiments replicate real-world processes? Should these experiments be used to obtain population estimates? Is it important to have representative subjects and settings in these experiments?

The concern about mundane realism reflects our tendency to view experiments in terms of verification, hypothesis testing, and description. But, experiments also can be used for purposes of discovery and for developing and testing theory. Experiments can be used to create conditions that do not exist now and to address “what if” questions. For developing and testing theory, the artificiality of an experiment may facilitate a clean test of a theory, lack of random sampling may not be a disadvantage because it is the theory that facilitates generalization across actors and settings, and triviality becomes a substantive rather than a methodological issue. Mundane realism may make verification and discovery more difficult and less instructive in some experimental situations, but may be beneficial in testing complex theories and in providing common meanings in other experimental situations.

### *A Brief Survey of Past Laboratory Experiments*

The human actor does not react to an environment, he enacts it. . . . we can know what we've done only after we've done it. Only by doing is it possible for us to discover what we have done.

(Weick [1969, p. 64])

Accounting experimentalists have enacted an environment which can only be discovered after the fact. To obtain some sense of the nature of this environment, we developed a list of laboratory experiments published in *Accounting, Organizations and Society (AOS)*, *The Accounting Review (AR)*, and the *Journal of Accounting Research (JAR)* from 1970–81.<sup>1</sup> The list in Appendix A includes 113 articles.<sup>2</sup> We are intrigued by three features of these articles.

First, even though *JAR* is often viewed as the major and premier outlet for analytical and market-based research in accounting, it has increasingly become the major outlet for experimental research as well. Table 1 presents the number of laboratory experiments appearing in each journal for three four-year periods. Fifty-seven percent of the laboratory experiments published from 1970–81 appeared in *JAR* and its annual supplement, 27 percent appeared in *AR*, and 16 percent appeared in *AOS*.

*AR* was a major outlet for laboratory experiments from 1970–73; half of the articles appearing in *JAR* were in its annual supplements. Both *AR* and *JAR* were major outlets for this research from 1974–77, but *JAR* became the major outlet from 1978–81. Sixty-three percent of the laboratory experiments from 1978–81 appeared in *JAR* and its annual supplements, 25 percent appeared in *AOS*, and 13 percent appeared in *AR*. In fact, 28 percent of the pages in the Spring and Autumn issues of *JAR* from 1978–81 were devoted to laboratory experiments.<sup>3</sup>

Second, there has been a shift in the types of subjects used in laboratory experiments in accounting. Table 2 presents a summary of the types of subjects used in experiments for each four-year period. Over 70 percent of the experiments from 1970–73 used student subjects, either alone or with nonstudent subjects. This percentage dropped to 59 percent from 1974–77 and to 46 percent from 1978–81. Over two-thirds of the experiments from 1978–81 used nonstudent subjects, either alone or with student subjects. Of the experiments using nonstudent subjects either alone or with student subjects, 39 percent have used professional accountants, 29 percent have used lending officers, 18 percent have used managers, 8 percent have used financial analysts, and 6 percent have used others.<sup>4</sup>

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<sup>1</sup> We decided to limit our survey to articles from 1970–81 because Dyckman, Gibbins, and Swieringa [1978] and Gibbins [1977] focus much of their reviews and assessments on experiments published before 1970.

<sup>2</sup> This list was intended to be extensive but not necessarily exhaustive. Some experiments may have been excluded and some studies inappropriately included.

<sup>3</sup> A benchmark may be useful in assessing these percentages. Higbee, Millard, and Folkman [1982] found that experiments reported in four mainline social psychology journals increased, on average, from 67 percent in 1969 to 74 percent in 1979.

<sup>4</sup> Higbee, Millard, and Folkman [1982] report that, on average, 61 percent of the subjects in articles in four mainline social psychology journals in 1969 were college students and that this percentage increased to an average of 70 percent in 1979. Normal adults, on average, were used in only 18 percent of the articles. These percentages suggest that there may be some truth in Donald Campbell's statement that "a psychology of the college sophomore is better than no psychology at all."

Third, there has been a shift in the types of laboratory experiments undertaken. Table 3 presents a summary of the articles classified by three types of experiments for each four-year period.<sup>5</sup> The “simulations” category includes experiments which have used business games and business simulators, the “generic tasks” category includes experiments which have used analogues or abstract tasks, the “judgment tasks” category includes experiments which have used “realistic” judgment tasks, and the “other” category reflects essentially quasi-experimental studies. The use of simulations has dropped from 31 percent for 1970–73, to 8 percent for 1974–77, and to 6 percent for 1978–81. The use of generic tasks has increased slightly from 23 percent for 1970–73, to 26 percent for 1974–77, and to 29 percent for 1978–81. The use of judgment tasks has increased dramatically from 35 percent for 1970–73, to 66 percent for 1974–77, to 65 percent for 1978–81.

These three features combine to suggest the following profile of a typical laboratory experiment in accounting: a study published in *JAR* which focuses on the effects of accounting information on judgments or decisions of nonstudent judges or subjects. The following discussion, observations, and questions try to put this profile in perspective.

#### SIMULATIONS

Several studies have used various business games or simulators as a basis for laboratory experiments in accounting.<sup>6</sup> A representative simulation is Bollom's [1973] use of a business game to determine whether interim data for a seasonal business provided useful information to

<sup>5</sup> These classifications reflect judgments about articles which have multiple objectives and features. Some experiments could be included in more than one category.

<sup>6</sup> Dalton and Miner [1970] used a management game to assess the influence of accounting background on team performance. Pankoff and Virgil [1970] constructed a laboratory stock market in which closing subjects purchased information on expectations, predicted closing market prices, and chose portfolios. Dascher and Copeland [1971] used a general business simulation to assess the effects of allocation or nonallocation of common costs among segments of a conglomerate. Holstrum [1971] used a business game which reflected a maintenance shop to focus on the effects of budget adaptiveness and tightness of a budgetee's responsiveness to random fluctuations and to environmental changes. Barefield [1972] simulated a manufacturing operation to examine the ability of subjects to use aggregated versus disaggregated data. Mock, Estrin, and Vasarhelyi [1972], Mock [1973], and Driver and Mock [1975] used simulated business settings to explore the relationship between information structures, decision approaches, learning patterns, and performance. Bollom [1973] used a business game to examine the effects of different interim reporting methods under conditions of rising and falling historical and current economic trends. Dermer and Siegel [1974] used a business game to assess the effects of behavioral measures in accounting for human resources. Vasarhelyi [1977] used an interactive planning simulator to assess the influence of managers' decision style on performance. Gordon, Rhode, and Merchant [1978] used a computer simulation of a market structure to focus on the effects of salary and human resource accounting disclosures on small group relations and performance. Benbasat and Dexter [1979] used an interactive management game to compare “value” and “events” approaches to providing accounting information. Brownell [1981] used a business game to focus on the effects of locus of control as a moderator of the relationship between budget participation and managerial performance.

**TABLE 1**  
*Articles by Years and Journal*

	AOS	AR	JAR(S)*	Total
1970-73 .....		10	16(8)	26
1974-77 .....	6	15	18(4)	39
1978-81 .....	<u>12</u>	<u>6</u>	<u>30(2)</u>	<u>48</u>
Totals .....	18	31	64	113

\* Number in parentheses reflects articles in annual supplement.

**TABLE 2**  
*Articles by Years and Subject Type*

	Student	Nonstudent	Both	Total
1970-73 .....	11	7	8	26
1974-77 .....	21	16	2	39
1978-81 .....	<u>16</u>	<u>26</u>	<u>6</u>	<u>48</u>
Totals .....	48	49	16	113

**TABLE 3**  
*Articles by Years and Type of Experiment*

	Simulations	Generic Tasks	Judgment Tasks	Other	Total
1970-73 .....	8	6	9	3	26
1974-77 .....	3	10	26		39
1978-81 .....	<u>3</u>	<u>14</u>	<u>31</u>	—	<u>48</u>
Totals .....	14	30	66	3	113

investors. Sets of simulated financial statements were formulated under conditions of rising and falling historical and current economic trends for various interim reporting methods. Subjects were given a hypothetical \$25,000 at the start of the experiment which could be used to buy stock, sell short, or hold; a decision had to be made each quarter. Subjects also were given a set of three past annual and nine past interim reports using a specific reporting method along with the stock price per share range for each year. Subjects then were given the first quarterly report and current market price and were asked to estimate the direction of the change in market price per share for the next quarter, how the annual results would differ from the preceding year's annual results, and to make an investment decision. Subjects were given subsequent quarterly reports and market prices and were asked to make additional, sequential investment decisions. Each set of statements had a built-in seasonal dimension as well as random fluctuations around the seasonal dimension. Rewards were offered to subjects who made the largest return on their investment. Bollom [1973] concluded that observed differences in average subject returns for different interim reporting methods were not significant, but observed differences in average subject returns for the economic conditions treatments were significant.

The Bollom [1973] study is typical of most simulation experiments in accounting. In general, these experiments have not tended to mirror any



particular naturally-occurring system. No attempt has been made to describe the simulated setting as an approximation of a specific natural setting in which accounting information is used for decision making and performance evaluation. Moreover, these experiments have tended to use student subjects, which also limits the extent to which the simulated settings mirror particular naturally-occurring systems. The construction of a laboratory "stock market in miniature" by Pankoff and Virgil [1970] to investigate the demand for accounting items by financial analysts and the effects of these items on their expectations, represents the most ambitious attempt to simulate a natural setting.

The results of these studies generally have been mixed, which is not too surprising given the dynamic nature of simulated settings. Subjects' behaviors are reactions to events which themselves are triggered by subjects' behaviors. In several studies, the dynamics of the game or simulator have produced effects which have dominated the effects of the accounting treatments. For example, in the Bollom [1973] study the effects of the economic conditions treatments were significant, but the effects of the accounting treatments were not. In addition, measures obtained tend to be incomplete and confounded and many properties of the events and of the subjects' behaviors are not measured. Since simulated features often are not varied independently of one another, a single run provides a single, complex observation.

Originally, simulations were developed to assist in theory building. This role is implicit in Guetzkow's [1967, p. 203] informal advice to "think of a simulation as a theoretical construction, consisting not only of words, not only of words and mathematical symbols, but of words, mathematical symbols, *and* surrogate or replicate components, all set in operation over time to represent the phenomena being studied. . . . When the constructions are realized in action, they are an operating vehicle through which many implicit consequences of theory may be exposed, although not tested, in the sense of providing a situation which permits verification of facets of the theory they constitute."

Simulations also can be used to replicate natural settings. They can be used to capture some of the reality of these settings, obtain high subject involvement, and preserve some confounding elements which are part of any natural setting. For example, "Looking Glass, Inc." is a simulation of a glass manufacturing corporation which was designed primarily as a research vehicle to generate hypotheses about managerial and organizational effectiveness (McCall and Lombardo [1978] and Lombardo and McCall [1982]). There are 20 positions ranging across three divisions and four levels. The divisions face different environments, ranging from volatile to stable. All problems contained in the simulation are based on actual events. The non-computer-based simulation is relatively brief (six hours), portable, and person-centered. A telephone system and a written communication system are used to aid subjects. Status differences are emphasized through accoutrements and office sizes and locations. The

simulation includes a simplified financial management system, the positions of corporate and plant controllers, and several reports include financial data and analyses. Even a glossy annual report is included. Looking Glass, Inc. is faced with 13 major capital problems involving investment divestiture or resource allocation. Information is provided about the urgency of each problem, the acquisition costs of alternative solutions, the impact of each alternative on operating costs, sales, net profit, etc., and the opportunity costs associated with each alternative.

The Looking Glass, Inc. simulation can be used to focus on how decisions are made in the chaotic, often confusing, and complex world of the manager in an organization. On an individual and group level, it is possible to observe (1) how problems are recognized or ignored, (2) the process of problem definition, (3) the search for and integration of information, (4) how solutions are generated and examined, (5) what actions are taken or not taken, (6) the impact of some decisions, and most important, (7) how good the actual decisions were given the information that was available somewhere in the organization.

Simulation experiments have essentially disappeared in accounting. Does their disappearance reflect technical problems in undertaking these simulations? Does it reflect dissatisfaction with mixed and insignificant results because of their dynamic nature and potential for confounding effects? Does it reflect the general disappearance of business games and simulators from business school curricula? Or, does their disappearance reflect judgments by accounting researchers about the relevance and role of settings versus stimuli for studying behavior in response to accounting information?

Building simulations can be difficult, time-consuming, and risky. Tight control over variables is impossible and perhaps undesirable, and keeping track of subjects or figuring out what they are doing can become an important problem. In short, simulations can become as unwieldy, unpredictable, chaotic, and hard to measure as natural environments.

But, some simulations of organizations (e.g., Looking Glass, Inc.) already exist which potentially could be used to focus on behavior in response to accounting information. These simulations may reduce the distance between observers and actors because actors may do more of what they do in everyday life and may do so more openly and with more involvement. The behaviors evoked in such simulations can be studied qualitatively, which suggests that this methodology may be responsive to a growing insistence that organizational studies represent more accurately the member's point of view (Schwartz and Jacobs [1979] and Van Maanen [1979]). These simulations can be used to generate and build theory (Fromkin and Streufert [1976]). They also can be tuned to make them more like the field ("free simulations" in which events that occur are shaped in part by the actors themselves) or more like the laboratory ("experimental simulations" where between-group variables are manipulated in the standard manner but over longer periods of elapsed time



and in such a way that actors believe that they are determining their own fate).

#### GENERIC TASKS

Several studies have used generic tasks to focus on behavior in response to accounting information. An attempt is made to highlight selected behavioral processes and certain conditions related to these processes. Variables and tasks are abstracted from natural settings and are represented in more fundamental form. The attempt is not to create tasks and settings which look like natural tasks and settings, but to create tasks and settings which evoke behaviors that unfold in natural tasks and settings. The hallmark of laboratory experimentation is abstraction—that is, the deliberate manipulation of one or more crucial variables, the deliberate control of many others, and the precise measurement of one or more variables which characterize the behavioral processes of interest.

For example, consider the studies by Cherrington and Cherrington [1973], Ansari [1976], and Rockness [1977] about the effects of performance reports. These studies focused on the use of performance measures to reward performance and examined the effects of variations in these measures under laboratory conditions. Neither the specific measures studied nor the tasks performed were intended to simulate realistic settings. The tasks included constructing “moon tents” and “shallow water cargo carriers,” assembling cardboard alphabets for preschool children, and verifying the location of integrated circuits on circuit boards. Their purpose was to show how variations in the performance measures for highly abstract generic tasks influenced basic activities which presumably operate in all human organizations.

A wide variety of tasks has been used in addition to those identified above. Some tasks have reflected simplified accounting situations. For example, some tasks have required the use of abbreviated financial statements<sup>7</sup> and other tasks have been related to simplified cost investigation settings.<sup>8</sup> Other abstract tasks<sup>9</sup> and probabilistic judgment tasks<sup>10</sup>

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<sup>7</sup> Tasks requiring the use of abbreviated financial statements have been used by Rose et al. [1970] to examine comparative judgments of numerical information, by Dopuch and Ronen [1973] to examine the effects of alternative inventory methods, by Ronen and Falk [1973] to focus on the relationship between the information measure of entropy and the perceived value of information, by Becker, Ronen, and Sorter [1974], Neumann and Friedman [1978], and Friedman and Neumann [1980] to determine whether subjects will rely on opportunity cost information if it is provided, and by Chen and Summers [1981] to examine the effects of accounting data prepared in probabilistic forms instead of the conventional deterministic form.

<sup>8</sup> Tasks related to simplified cost investigation settings have been used by Eggleton [1976] to determine whether subjects could distinguish between selected time series, by Chang and Birnberg [1977] to focus on the ability of subjects to respond to a change in the methods used to calculate data presented, by Magee and Dickhaut [1978] to examine the heuristics used by individuals in choosing whether to investigate cost variances, and by Brown [1981] to focus on the effects of situational variables and payoff structures on the use and effects of anchor and adjustment heuristics.

also have been used to focus on the use of accounting information.

The studies which employ generic tasks differ from simulation studies in several respects. First, experimenters who use generic tasks tend to exert more control over antecedent conditions by decoupling stimuli, events, and behavior across trials. In a simulation study, the temporal flow of stimuli, events, and behavior is regulated to some extent by the dynamics built into the simulation and by the behavior of the subjects. Because stimuli and events are themselves triggered by the prior behavior of subjects, their behaviors cannot be attributed to a predetermined, fixed sequence of stimuli and events. Many of the studies which have used generic tasks reflect a stimulus-response pattern in which trials have been used to exert control over the content and timing of stimuli and events to which observed behavior can then be attributed. In other words, experimenters using generic tasks have increased their influence over or manipulation of the antecedent conditions of the behaviors studied.

Second, studies which include generic tasks tend to focus more on the effects of stimuli and less on the effects of the setting in which such stimuli are or might be used than the simulation studies described above. In most simulation studies, investigators attempt to create a setting in which stimuli, events, and behavior interact. Subjects receive (or purchase) information, make predictions and decisions, learn the results of their activity, and then make another round of predictions and decisions. The results often are used as the basis for performance measures and rewards.<sup>11</sup>

However, investigators who use generic tasks tend to present subjects

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<sup>9</sup> Abstract tasks have been used by Dickhaut and Eggleton [1975] to examine comparative judgments of numerical information in an accounting setting where a budgeted amount was compared with actual accounting numbers in a nonconnotative setting in which an expectation was compared with detailed observations of the expectations; by Birnberg and Slevin [1976] to manipulate statements about the mean and dispersion of data about past activity levels to examine the effects of confidence interval statements in place of or in addition to point estimates in accounting reports; and by Ashton [1976], who used a product-pricing task to focus on the effects of an accounting change in one setting and then altered the setting to focus on subjects' ability to obtain information about the decision maker's model and to apply this information to their own decision making (Ashton [1981]).

<sup>10</sup> Ronen [1971] focused on a sequence effect in problems involving disaggregated probabilistic information; Hirsch [1978] also focused on this sequence effect by using both probabilistic and business tasks; and Dickhaut [1973] used Bayes' Theorem to examine probabilistic estimates of subjects based on messages they received from alternative information structures. A task setting in which subjects make binary information purchase decisions for a simulated decision maker has been used by Uecker [1978] to test whether subjects learn the optimal information system for decision makers, by Uecker [1980] to test whether knowledge of the decision rule would improve subjects' ability to choose an optimal information system, and by Hilton, Swieringa, and Hoskin [1981] and Hilton and Swieringa [1981; 1982] to examine the extent to which subjects correctly perceive the effects of accuracy, initial uncertainty, and flexibility on information value.

<sup>11</sup> The studies by Cherrington and Cherrington [1973], Ansari [1976], and Rockness [1977] are similar to several simulation studies in that they focus on the effects of performance reports in a setting where a limited amount of interaction occurs among stimuli, events, and behavior.

with information or displays only and to focus on relatively limited responses to these presentations. In most studies, stimuli (presentations or displays) do not interact with other stimuli, events, or previous responses. The stimuli have been generic in the sense that they tend to reflect characteristics that may be common to general settings rather than to any particular setting. Moreover, the subjects in most of these studies have been students, which is consistent with the generic nature of the stimuli.

Third, where simulation studies tend to use features of the simulated setting as the basis for the hypotheses to be tested, many studies using generic tasks rely on formal laws, models, or theories as the basis for the hypotheses to be tested, as a criterion against which to compare subject performance, or as a framework for examining elements of decision processes.<sup>12</sup> This reliance on formal laws, models, or theories is an important development which has contributed to better designed experiments and to the integration of analytical and experimental research in accounting.

In summary, a diverse set of experiments has used generic tasks to focus on the effects of accounting information. These experiments involve considerable control over antecedent conditions and examination of a restricted range of behavior. Both features tend to increase the distance between observers and actors because actors do less of what they do in everyday life and do so less openly and with less involvement. These features also may combine to create reactive situations in which slight changes in manipulations or measures may produce different results. Many of these experiments have been conducted within the context of formal hypotheses, models, or theories. We believe that additional opportunities exist for using such experiments not only for purposes of verification or hypothesis testing, but also for purposes of discovery and theory generation.

#### JUDGMENT TASKS

Many of the more recent experiments in accounting reflect studies of judgment which focus on the behavior of subjects in response to accounting displays, presentations, etc. These experiments tend to focus on

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<sup>12</sup> Where Cherrington and Cherrington [1973] used basic principles of operant conditioning to provide a framework for their study, Foran and DeCoster [1974] used cognitive dissonance theory, Ansari [1976] used cognitive consistency models, Rockness [1977] used expectancy theory, and Shields, Birnberg, and Frieze [1981] used attribution theory to provide frameworks for their studies. Studies by Rose et al. [1970] and Dickhaut and Eggleton [1975] used Weber-Fechner's Law for physical stimuli to assess comparative judgments of numerical information. Ronen and Falk [1973] compared the entropy measure suggested by Lev [1968] with subjects' perceived value of information. Ashton [1976] and Chang and Birnberg [1977] used the "functional fixation hypothesis" as a framework for their studies. And Bayesian probability revision and expected value maximization have provided the framework for studies by Ronen [1971], Dickhaut [1973], Hirsch [1978], Uecker [1978; 1980], Brown [1981], Hilton, Swieringa, and Hoskin [1981], and Hilton and Swieringa [1981; 1982].

certain actors who presumably use accounting information in making various decisions or judgments. The actors are asked to make these judgments under conditions which are experimentally created to focus on the actors' use of accounting and other information.

The research design that was prominent through the early 1970s to focus on judgments was a variation of the "static group comparison" design (Campbell and Stanley [1963]). In pure form, this design includes one group which is measured after exposure to an experimental treatment and a different group which is measured without exposure to the experimental treatment. Rather than use a control group that has received no experimental manipulation, many judgment studies in accounting have used several experimental groups and have made group comparisons based on postexposure tests only.

For example, consider a study by Barrett [1971] about the effects of accounting for intercorporate investments on the judgments of financial analysts. Each participating financial analyst received a prospectus for two hypothetical companies. Both prospectuses were from one of three experimental treatments: (1) both companies used the cost method to account for intercorporate investments and the quoted market value for these investments was disclosed in the footnotes; (2) both companies used the cost method to account for intercorporate investments and the quoted market value, annual earnings under the equity method, and cumulative investment under the equity method were disclosed in the footnotes; and (3) both companies used the equity method to account for intercorporate investments and the quoted market value, annual earnings under the cost method, and the original cost of the investment were disclosed in the footnotes. Barrett focused on the effects of the experimental treatments on the average value of the analysts' estimates of dollar price per share and the amount of uncertainty associated with these estimates. Barrett concluded that the accounting methods used for intercorporate investments did not influence the judgments of financial analysts as long as there was enough information in the footnotes to the financial statements to allow analysts to recast the data in terms of the alternative accounting method.<sup>13</sup>

<sup>13</sup> Revsine [1971] discusses the design and findings of this study. Similar static group comparison designs have been used by Elias [1972], Hendricks [1976], Schwan [1976], and Tomassini [1977] to focus on the effects of human resources information; by Hofstede [1972] to focus on the effects of subject type, earnings per share trend, and consistency of trend with the president's letter on subject perceptions and information processing; by Oliver [1972] and Keys [1978] to test the effects of confidence interval statements on loan decisions of bankers; by Abdel-khalik [1973; 1974b] to examine the effects of aggregating accounting reports on the quality of the lending decision of loan officers and students; by McIntyre [1973] to investigate the effects of current-cost financial statements on common stock investment decisions; by Acland [1976] to focus on the effects of behavioral indicators on investor decisions; by Patton [1978] to focus on the effects of consolidating municipal financial reports on municipal bond evaluation; by Belkaoui [1980b] to investigate the impact of socioeconomic accounting statements on investment decisions; and by Ramanaathan and Weis [1981] to focus on the effects of supplementing conventional fund-by-fund colleague financial statements with supplemental consolidated data.

Since the mid-1970s, another research design has been used as the basis for most judgment studies in accounting. Using the Brunswik [1952] lens model as an analytical framework, human judges have been viewed as making decisions, judgments, or predictions based on a set of explicit cues or pieces of information from the environment which are probabilistically related to a relevant environmental event or criterion (Libby and Lewis [1977, p. 248]). In the typical study, subjects make judgments for a large number of cases which are based on the same set of cues or stimuli. A model is then used to represent the functional relationship between the cues and responses.

For example, Ashton [1974] asked 63 practicing auditors to judge the strength of the internal control in a payroll system using six dichotomous internal control factors. Thirty-two cases were developed using a  $\frac{1}{2}$  fractional replication of a  $2^6$  factorial design. Each auditor judged the strength of the system profiled in each case on a six-point scale (where 1 was extremely weak and 6 was adequate to strong). The cases were administered a second time six to thirteen weeks later. An analysis of variance model was used to represent the relationships between the six cues and responses. Ashton concluded that the auditors' judgments were highly consistent over time and that there was considerable agreement or consensus among the auditors. The main effects of the analysis of variance model accounted for over 80 percent of the variance on average. Two-thirds of the auditors made significant use of at least five of the six factors.<sup>14</sup>

Judgment studies have the potential for reducing the distance between the observers and actors because they insert a technology between the two that may be equally understood by both. Each may understand more clearly the descriptions and actions made by the other because each has a similar referent in mind. This common understanding is less likely in simulations because there may be many realistic trappings which people can notice and invest with idiosyncratic meanings. It is also less likely in generic tasks because people may have difficulty relating the setting to anything else they do on a regular basis.

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<sup>14</sup> This study was replicated by Ashton and Brown [1980] and by Ashton and Kramer [1980]. A similar design was used by Joyce [1976] to analyze auditor's allocations of time to categories of audit work related to accounts receivable. Similar research designs have been used by Libby [1975*a*; 1975*b*], Kennedy [1975], Casey [1980], Zimmer [1980], Abdel-khalik and El-Sheshai [1980], and Holt and Carroll [1980] to focus on the judgments of commercial loan officers; by Boatsman and Robertson [1974], Moriarity and Barron [1976; 1979], Hofstedt and Hughes [1977], and Firth [1980] to focus on materiality judgments of auditors and others; by Wright [1977; 1978] to focus on predictions of stock prices using accounting and market indicators; by Harrell [1977] to focus on air force officer evaluations of training wings and the effects of feedback; and by Kessler and Ashton [1981] to analyze the effectiveness of different types of feedback. Other judgment studies in accounting have focused on different elicitation methods for obtaining subjective probability estimates (Corless [1972], Felix [1976], Chesley [1976; 1977; 1978], Crosby [1980; 1981]) and the effects of different heuristics and biases in accounting and auditing contexts (Swieringa et al. [1976], Uecker and Kinney [1977], Joyce and Biddle [1981*a*; 1981*b*]).



Figuratively, simulations and generic tasks bring subjects to the laboratory, whereas judgment tasks take the laboratory to the subjects in the sense that they continue to do what they would normally do anyway, with the exception that they do it under closer scrutiny with more structure imposed on the stimuli (e.g., standard format). Subjects remain largely on their own turf using well-rehearsed routines which they impose on materials that resemble their normal inputs. They, rather than the investigators, are the experts unless—and this is a crucial qualification—the investigators also have expertise with the same materials. When both the investigators and the subjects are experts on the same topic (e.g., both are accountants), meaningful discussion is possible and at a relatively high level of sophistication. Furthermore, it is much more difficult for either party to dissemble, since both have a profound understanding of what is going on and where fabrication is possible.

The studies which employ judgment tasks are similar to those which use generic tasks in that experimenters have tended to exert considerable control over the information and displays presented to subjects and to focus on relatively limited responses to these presentations. The judgment studies which have used the static group comparison design are similar to studies which have used generic tasks in that they have been conducted in the context of various hypotheses, models, or theories and have used a between-subject design. Judgment studies which have used the cue-utilization design have tended to be closely related to a single, well-developed framework—namely, the Brunswik lens model. This common context has contributed to efficiencies in the design of experiments and the communication of results. Moreover, judgment studies which have used the cue-utilization design have used a within-subject design which narrows the focus of an experiment to the judgments of individual subjects, but which can be extended to allow the experimenter to make comparisons between subjects as well. This design has facilitated more precision in manipulating, controlling, and measuring variables of interest.

#### AN OVERVIEW

Our brief survey of past laboratory experiments in accounting suggests increasing activity by experimentalists as well as some shifts in the types of subjects used and in the types of experiments undertaken. These experiments also provide a glimpse of how experimenters view the role and effects of accounting information.

First, accounting experimenters tend to view cognitive processes as very important in understanding the role and effects of accounting information. The behavior observed in a typical accounting experiment is in the form of a response which is generated by displays, presentations, instructions, etc. But, are the effects of accounting information limited to such displays, presentations, and instructions? Are the interesting and



important effects of accounting information limited to cognitive processes?

Experiments may be better suited for studying cognition than for studying either sentiment or action. However, sentiment and action may dominate cognition, but this may never be discovered in conventional laboratory studies. If sentiment is not allowed to affect cognition, researchers may conclude that it will not affect cognition, in which case they will focus on an increasingly closed world in which cognition affects cognition. Retreat from everyday life to a wholly cognitive version of what is happening virtually guarantees irrelevance.

Second, accounting experimentalists have used a surprisingly limited variety of tasks in settings which can best be described as reactive. The nature of these tasks and settings raises the prospect of many conclusions evaporating when different technologies are used. The tasks in many accounting experiments tend to be well defined, the information given is perfectly reliable, the range of hypotheses that are considered about the data is restricted by the particular dependent variable. Yet, in many everyday settings, tasks are ill defined, information must be searched for, data are rarely perfectly reliable, and hypothesis formation, confirmation, and disconfirmation occur within a broad range of possibilities. How can we reconcile these differences? Moreover, heuristics are ways of coping with complexity, ambiguity, costly processing, etc. They are likely to be learned responses which work well in some situations and poorly in others. How can complexity and ambiguity be reflected in experimental settings.<sup>15</sup>

Third, accounting experimenters have tended to focus on the role and effects of accounting information before decisions or judgments are made. This is what Demski and Feltham [1976, p. 9] refer to as the “decision-facilitating” role in which accounting information is provided to a decision maker before a decision is made to help resolve some form of uncertainty in the decision problem at hand. But, is the role of accounting information limited to this decision-facilitating role? What is the role of this information after a judgment or decision is made and implemented? Demski and Feltham [1976, p. 9] describe the “decision-influencing” role of accounting information as providing a decision maker with information after a decision has been made and implemented to evaluate the decision maker’s performance, with the purpose of motivating action selection. Is this decision-influencing role of accounting information likely to interact with its decision-facilitating role?

For example, in evaluating a loan application a lending officer will make a prediction about whether the applicant will default on future loan payments and may rely on accounting and other indicators in making this prediction. However, the loan officer or someone else may use

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<sup>15</sup> See Einhorn [1976] for a discussion of these questions.

accounting information to justify the prediction and any subsequent decision about the loan application and may use accounting information in bargaining with the applicant about the terms of the loan if it is accepted. Moreover, if the loan becomes part of a loan portfolio, someone will likely be held accountable for keeping track of the loan, reporting on its status, renegotiating or restructuring the loan, etc., and accounting information is likely to be used in these activities as well.

Fourth, accounting experimenters have focused almost exclusively on single-person settings. Yet, multiperson settings, as envisioned by the principal-agent relationship, tend to reflect continuing relationships, which means that issues of trust, disclosure, assumed similarity, social comparison, reference groups, etc., determine judgments, inferences, and outcomes. Dyadic concepts are not just properties that enrich context, they are basic to the phenomenon itself. The appropriate model is not so much “information transmission” as it is the “communication game” in which purposeful social interaction occurs “within a socially defined context, involving interdependent social roles and conventional rules, stratagems, and tactics for making decisions and obtaining various goals” (Higgins [1981, p. 346]). It is important to realize that when someone says we should pay more attention to the situation, much of what we call the situation is another person.<sup>16</sup>

Finally, accounting experimenters have increasingly narrowed the focus of their experiments. They have done this by relying more heavily on formal models and theories in providing a context for their studies. They also have done this by increasing their control over stimuli (information and displays) presented to subjects and by structuring the responses of subjects to stimuli. These developments have increased the sophistication and quality of experiments in accounting, but they also have limited the scope of these experiments.

One could conclude from a reading of the experiments surveyed that laboratory experiments are limited vehicles for observation of behavior. These experiments have been used to address limited questions and have obtained limited answers to these questions. We do not view laboratory experiments themselves as being inherently limited or limiting. Rather, we believe that many of the limitations associated with these experiments tend to reflect the limitations of experimenters, their decisions, and their concerns. In the next section we discuss several trade-offs and nuances of experimentation to encourage experimenters to consider a more complicated view of experiments and how to learn from them.

### *Experimentation Reconsidered*

“Is there any other point to which you wish to draw my attention?”

<sup>16</sup> Much of what we call the situation also includes markets. Many decisions in everyday life are made in the context of markets. Can experiments be extended to include markets for information, decision makers, etc.? Some experimental economics studies which have created laboratory markets are discussed later in this paper.

“To the curious incident of the dog  
in the night-time.”  
“The dog did nothing in the night-time.”  
“That was the curious incident,”  
remarked Sherlock Holmes.

—Sir Arthur Conan Doyle, *Silver Blaze*

In the above quotation, Sherlock Holmes was trying to demonstrate both that observation of a nonevent can instruct and that observational skills are necessary for effective learning. The fact that the dog did not bark was extremely important, as it suggested the fact that the intruder being sought had to have been familiar to the dog. This narrowed the field of suspects considerably. In this section, we examine several subtleties of experiments to demonstrate that experimenters should adopt a more differentiated view of what can be learned from them.

We can illustrate the basic argument by an incident which happened during military maneuvers in Switzerland. The young lieutenant of a small Hungarian detachment in the Alps sent a reconnaissance unit out into the icy wilderness. It began to snow immediately, snowed for two days, and the unit did not return. The lieutenant suffered, fearing that he had dispatched his own people to death. But the third day the unit came back. Where had they been? How had they made their way? Yes, they said, we considered ourselves lost and waited for the end. And then one of us found a map in his pocket. That calmed us down. We pitched camp, lasted out the snowstorm, and then with the map we discovered our bearings. And here we are. The lieutenant borrowed this remarkable map and had a good look at it. He discovered to his astonishment that it was *not* a map of the Alps, but a map of the Pyrenees.

Experiments are like maps. They help observers get their bearings and they animate them. Experiments generate outcomes that help people discover what is happening and what needs to be explained.

#### STRUCTURE AND FREEDOM

The laboratory experiment is often viewed as a research strategy for producing phenomena which is in contrast to naturalistic studies which preserve and record existing phenomena. To produce phenomena, the laboratory experimenter adopts a variety of procedures to manipulate, control, and measure variables.<sup>17</sup> Typically, these choices involve trade-offs between structure and freedom.

Willems [1969] has suggested that observers trade structure for freedom along two dimensions (see fig. 1). The first dimension is the extent to which the antecedent conditions of the observed behavior are influenced by the experimenter. The second dimension is the extent to which restrictions are imposed on the form of the observed behavior.

Observation under conditions of high manipulation of antecedent conditions and high restriction of response measures occurs when independ-

<sup>17</sup> See Runkel and McGrath [1972, pp. 59–80] for a discussion of how variables can be treated in research designs.

		Degree to which Antecedent Conditions are Manipulated	
		High	Low
Degree to Which Restrictions are Imposed on Response Measure	High	(1)	(2)
	Low	(3)	(4)

FIG. 1.—Trade-offs between structure and freedom in inquiry (from Willems [1969]).

ent variables are induced by sequences of prearranged stimuli and responses are limited to specific forms (e.g., the subject makes a single rating). Such an experiment involves a highly structured situation that might be relatively uninformative since so little meaningful variation can occur. To introduce more variation the experimenter could manipulate fewer antecedent conditions so that behavior is instigated more spontaneously (cell 2), or measure a greater variety of outcomes (cell 3), or both (cell 4). Cell 4 may be no more instructive than cell 1 because there is nothing but variation and no order is imposed on the rapidly accumulating observations. To impose some order on this completely unstructured situation, an experimenter can either focus attention on fewer behaviors (cell 2), or intervene more forcefully to evoke a more restricted range of variations (cell 3).<sup>18</sup>

#### INSTRUCTION AND CONTROL

The dimensions in figure 1 suggest that a more structured inquiry involves more control, but more control may result in less instructiveness. Experiments are vehicles to systematize the observation of behavior. Their purpose is to be informative, enlightening, illuminating, instructive. Yet, procedures that manipulate, control, and measure variables make observing more systematic, but they may also reduce what is observed and learned. Extensive controls put distance, trappings, and routines between the observer (experimenter) and the observed (subjects).

Warner [1981, p. 178] has urged that observers “seek instruction rather than control”—that they “not give up theoretical and pragmatic instructiveness in order to attain experimental control.” If we view experiments as ways to systematize observing in the interest of instruction, then a normal hallmark of experiments—experimenter control over conditions—recedes in importance. Control now becomes salient as a potential threat to instruction. To learn more is to control less, up to a point. Viewed from the standpoint of instruction, control is a mixed blessing as are

<sup>18</sup> See Willems [1969] for a discussion of studies which can be included in each cell.

several additional accompaniments of experimentation. We reexamine six of these accompaniments to develop a more differentiated view of the ways in which experiments facilitate observing.

#### ILLUSION OF CONTROL

One subtlety of experimentation viewed as systematized observing is the illusion of control. One reason that Warner made his recommendation to seek instruction rather than control was that he viewed experimental control as an illusion: “experimenters focus upon controlling some parameters of experimental situations, and they fail to notice that many other parameters are shifting freely, in ways that confound interpretations” (Warner [1981, p. 179]). Even though experimenters routinely talk about holding variables constant, they usually fail at this. It may be more accurate to describe experimentation as an effort to ensure that the effects of uncontrolled variables influence behavior equally under the different levels of the independent variable. The crucial concern is that potential independent variables not have a differential impact on the variables of interest.

When laboratory findings fail to replicate in nonlaboratory settings, this usually occurs because some new variables interact with the variable studied and alter the effect. Of all the possible variables that influence behavior in any real situation, a laboratory experiment selects only a few for testing. As a result, hidden or unsuspected interactions in nonlaboratory settings may easily nullify, or even reverse, conclusions arrived at in the laboratory. This does not mean that the laboratory findings are not applicable. It does mean that they are applicable only under certain conditions that must be specified.

Experimental effects often are small and may be masked by other variables when they unfold outside the laboratory. Experimenters are not any more interested in trivial events per se than are other people. But, the effect of controlling extraneous or irrelevant variables in the laboratory is to increase the precision of an experiment and to increase the risk of discovering effects which are small. In addition, experimenters often study small events because they are interested in precise laws, variables that others neglect, and ways in which small events amplify: “The laboratory is an adjunct and corrective device for uncontrolled observations, not a substitute . . . If under a wide variety of conditions the effect is consistently small, however, it may remain as an interesting laboratory phenomenon judged to be of small consequence in the overall behavior. This conclusion is possible only *after* the phenomenon of interest has been studied carefully” (emphasis added; Underwood and Shaughnessy [1975, p. 18]).

It is also important to remember that variables always change when they are brought into the laboratory. Because of this, the question of generality is always uppermost for experimenters, though it often takes

the form of concern about *generality* within the laboratory. Typical questions include: will the relationship hold with other subject populations, with other levels of the independent variable, across tasks, with other measures, etc.? As experimenters answer questions such as these within the laboratory they gradually incorporate more of the variables that potentially interact with phenomena outside the laboratory.

#### THE ECOLOGY OF WORK

A second subtlety of experimentation as systematized observing is that some presumed liabilities of experimental settings may be assets for observers who focus on questions related to organizations. In particular, we are intrigued by the close parallels between standard experimental settings and superior-subordinate relationships in ongoing organizations.

To illustrate, participants in experiments are apprehensive about being evaluated, but so are ambitious employees. Laboratory tasks require limited skills, ignoring the "rest" of what the person brings to the laboratory, but the same holds true with a division of labor and partial inclusion. Relationships between experimenter and subject involve asymmetrical power, but the same holds true for superiors and subordinates. Subjects seldom know why they are doing the things they do in laboratories, but employees often operate under similar conditions of ignorance and faith. Participants in laboratory groups seldom know one another intimately, but the same is true in organizations where personnel transfers are common, where temporary problem-solving units are the rule, and where impression management is abundant. People participate in experiments for a variety of reasons, but the decision to participate in an organization is similarly overdetermined. Finally, people are suspicious of what happens to them in laboratories, but so are employees suspicious as they become alerted to the reality of hidden agendas and internal politics.

These parallels between experimental and organizational settings suggest that the former may capture more features of the latter than is generally recognized. While we view these parallels as potential assets, Argyris [1975] views them with alarm. He notes that people who participate in laboratory research are acting in situations similar to those which organizations create for their lower employees (e.g., treat employees by Theory X philosophy). Thus, laboratory studies can be generalized only to authoritarian and socially isolated settings. Argyris [1975] also notes that research participants and lower-level employees respond to Theory X treatment by subversion, withdrawal, disingenuous responding, dependency, and short time perspectives.

It is possible to examine Argyris' concerns from a different perspective, that of instruction: (1) Since lower-level employees outnumber higher level employees and since subordinates ultimately determine the amount of influence they will accept from those who lead, the capability to generalize from laboratories to constrained populations in organizations



is not trivial. (2) Subversion can be treated as trouble or as datum to be explained. (Rosenthal transformed the "trouble" of experimenter bias into the datum of teacher expectancies that affect pupils.) (3) All laboratory empirical work is a unilateral influence attempt, in the sense that the experimenter strives to have the subject accept his or her single definition of the situation. (4) Definitions of the situation in rigorous research are *not* negotiable; if they become so, the resulting study is unethical (Rutstein [1969] and Weick [1977]).

#### THE PHENOMENOLOGY OF WORK

A third subtlety of experimentation which may explain why laboratory experiments often produce less instruction than they could is that experiments are often designed with this recipe in mind: "if persons are confronted with stimulus *X*, they will do *Y*." That straightforward, input-output statement is misleading because it excludes context and the perspective of the subject, thereby limiting its generality. A more appropriate recipe for designing laboratory experiments may be the following: "if persons are in situation *X*, performance *Y* will be judged appropriate by native actors."

In this revision, stimuli have been stretched into situations, responses have been stretched into performances, context has been elaborated by inclusion of other actors observing and judging the appropriateness of what the target person does, and goodness-of-fit (appropriateness) has been added as an additional output measure. If a stranger wanders into the middle of an audit and wants to pass unnoticed as one of the auditors, that person has to understand what auditors do, when they do it, and how they talk about it. That person also has to see what auditors ignore, overlook, and take for granted. If, having observed auditors, the stranger can sit alongside them and act without appearing odd, then the chances are good that the auditors' work has been understood.

Designing a study which is consistent with the second recipe is difficult. The experimenter is forced to consider the ecological validity of both the setting and the task: (1) in what situation should the task be embedded; (2) to whom is the subject accountable for performance; (3) what constitutes more and less appropriate performance, in whose view, and how is this conveyed to the subject; (4) who outside the laboratory might judge performance of people on tasks resembling this one; (5) what are the consequences of inappropriate performance, how visible are they, how severe are they?

To summarize the point, the second recipe is the criterion naturalistic observers impose on themselves to see if they understand events in the field. If they can pass as a native in natural settings, then they understand some of what is occurring. Thus, they use "success in passing," not "truth," as their criterion of understanding. Assuming that experimenters also want to say something relevant about naturally occurring settings, we suggest that experimenters should act like naturalists *earlier* in their

observing. They should act like naturalists when they create an experiment, not later when they have results and try to find some naturally occurring niches where these results fit. Even if experimenters do not change anything when they design studies using the second recipe, they will be in a stronger position to address the issue of relevance because their departures from realism are informed rather than inadvertent. Furthermore, use of the second recipe alerts experimenters to properties of the experimental setting that may interact with manipulated variables. Depending on the experimental outcomes, these potential interacting variables may be the focus of the next study. Experimenters can adopt the mind-set of naturalistic observers and, in doing so, can avoid the harsh discontinuities between the laboratory and field that often plague them.

#### ALTERNATIVE INTERPRETATIONS

A fourth subtlety of experimentation is that alternative explanations take on a more benign character. While they still signify possible flaws in foresight and control, they also signify a more restricted version of what might have occurred to produce outcomes. Alternative explanations can be instructive in the sense that they focus attention on a limited set of possibilities.

Descriptions of alternatives routinely exclude a great deal. People preoccupied with control often overlook this. Instead, they lament the absence of one best explanation. Alternative explanations are not mere noise. They are attempts to learn and should be treated as such.

The increasing use of increasingly informal alternative explanations to dismiss multiple confirmed findings has triggered statements of dismay by experimenters (e.g., Mills [1977] and Nisbett [1978]). Careful analysis of these arguments suggests that all of them involve issues of instruction and control. Freese and Rokeach [1979, p. 200] suggest that there are five conditions in contemporary research that explain an upsurge in alternative explanations: (1) There is an emphasis in the research literature on statistically significant short-term effects rather than long-term effects. Short-term effects following experimental treatments may be more vulnerable to alternative interpretations because such factors as demand characteristics and experimental bias should dissipate the longer the time from the ad hoc experimental circumstances that produce them. (2) Dependent measures are typically obtrusive rather than unobtrusive, and the former may be more vulnerable to alternative interpretations. (3) Dependent measures are frequently "paper-and-pencil" rather than behavior, and paper-and-pencil measures may be more vulnerable to certain kinds of alternative interpretations. (4) Most research relies primarily on single rather than multiple dependent measures; a single measure may be more vulnerable than multiple convergent and divergent measures. (5) Because it is easier to demonstrate statistically significant effects with socially trivial attitudes and behaviors, we typically focus upon trivial effects in experimental research rather than changes in central attitudes,

values, and behaviors. Trivial effects may be more vulnerable to alternative interpretations. To be more specific, research on changes in central values and their related attitudes and behaviors suggests it is unlikely that presently known alternative interpretations can plausibly account for findings that demonstrate long-term, central changes ascertained by unobtrusive behavioral as well as obtrusive paper-and-pencil measures.

These five conditions can be remedied procedurally, can be remedied more readily in the laboratory than in the field, can be treated as testable hypotheses, can be weakened when addressed by cumulative theory and data, and can be neutralized by careful a priori attention to possible statistical artifacts, faulty research designs, or uncontrolled theoretically relevant variables. To reduce the grip of alternative interpretations, one becomes more systematic and documents more fully what actually occurs. "Experiments are often instructive, theoretically or pragmatically, in spite of loose control: Instruction derives more from effective analyses of what actually happened than from effective predictions of what will happen" (Warner [1981, p. 179]).

#### THE INDETERMINACY OF WORK

Fifth, researchers who rethink experimentation should reexamine the assertion that experiments are attractive because they allow people to observe cause-effect relationships. An inability to detect causation is often treated as evidence that an experiment is flawed or insensitive, when in fact exactly the opposite is true. If we use the experience in the physical sciences as a comparison, it is probable that as our measures become more sensitive, they will detect more indeterminacy in relations among the objects studied. This has happened in the physical sciences. There is no reason to think that social scientists will escape this outcome as their measurement improves: "The typical plaint 'If only our measures were better, we would be better able to understand the causal pattern that exists in this interaction' betrays an ignorance or a conscious rejection of the forces of uncertainty that Heisenberg demonstrated more than a half century ago. There can be no doubt that we need better measures, but with them an effect opposite that often presumed is most likely to occur; a more developed methodological armamentarium would likely have the effect of demonstrating ever more compellingly the indeterminacy of the basic stuff of our science" (Crano [1981, p. 318-19]).

Evidence that generates several alternative interpretations could be evidence that is more, not less, accurate because it reflects actual indeterminacies present in social events. Models of organizational functioning that give prominence to indeterminacy, models such as obstructed learning (Staw [1980]), loosely coupled systems (Weick [1982*a*]), organized anarchies (March and Olsen [1976]), may be more appropriate as vehicles to frame laboratory questions precisely because they avoid imputations of strict causality.

Laboratory technology need not be a static technology. Its conventions

and interpretations can be accommodated to shifts in theory and epistemology just as easily as other modes of inquiry can and with clearer evidence of the effects of those shifts.

#### OBSERVING AND THINKING

Finally, viewing experimentation as systematized observing in the interest of instruction suggests a subtle tension between observing and thinking as sources of understanding. Methodology books tend to emphasize abstractions through proper thinking by giving advice on how to tell if, when, where, and why something is happening rather than what is happening. Schwartz and Jacobs [1979, p. 307] pose the issue this way:

It is not possible to be a sociologist without knowing how to construct verbal representations of society and its parts, quite independent of how good or bad they are. This is also why observational skills, in and of themselves are not central to the discipline. Although it is necessary that somebody have these skills and use them, it is not necessary that everybody have them and use them. The observing can be done by the census, graduate students, or other sociologists while you do the analyzing. In contrast, other disciplines such as microbiology, radiology, archeology, and psychotherapy, require almost every practitioner to learn how to observe and recognize things in detail.

Research that is grounded in thought downplays replication (do it once and do it right), presumes that one direct observation is enough (indirect information through data sets is sufficient for follow-up), and encourages a division of labor (the analyst need not be the observer). A high premium is placed on avoiding error within the confines of a single study.

Research that is grounded in observation translates into a different set of practices. Schwartz and Jacobs [1979] suggest that researchers should know what they are talking about by seeking firsthand observation and avoiding secondary sources of data, by combining analysis and observation into the same person, and by interpreting only what they have observed firsthand. They also suggest that researchers should detect errors by replication across studies (Epstein [1980]), not by precision within a single study, and should organize inquiry around themes not studies. They suggest that single, self-contained studies are likely to be less useful than multiple, diverse studies which focus on a common theme.

Neither observation nor thought suffices by itself as a mode of knowing. Ever since social psychology became separated from abnormal psychology, which has its roots in psychotherapy and pragmatic, direct observation, it has focused on phenomena that can be grasped largely through abstract thinking (e.g., prisoner's dilemma "is" competition and cooperation). Social psychological knowledge is a way of talking and writing about social situations, not a way of observing them. To give more primacy to observing in the interest of instruction may be to unlearn more social science than people realize.

#### CONCLUSIONS

If we view experiments as ways to systematize observing only in the interest of control, we may place too much of a premium on explicit

design in experimentation. Effective experimenters are those that efficiently design their experiments to pursue specific goals. But, if we view experiments as ways to systematize observing in the interest of instruction as well, we may be encouraged to keep an open mind—to “exhibit a healthy regard for serendipity, the accidental, the unplanned” (Warner [1981, p. 179]). Experiments always yield unexpected results. The issue is not whether surprises occur, but whether they are perceived. That is, the issue is whether we will perceive the fact that the dog did nothing in the nighttime, and its implications for instruction.

### *Realism Reconsidered*

Laboratory experiments are often faulted for their “artificiality” or lack of “external validity.” The typical argument is that these experiments tend to use unrepresentative subjects (often students) in settings which may bear little resemblance to everyday activities.<sup>19</sup> How can the findings of these experiments be generalized to the “real” world of ordinary people engaged in everyday activities?

In this section we reconsider the issue of realism in experiments. We do this because accounting experimenters worry about realism.<sup>20</sup> We believe that some of their concern is misplaced. We also believe that some of their concern is mistaken. We shall argue that realism may conceal information that artificiality may disclose—that artificiality may make it easier to observe clear disconfirmations of theory and clear examples of novel relationships that are normally concealed by the sheer mass of covarying variables in realistic settings.

#### EXPERIMENTAL AND MUNDANE REALISM

There are two senses in which an experiment can be said to be realistic (Aronson and Carlsmith [1968] and Carlsmith, Ellsworth, and Aronson [1976]). In one sense an experiment can be said to be realistic if the

<sup>19</sup>See Chapanis [1967] for a discussion of this argument. Dipboye and Flanagan [1979] present the results of a content analysis of three psychology journals for 1966, 1970, and 1974 to determine the types of organizations, subjects, and dependent measures studied. They conclude that field research appears to be as narrow as laboratory research in the actors, settings, and behaviors sampled. “Indeed, if laboratory research can be described as having developed a psychology of the college sophomore, then field research can be described as having produced a psychology of the self-report by male, professional, technical, and managerial personnel in productive-economic organizations” (Dipboye and Flanagan [1979, p. 146]).

<sup>20</sup>The following statements are from Libby and Lewis [1977]: “As an additional caution, it should be noted that generalizations from experimental work to the real world are large steps” [1977, p. 254]. “A related suggestion (with the lens model) is the need for research in more realistic settings. Findings of significant contextual effects require that great care be taken in maintaining the representativeness of chosen research designs. . . . Context specific findings make generalization difficult. Also, as we are interested in generalizing our results to actual accounting contexts, studies in natural settings can provide confirmation of the generality of the experimental findings. . . . The major limitation of the lens model approach is that, to take advantage of all of its power, it may only be used to study highly structured, repetitive tasks” [1977, p. 264].

situation is realistic to the subjects, if it involves them, if they are forced to take it seriously, if it has impact on them. In other words, an experiment is realistic if laboratory events are believed, attended to, and taken seriously. This type of realism is referred to as “experimental” realism. A second sense in which an experiment can be said to be realistic is if the events occurring in the laboratory are likely to occur in the “real world.” In other words, an experiment is realistic if the laboratory events are similar to real-world events. This type of realism is referred to as “mundane” realism.

We believe that the argument that laboratory experiments are artificial (i.e., not realistic) and therefore less useful may reflect a confusion between experimental and mundane realism. All experimenters strive for experimental realism. They try to have the greatest impact on subjects within the limits of ethical and other constraints. The art of experimentation is to use those procedures which will accurately capture variables of interest and also have the most impact on and credibility for subjects. Where some experiments achieve this impact and credibility,<sup>21</sup> other experiments may not capture even the attention of subjects, let alone influence their behavior.<sup>22</sup>

If laboratory experiments have experimental realism, there appears to be little, if any, need to strive for mundane realism.<sup>23</sup> The experimental situation may be meaningful for subjects even though it may be far removed from their experiences outside the laboratory. There are situations where adding mundane realism may help achieve experimental realism by making variables and treatments more salient.<sup>24</sup> But, even in these situations we are somewhat skeptical about the value of mundane realism per se, because it may make it more difficult to learn from the experiment.

#### MUNDANE REALISM AND INSTRUCTION

Mundane realism may make it more difficult for experiments to instruct. The difficulty may arise because mundane realism may stir together cognition and action so that distinct components and relations are no longer visible. Mundane realism may lead subjects to emit familiar, overlearned routines about which they are inarticulate. The routines themselves are likely to be smoothed performances that contain short-cuts, substitutions, and cryptic versions of the acts from which they were assembled. Overlearned skills, by definition, may be less sensitive to experimental manipulations than newer skills. Furthermore, realistic settings are rich in cues that reinstate significant past events for subjects.

<sup>21</sup>Concerns may arise about whether this impact and credibility would have been achieved under different conditions.

<sup>22</sup>Concerns about too much or too little impact go well beyond the issue of artificiality or lack of mundane realism.

<sup>23</sup>See Aronson and Carlsmith [1968] for a more complete discussion of this point.

<sup>24</sup>See Aronson and Carlsmith [1968] for specific examples.



Since these pasts differ, however, the meanings imposed may differ and unexplained variance may increase. Realistic events, because they may connect so fully with divergent needs, may produce overdetermined behavior that is driven by multiple meanings, most of which are not observed by either the subjects or the experimenter.

Thus, everyday events that occur in realistic laboratory settings may be more “real” and vivid, but when subjects respond to them their responses may be more taken for granted, more of a composite mixture, more overdetermined, and less easily influenced than newer responses in response to more artificial stimuli.

Mundane realism may severely edit and abbreviate action. It may remove much of what experimenters need to observe. Furthermore, it can never be complete. Any effort to reproduce the natural world will be incomplete and flawed. Nor does mundane realism short-circuit issues of generalization. The mere assertion that laboratory findings have external validity must still be confirmed empirically for the assertion to carry any weight. However, efforts to build mundane realism into experimental settings can solve specific problems; we shall describe these occasions shortly.

#### ARTIFICIALITY AND VERIFICATION

The basic advantage of deliberate artificiality is that it may allow for more direct tests of theory, and this more direct access to theoretical propositions may improve generalization because it is theoretical statements, not raw findings, that are used to explain phenomena in the real world. Strong theories generate more accurate predictions. This relationship is implicit in Mohr’s [1982, p. 5] description of theory development as the “development of powerful explanatory generalizations about human behavior where powerful signifies highly accurate with respect to a large and well-defined scope of occurrence of an important behavior.”

The concept of scope conditions is the key to understanding why artificiality may be beneficial. Any theory contains both generalizations and scope conditions. Generalizations assert relations among abstract concepts, and scope conditions “specify the circumstances under which the relationship specified in the assumptions is expected to hold true” (Webster and Kervin [1971, p. 266]). Scope conditions tell where the theory claims to be able to make its predictions.<sup>25</sup> Any situation, whether a natural empirical setting or an artificial laboratory environment, is an appropriate site for theory testing as long as it meets the scope conditions.

Most natural empirical settings contain more variables than are specified in any one theory. This means that any theory by itself cannot make precise predictions in natural situations because it incorporates only a handful of those factors that are present. Just because the theory leaves

<sup>25</sup>For example, the generalization that people with higher status have more influence in task-oriented groups but not in socioeconomic groups incorporates kind of group as a scope condition that limits the generalization about status and influence.

things out does not mean that it is necessarily trivial or irrelevant. It does mean that researchers can never get a clear confirmation or disconfirmation of the theory nor can they decide how much faith to put in the theory's generalizations. The clearest information on these points is generated when scope conditions not specified in the theory are removed or held constant. This stripping produces artificiality which, in the case of theory testing, can be an asset: "the more artificial the setting in the sense that it contains *all* and *only* the theoretically specified factors, the more precisely the one theory in question may be expected to predict" (Webster and Kervin [1971, p. 268]). Thus, experimenters may need more, not less, artificiality so that all variables that are not relevant to the theories being tested can be eliminated from an empirical setting.

The fact that artificiality may facilitate verification is known by most experimenters, yet they often overlook it. It gets overlooked when they work with underspecified, vague, or nonexistent theories and try to generalize anyway by applying findings directly. It gets overlooked when they use the natural situation rather than a theory to generate lists of scope conditions, and generate longer, more diffuse lists. It gets overlooked when theoretical variables are stated at lower levels of abstraction, thereby entailing longer lists of more specific scope conditions to pinpoint precisely where the predicted relationship should occur. And it gets overlooked because they forget that situations which are rare in the natural world often are ideal to test theoretical derivations. This lesson is forgotten because common events are used disproportionately as models for procedures, propositions, and predictions. Less common events are underrepresented, even though they may contain much more powerful generalizations.

#### ARTIFICIALITY AND DISCOVERY

Deliberate artificiality may promote not only theory testing, but also theory construction. This may be its more crucial role for accounting research. The basic argument is stated by Henshel [1980, pp. 471–72]:

In the context of discovery, however, one can perform an experiment by intentionally creating conditions that have no existing counterpart outside the laboratory. In place of the controlled reproduction of reasonably plausible situations (for the purpose of verification of hypotheses) we may seek new regularities *under conditions that do not exist anywhere in natural settings*. One utilizes one's advantage in the laboratory experiment of physical control. This control permits the arrangement of variables in novel configurations, thus creating *unique* physical or social environments in order to answer "what if" questions . . . . In such an experiment one does not attempt to verify regularities that may or may not *presently exist* in the "real" world (the world outside the laboratory); one rather seeks to discover regularities that are *capable* of existing, if only the outside world ever provided the appropriate conditions.

The biofeedback principle is a useful example of discovery. It operates in a patterned, lawlike manner yet does not exist visibly in nature unless conscious efforts are made to create it. "Experimentation that faithfully

reproduced external world conditions would never have detected it" (Henshel [1980, p. 473]). People are not capable of monitoring their own brain waves, skin temperature, or cardiovascular performance without special arrangements. Discovery occurred only when experimenters asked what would happen if people could see these data. The question itself would not have been asked if experimenters had not been skeptical about older classifications of bodily conditions capable of voluntary control. Biofeedback was discovered only when artificiality was maximized.

The literature of experimental economics provides another example of discovery. Laboratory markets were initially used to test the basic law of supply and demand; but as experimenters sought explanations for the convergence observed, they increasingly used these markets for discovery by focusing on information processing, signaling, information transfer, alternative market institutions, and so forth. The laboratory markets essentially became used to answer "what if" questions.<sup>26</sup>

Levine and Plott [1977] and Plott and Levine [1978] provide another example of the use of experiments to answer "what if" questions. They were interested in how an agenda might be used to induce a large group of individuals (a flying club) to choose a particular option (a specific fleet of airplanes) from a sizable set of possibilities. They obtained information about the pattern of preferences. Application of axiomatic social choice theory and game theory to these data suggested that the voting procedures might be influential when the group made its final choice. They then developed a simple model to help design an agenda which was intended to induce the group to choose the alternative that was preselected by the authors. The group used the agenda and the choice was the one predicted. Experimentation was then used to determine if the actual choice by the group was accidental:

Was the result a happy accident or was the decision a direct consequence of our efforts? In order to partially resolve this question, we turned to experimentation. If by using the methods we developed we were unable to influence groups involved in conflicts similar to the club meeting, then we would be willing to dismiss the club experience as an accident. The experimental results below indicate that the club decision cannot be dismissed as accidental. The principles we outline for determining the agenda's influence are in need of improvement, but their fundamental importance within a range of circumstances is established. (Plott and Levine [1978, p. 46].)

Most of the accounting experiments described in our brief survey tried to address "what if" questions. They attempted to study the effects of alternative accounting methods, additional disclosures, alternative formats and displays, accounting changes, alternative performance measures, and so forth on users of accounting information. These experiments

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<sup>26</sup>For example, a recent study by Smith et al. [1982] explores the extent to which some measure of the market's state (such as mean price) is nearer to a competitive equilibrium in double auction experimental markets than in markets organized under a different institution of contract (such as a sealed-bid auction).

were designed to create conditions that had no existing counterpart outside the laboratory. Trying to study the effects of alternative accounting methods, additional disclosures, etc., in natural settings would require the type of efforts described by Campbell [1969] in his classic article, "Reforms as Experiments."

What is especially appealing about using experiments to discover new phenomena is that this does not automatically preclude generalization. Rather than use lawlike statements as the medium to transfer the laboratory finding into the real world, the experimenter rearranges the real world so that it duplicates the novel conditions created in the laboratory. When the real world resembles the laboratory, events that unfold in one place are likely to unfold in the other one. And this similar unfolding occurs without any necessary mediation by theory. Henshel [1980, pp. 474–75] used the common laboratory task of a five-person communication network to illustrate the argument that alterations in the real world can facilitate generalization:

For example, of experiments in communication networks of the Bavelas variety it may be asked: Where do we actually find networks of communication like that? How can such findings be generalized to nonlaboratory settings? If they cannot be, then of what use were these artificial experiments? But such questions mistake the purpose of investigating phenomena under unnatural conditions. Instead of asking whether an interesting phenomenon produced in a social laboratory can be found at present outside laboratory walls, so that the laboratory finding can be generalized, one *might* ask: Does the investigation uncover a *desirable result* (for example, high morale) that can be lawfully described in the confines of the experiment, even though the regularity may be otherwise nonexistent? If so—if (a) the result was a desirable effect and (b) the conditions were "unnatural"—then perhaps the theory guiding the experiment can be used to create that result beyond the laboratory walls by deliberately reproducing the strange, hitherto nonexistent conditions in the outer world . . . Similarly, experiments in social communication networks may have little worth in explaining and predicting existing social phenomena. However, if it seems desirable, such networks can be created outside the laboratory precisely to produce the effects detected in the laboratory. Whenever a potentially beneficial effect is observed in the laboratory, the objective might be *to make the external world match the laboratory, not to make the laboratory match the external world.*

Contemporary computer installations with isolated consoles increasingly resemble the stark "artificial" laboratory of partitions, message slots, and no face-to-face contact first constructed by Bavelas. Furthermore, the importance of network centrality that he observed in the laboratory is now being played out again for higher stakes in debates over who has access to whose data.

Applied to accounting research, Henshel's argument implies that preoccupation with the Brunswik lens model may not be as parochial as some might claim if there exists the possibility that actual accounting practice can be altered so that it conforms more closely to practices used in the laboratory to test the lens model. A more refined version of this point is that the lens model may have more explanatory power for actual accounting judgments, the closer the setting in which those data are gathered resembles the settings in which accounting judgments are made. Similar-

ity can be created from either side. Either the laboratory can be made more like the world (simulation) or the world can be made more like the laboratory (organizational design).

#### THE ROLE OF MUNDANE REALISM

Realistic settings do not preclude either verification or discovery, but, with two exceptions, they may make both activities more difficult and less instructive. The two exceptions involve complex theories and situated meanings.

Theories vary in complexity. Precise tests of theories that predict interactions among larger sets of variables require more complex experimental settings. Realistic scenarios are often an efficient way to package the variables in complex theories so they do not overload subjects (Fromkin and Streufert [1976]). Thus, complex theories can best be tested in complex settings, and complexity becomes more plausible and manageable the more it is operationalized within realistic events.

This straightforward relationship is itself more complex than it looks. Complex theories tend to characterize earlier rather than later stages of inquiry. Events seem complex and are described with intricate notation largely because at first we do not have a clear idea about what is going on. As understanding improves and as explanations become more economical, the requisite complexity and mundane realism of experimental settings can be reduced. Increased theoretical understanding is signaled by increased artificiality of the settings in which theories are tested. Thus, we may need more mundane realism to address those problems about which we know less and more artificiality to address those about which we know more.<sup>27</sup>

Complex theories have their origins not just in ignorance, but also in thick descriptions of the natural world. Complex theories often represent modest inductions from descriptions of complex settings. This means that those same complex settings will need to be reproduced to provide a fair test of theories that largely recapitulate most of the complexity that was originally observed. Again, one of the more efficient means to test theories that have concrete referents may be to build realistic settings that resemble the starting point for the ideas. Modest induction, just as was true with ignorance of the phenomenon, tends to be associated with earlier rather than later stages of inquiry.

When experimenters do not know much about a phenomenon, either high artificiality or high mundane realism can be instructive. Artificiality allows for "what if" questions; mundane realism allows for accurate tests of initially complex theories. Blends of mundane realism and artificiality, however, may impede learning. If mundane realism is added to an artificial setting, interactions may become concealed and discovery may be incomplete. If mundane realism is compromised by artificiality, theo-

<sup>27</sup>This assumes that theories about the former are more complex than are theories about the latter.



retical complexities may be removed and verification may be incomplete. We believe that experimenters may have learned less from experiments than they could have because they have tried simultaneously to increase external validity by adding mundane realism and internal validity by adding control. The resulting compromises may have achieved neither. Mundane realism is not necessarily a liability, especially in early stages of inquiry, nor is artificiality a liability if results are embedded in theory.

Aside from providing appropriate sites to test complex theories, mundane realism can also supply vivid *common* meanings to experimental situations, thereby facilitating generalization. "The meaning the subjects assign to the [experimental] situation they are in and the behavior they are carrying out plays a greater part in determining the generalizability of an experiment's outcome than does the sample's demographic representativeness or the setting's surface realism" (Berkowitz and Donnerstein [1982, p. 249]).

Generalization occurs not only when findings are embedded in theory, but also when psychological attributes such as definition of the situation, interpretation, and judgment are common across situations. Earlier, we noted that mundane realism can diversify rather than focus interpretations, so that it should be used with caution. For example, the artificial activity of delivering an electric shock to another person seldom occurs in everyday life, yet it focuses the definition of the situation on one where the perpetrator is intentionally hurting another human being. However, there are issues where more mundane realism can have the same effect of focusing attention. For example, the more realistic setting produced by having a weapon present when a person delivers shock also implants a common meaning—aggression is tolerated here—that would be difficult to create by more artificial props. In the case of the mere sight of a weapon, subjective definitions become both more homogeneous and more like definitions in everyday life that enhance the incidence of aggression. Theory plays a secondary role in generalizations made under these conditions, while common meaning plays a more prominent role.

Some meanings are tied to specific everyday settings and can be produced in a vivid, controlled, homogeneous manner only when those settings are reproduced accurately. If researchers investigate such phenomena and if they regard meaning rather than theory as more important for generalization, then mundane realism can be beneficial.

## CONCLUSIONS

The following conclusions emerge from our reconsideration of realism. First, good theory may be more important than good mundane realism. Since theories rather than findings are the means for generalizing from the laboratory to the field, powerful ideas may aid understanding more than a surface resemblance between the laboratory and the field.

Second, the field can be less real than the laboratory. The field can be less real than the laboratory because it conceals and abbreviates many



basic relationships from which other relationships derive. The field merely reflects the final edited version of quite different processes that happened earlier, more privately, on a smaller scale, and that over time have become modified into what we now arbitrarily call the real world. Those earlier origins were and are also real. People who describe the present real world have trouble observing those different earlier processes that give it the shape it now has. The irony, therefore, is that the laboratory now becomes the real world in which one can observe more clearly what is actually going on in concealed form in that more abbreviated artificial world our subjects wander in from.

Third, conventional laboratory settings already contain as much organizational realism as experimenters may want. Our earlier description of the laboratory as the embodiment of Theory X demonstrated that what happens in the laboratory happens all the time in real organizations. While subjects' apprehension about being evaluated may be troublesome for other research topics, it is a prominent feature of accounting settings and simultaneously adds both mundane and experimental realism to accounting experiments.

Fourth, mundane realism may postpone discovery. Research is most helpful when it adds to commonsense understanding rather than duplicates it. Additions are most likely when people examine the consequences of novel combinations of variables, combinations that would never occur visibly in the natural world they summarize as common sense. Research settings that duplicate the real world from which common sense derives typically demonstrate what everybody has already found to be true of those settings. This redundancy is visible in numerous accounting experiments. As mundane realism is replaced by uncommon contingencies, common sense provides weaker predictions about what should occur and instruction increases. This is happening now in experimental economics and provides a model for other areas.

Fifth, trade-offs between artificiality and mundane realism are self-defeating. Because generalizability has been equated with surface resemblance of settings and similar demographics among subject populations (e.g., surrogate subjects), experimenters may have felt compelled to make their controlled settings as realistic as possible. This hedging has made it harder to observe what is happening, but since the mundane realism is incomplete, it has also made it harder to generalize the results since no other setting resembles the hybrid created in the laboratory. We believe that movement away from hybrids toward greater artificiality and greater mundane realism will improve understanding.

### *Summary and Conclusions*

"Let the jury consider their verdict," the King said, for about the twentieth time that day.

"No, no!" said the Queen. "Sentence first—verdict afterwards."

—Lewis Carroll, *Alice's Adventures in Wonderland*

Our brief survey of the experiments listed in Appendix A suggests increasing activity by experimentalists in accounting.<sup>28</sup> But, accumulation of knowledge requires more than an increasing literature. We believe that recent experiments in accounting reflect significant progress over earlier studies. These experiments reflect more reliance on formal models or theories, more ties to the work of others, and more rigor (e.g., more precision and control).<sup>29</sup> However, we have raised some questions about the design and content of experiments which have used simulations, generic tasks, and judgment tasks. In particular, we have expressed our concern about the increased structuring of stimuli (antecedent conditions) and of responses to these stimuli (restrictive responses) because it may limit observation by increasing the distance between the observers and the actors and may limit instruction—what we can learn from experiments.

We have refrained from rendering a verdict about these experiments and have, instead, recommended a sentence. We have recommended that experimenters adopt a more differentiated view of what they can do in order to learn from experiments. We have discussed the trade-offs between structure and freedom in inquiry and between instruction and control. We have suggested that experiments be viewed as ways to systematize observing in the interest of instruction. This view provided the context for discussing several subtleties of experimentation. Several potential limitations of experiments for purposes of control, such as illusion of control, Theory X settings, stimulus-response designs, alternative interpretations, insensitive methods, and so forth, may provide opportunities for instruction.

We also have recommended that experiments adopt a more differentiated view of artificiality and mundane realism. We have described how artificiality may facilitate direct tests of theory and also theory construction and discovery. We have described how mundane realism may limit instruction in some experimental situations because it can never be complete and may evoke severely edited and abbreviated actions. But, we have described how mundane realism can facilitate the testing of complex theories and can provide common meanings for some experimental situations.

In conclusion, we believe that continued progress can be achieved by reconsidering the trade-offs between instruction and control and between artificiality and mundane realism. Because accounting experimenters have achieved a fair amount of precision and control in their experiments, they can now consider the instructiveness of their experiments as well. Because they have tried to blend artificiality and mundane realism in

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<sup>28</sup>Just when accounting researchers are embracing laboratory experiments more enthusiastically, psychologists appear to be growing more skeptical about their value (e.g., Gergen [1978]). Who should listen to whom?

<sup>29</sup>See Dyckman, Gibbins, and Swieringa [1978] for a discussion of these and other criticisms of earlier experiments in accounting.

their experiments, they can now consider more pure expressions of each. It is in this sense that we agree with Ellis' statement that "what we call progress is the exchange of one nuisance for another."

## APPENDIX A

### *Experimental Research in Accounting*

#### 1970

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