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Generalizing from "Imperfect" Replication*

An analysis of variance approach is suggested for evaluating the impact of various salient interstudy differences on the results of comparable but not perfectly replicative studies. As an application, multivariate analysis of variance is used to assess differences in parameter estimates across 37 "quasi-replicative" studies based on the Fishbein Behavioral Intention Model.

At the core of research on most marketing problems lie attempts to assess relationships between variables which are important to understanding how choice processes work and how they are affected by various factors which are either controllable or assessable by the marketing manager. These relationships may be aggregate and may involve hard, economic measurement (sales and advertising by brand, for example), or they may be softer and more disaggregate (response of different segments of consumers to certain advertising copy, for example).

As understanding of buyers' choice processes builds up, so do collections of studies which use similar research techniques to approach similar sorts of problems. While these bodies of literature, published and unpublished, generally differ in enough important respects that they cannot reasonably be viewed as replications in a technical sense, they often use approaches similar enough to one another that they might be viewed as "imperfect" replications. In fact, methodological differences in a set of studies may allow insights not available from either a set of "perfect" replications or a single study.

This paper proposes a procedure for grouping comparable studies into a quasi-experimental design and using analysis of variance procedures to

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assess systematic differences in results which may result from salient differences among the studies. Relevant differences over a given set of studies can be considered quasi-experimental treatments in the ANOVA framework, with appropriate and comparable output measurements from the quasi-replicative studies serving as a univariate or multivariate dependent variable.

Replication in Consumer Research

While replication lies at the heart of generalization of any body of knowledge, consumer research lacks replication for a variety of reasons (Kollat, Engel, and Blackwell 1970). Even in areas with a body of research centering on related issues, there is often lack of interstudy consistency in design (Jacoby 1978), in application (O'Shaughnessy and Ryan 1979), and in conceptual basis (Finkleman 1978).

If we divide replication into exact replications (parallel studies) and related (converging) studies (Gardner, Hake, and Erickson 1956), we can see perfect replication almost never appears in the published literature in consumer research. Given the interdisciplinary nature of consumer research, the resulting different interests of researchers and demand for originality in design and execution, it seems unlikely that a body of exact replication will soon exist. There are, however, areas of inquiry with fairly sizable collections of studies related in terms of theoretical underpinnings, data collection procedures, and statistical methodologies. These "imperfect replications" will generally differ in regard to important theoretical and methodological issues, as different specifications of a model or different variable operationalizations may be employed by different researchers.

Examples of attempts to organize, synthesize, and generalize from such collections include reviews of research on advertising carry-over effects (Clarke 1977) and multiattribute consumer choice models (Wilkie and Pessemier 1973). Schultz and Leone (1980) have also attempted to gather propositions in a variety of areas by classifying results for groups of studies in terms of consistencies of qualitative effects. The difficulty inherent in comparing results in imperfect replications is evidenced by a matrix in Wilkie and Pessemier's review in which the 42 studies examined differ on 31 potentially salient dimensions.

Replication Analysis

The approach suggested here to develop generalizations from a set of imperfect replications involves three steps: (1) collecting studies which contain comparable interrelationships of constructs; (2) identifying characteristics of studies that might cause systematic differences in these relationships and configuring them into an after-the-fact quasi-

experimental design; and (3) using analysis of variance to determine which interstudy differences affect the relationship. The approach has similarities with other attempts to generalize models of consumer behavior in terms of both predictions of values of variables and of interrelationships among them. For example, Ehrenberg (1972) has sought empirical regularities ("laws") directly in patterns of brand assortments and purchase occasions for a wide array of durable and semidurable products. The approach used in this paper, however, is more in the spirit of methods recently proposed for systematizing consistencies across studies in cases where consistencies were not sought by the individual writers (Kassarjian 1977; Rice 1978) and in developing overall type 1 error probabilities by combining independent but exact replications (Rosenthal 1978).

Step 1: Collecting Studies in Relevant Settings

The estimated relationships among variables obtained from various studies provide values of the dependent variable. Parameter estimates from the various studies may not be in directly comparable units due to differences in measurement of estimation methodology. However, results may be available in some scaleless form such as a correlation coefficient of some order or beta coefficient. If not, an elasticity can be derived from a regression coefficient, or some sort of "normalized" measure can be developed which avoids inappropriate comparison of utilities over subjects or over studies (Bass and Wilkie 1973).

The approach is most promising when replications are not "too" imperfect, that is, when the following conditions are met: (1) a well-developed theory exists, preferably with the form of functional relationships among the variables specified; (2) a relatively small set of empirical results is of interest; (3) output measurements from the various studies are expressed in or easily converted to comparable units; (4) measurement and estimation methodologies are easily comparable over studies; and (5) enough studies exist to provide a sample size adequate for estimation of ANOVA parameters.

Examples of set of studies with potential include:

1. Thirty-seven applications of the Fishbein behavioral intention model which meet all five conditions are used as an illustrative example later in this paper. Despite similarities, these studies involved several interstudy differences in terms of design and execution.

2. The 69 applications of Koyck distributed lag procedures discussed by Clarke (1977) meet conditions 2, 3, and 5 reasonably well, although the underlying theories are not as explicit as the Fishbein applications and the results as a whole are technically problematic. There are, however, a rich set of potentially relevant differences over studies, including differences in the number of explanatory variables used and variety in the products, markets, and measurement periods.

3. Various econometric estimates of demand elasticities with regard to price, advertising, or distribution, and various aggregate econometric estimates of marginal or average propensities to consume offer a somewhat more complex problem (Farris and Albion 1980). Elasticities are expressed in common units, and estimates from a wide variety of applications should provide reasonably large samples of estimates with comparable theoretical foundations. However, the estimates generally come from structurally different models, involve different measurements in terms of time frame, and are estimated using different types of data (aggregate time series, consumer panel reports, etc.) using a variety of estimation procedures. In this case, the design of the quasi experiment may be so complex that few "quasi replications" are available for within-cell analysis of the type feasible with the Fishbein and Koyck studies.

4. Studies estimating relationships among a number of sociopsychological variables are available which use the Howard and Sheth (1969) model as a basis. There are two major difficulties in using this approach with these studies. First, estimates are based on diverse measurement and estimation procedures, although these can be converted into common units of measure by calculating elasticities from the regression estimates. Second, the theory is specified qualitatively and functional relationships are not generally explicit. Complex sets of empirical relationships may yield up to a hundred parameters in a given model and only a handful of replications are available for comparison (Farley and Lehmann 1977). In order to aggregate results from various studies, it may be necessary to make strong assumptions about equilibrium states for combinations of parameters involving several different variable pairs in order to build up acceptable sample sizes. Even with these assumptions, a complex design with few within-cell replicates and many empty cells is likely to result.

These four examples represent increasing levels of difficulty faced in application of the "imperfect replication" approach, but the more difficult applications are likely to produce more benefit. Since the underlying studies are expensive and complex in these cases, it is especially useful to be able to estimate outcomes under certain new conditions, which are in effect empty cells in the design, without having to execute additional complex studies.

Step 2: Identification of Relevant Differences among Studies

Based on either theoretical or methodological considerations, factors which might vary over studies and be used as quasi-experiment variables include: form and structure of the model; variable or construct definitions; time frame; product, object, or concept being studied; researcher characteristics or discipline; research design and related methods of data collection; estimation procedures used; and type of

sample. This list is not exhaustive and will vary over applications, but there should be prior expectations of which factors might be relevant in any particular set of studies.

Given the nature of imperfect replications, many factors may vary across studies. Some will be idiosyncratic to individual studies, and it will not be possible to separate their effects from the random error associated with their estimates. Other factors may be involved in several studies so that a pooled estimation of their impact is possible. The factors as well as interactions must generally be ordered a priori, as it is unlikely that a collection of studies will fit neatly into any formal experimental design, even including various unbalanced or incomplete designs. Further, it is likely that certain problems (e.g., nested effects and empty cells) will plague attempts to unambiguously identify some sources of systematic variation, and only limited within-cell replication (necessary to make inferences about interaction effects) will be available. Because of this, it will generally be necessary to select a subset of direct effects and specific interactions for analysis.

Step 3: The Use of Analysis of Variance

With dependent variables defined in step 1 and design determined in step 2, analysis of variance is used to adjust original measurements for the quasi-experimental effects to produce the generalizations over studies that are being sought. If, in addition, there are measurements which are metric (e.g., sample sizes in situations in which the array of dependent variables includes an estimate of variability of an estimated parameter), these can be incorporated into the analysis as covariates (Farley et al. 1979). This use of ANCOVA and MANCOVA requires explicit specification of an error structure associated with parameter estimates from the contributing studies, and this specification raises questions about use of statistical inference on "known" numbers. However, since estimated model parameters in most studies are subjected to statistical tests (chiefly t-, F-, and χ^2-tests which assume normality of deviations between respective estimated and hypothesized values), specification of a normal error structure in the ANCOVA is a reasonable extension of the statistical methodologies used in the original studies.

An Application: Patterns of Parameter Estimates in Tests of Fishbein's Behavioral Intention Model

Studies based on the Fishbein model of behavioral intention are numerous, have similar structures in terms of both measurement and analysis, and estimate few intervariable relationships. The central equation in Fishbein and Azjen's theory (1975), common in many applications, is: $B_1 = (A_{act})\omega_1 + (SN)\omega_2$, where ω_1 and ω_2 are empiri-

cally determined standardized regression coefficients (beta weights) and thus directly comparable over studies. Measures of goodness of fit of the model are similarly scaleless and are also comparable over studies.

Variables are defined as follows: B_I = behavioral intention; A_{act} = attitude toward behavioral act, which may be measured directly or computed as $\Sigma_{j=1}^{n} B_i a_i$, where B_i is the probability that the performance of a specific behavior will lead to an ith outcome, a_i is the positive or negative evaluation of the ith outcome, and n is the number of salient outcomes; SN = the subjective norm, which can also either be measured directly or computed as $\Sigma_{i=1}^{k} NB_j MC_j$, where NB_j is the probability that the performance of a specific behavior is expected by a jth group or individual, MC_j is the motivation to comply or not to comply with the expectation of the jth group or individual, and k is the number of salient groups or individuals.

Twenty-six studies have reported 37 tests of Fishbein's model that incorporated comparable measurement and parameter estimation. These tests were included in nine of 11 papers reviewed by Azjen and Fishbein (1973) (Fishbein 1966; Carlson 1968; Azjen and Fishbein 1969, 1970, 1972; Fishbein et al. 1970; Azjen 1971; Darroch 1971; Devries and Azjen 1971); eight of 10 reviewed by Ryan and Bonfield (1975) (Lutz 1973*a*, 1973*b*; Ryan 1973, 1974; Wilson, Mathews, and Monoky 1973; Bonfield 1974; Weddle and Bettman 1974; Wilson, Mathews, and Harvey 1975); plus nine others (Bright and Stammers 1971; Glassman 1971; Jaccard and Davidson 1972; Schwartz and Tessler 1972; Cowling 1973; Davidson and Jaccard 1975; Werner, Middlestadt-Carter, and Crawford 1975; Ryan 1978; Vinokur-Kaplan 1978). Each produced a set of three parameters (two variable weights and one goodness-of-fit measure), with means, standard deviations, and ranges as indicated in table 1. Sign and t-tests indicate a significant positive value for $\bar{\omega}_1$, $\bar{\omega}_2$, and $\bar{R}$, and all three parameters tested statistically as unequal to zero in most individual cases. Sample sizes ranged from 20 to 282, averaging 103.

Despite an unusual degree of consistency in theory and method, the differences across individual studies can lead them to be viewed as imperfect replications which use the same model but different situations and measurement methods. Some of the differences are idiosyncratic (e.g., prior hypotheses about the relative weighting of the predictor variables and inclusion of a third predictor variable) and do not provide a basis for grouping the studies. In contrast, five more general differences which might influence the parameters can serve as a basis for grouping and hence as classification suitable for an ANOVA:

1. The form of the attitudinal variable—either A_{act} or $\Sigma B_i a_i$.
2. The form of the normative variable—either SN or $\Sigma NB_j MC_j$. Recently, $\Sigma B_i a_i$ and $\Sigma NB_j MC_j$ have been proposed (Fishbein and Ajzen 1975; Ryan and Bonfield 1975) as antecedents to A_{act} and SN,

TABLE 1

	$\bar{\omega}_1$	$\bar{\omega}_2$	$\bar{R}$
Mean	.450	.301	.709
Standard deviation	.216	.227	.124
Range	−.15–.75	−.03–.95	.600–.940
Interquartile range	.27–.64	.21–.55	.670–.840
Number positive	36	36	

respectively. If this is so, A_{act} and SN should be stronger predictors of B_1 than $\Sigma B_i a_i$ and $\Sigma NB_j MC_j$.

3. The method of inquiry—relational tests carried out within or without an experimental design. There is an inherent tradeoff in emphasizing internal or external validity (Runkel and McGrath 1972). The tight controls utilized in an experiment should lead to better model fit than that obtained in a survey setting.

4. The researchers' predominant discipline affiliation—either marketing or social psychology. Accepted sins of omission and commission vary across disciplines in regard to research standards. Psychologists, for example, have a long history of concern for measurement issues that have only recently emerged in marketing research (Ray 1979). Greater concern with measurement issues should reduce the random error in the model, thereby increasing obtained fits.

5. The type of subjects employed, either students or "real world" respondents. The use of student samples has long been debated in the marketing literature (e.g., Alpert 1967; Sheth 1970; Ferber 1977).

Results

Since the three criterion variables are continuous and the five systematic differences among studies are dichotomous, differences in parameters can be analyzed with a 2^5 multivariate analysis of variance (MANOVA) (Bock 1975). It is obvious that the available 37 observations cannot fill each of the 32 cells with the replications necessary for a complete analysis of all interactions of the full factorial design. Further, since the occurrence of observations in cells was not the result of an experimental design, 15 of the 32 cells in a full factorial design were in fact empty and nine cells had only one observation. As a first step, a nonorthogonal multivariate analysis of variance (MANOVA) was performed (Bock 1975) which was restricted to testing main effects pooling the within cell and interaction sum of squares as the error term. Of the five main effects, only the discipline of the researcher was significant (table 2).

To seek an explanation for these results, analysis of a series of four 2×2 factorial designs crossing the researcher's discipline main effect

TABLE 2 Five Factor Main Effect Dependent Variable Means and MANOVA

Variable	$\bar{\omega}_1$	$\bar{\omega}_2$	R	N
Attitudinal form, $F(3,29) = 2.76, P \leq .06$:				
A_{act}	.46	.28	.72	28
$\Sigma B_i a_i$	.41	.37	.67	9
Normative form, $F(3,29) = .86, P \leq .47$:				
SN	.45	.25	.72	13
$\Sigma NB_j MC_j$	.45	.33	.71	24
Researcher discipline, $F(3,29) = 7.2, P \leq .001$:				
Social psychology	.43	.38	.79	18
Marketing	.47	.23	.64	19
Research method, $F(3,29) = .87, P \leq .46$:				
Experimental	.46	.35	.73	13
Correlational	.44	.27	.70	24
Type of sample, $F(3,29) = .41, P \leq .74$:				
Students	.48	.29	.73	26
Real world	.39	.34	.65	11

with each of the other four main effects produced at least four observations in each cell. Of these, only the analysis-crossing attitudinal form and the researcher's discipline produced significant effects (table 3). A discriminant analysis, which explicitly considers joint effects indicated by the multivariate F-tests, was performed to determine which dependent variable contributed most heavily to respective between-group differences. The standardized weights, also shown in table 3, indicate that ω_2 contributed most heavily to the attitudinal main effect differences whereas R contributed most heavily to the effect of the researcher's discipline and the interaction.

The interaction effect, graphed in figure 1, shows that social psychologists obtained better fits than did marketing researchers regardless of attitudinal form. This finding is consistent with the expectations mentioned earlier. Social psychologists also obtained a better fit with the $\Sigma B_i a_i$ attitude form than with the A_{act} form, whereas the opposite held for marketing researchers. This finding has important implications since A_{act} should consistently provide a better fit than $\Sigma B_i a_i$ if in fact $\Sigma B_i a_i$ is an antecedent to A_{act}. This may be due to the greater reliability of the $\Sigma B_i a_i$ scale where the tendency of correlated responses to "halo" (Beckwith and Lehmann 1975) makes the averaging properties of a multi-item scale a better measure of attitude than the single-item A_{act} in spite of the tendency for multiplication of scores to decrease reliability (Bohrnstedt and Marwell 1978). The method used to select attitudinal outcomes also help explain the better fit obtained by social psychologists when using the multiplicative form of attitude. Because

TABLE 3 **2 × 2 Factorial Dependent Variable Means, MANOVA, ANOVA, and Standardized Discriminant Weights**

I. Means

	$\bar{\omega}_1$	$\bar{\omega}_2$	$\bar{R}$	N
Interaction of discipline and attitudinal form:				
Social psychology × A_{act}	.41	.36	.77	14
Social psychology × $B_i a_i$	.48	.43	.83	4
Marketing × A_{act}	.51	.20	.67	14
Marketing × $B_i a_i$	.35	.32	.53	5

II. Inferential Tests

	Univariate $F(1,33)$	Standardized Discriminant Function Coefficient
Attitudinal form main effect, $F(3,31) = 3.01, P \leq .045$:		
$\bar{\omega}_1$	$.38, P \leq .54$	−1.18
$\bar{\omega}_2$	$1.21, P \leq .28$	−1.63
R	$2.99, P \leq .09$	−1.18
Researcher's discipline main effect, $F(3,31) = 8.87, P \leq .001$:		
$\bar{\omega}_1$	$.34, P \leq .56$	−.42
$\bar{\omega}_2$	$4.51, P \leq .041$	−.27
R	$26.67, P \leq .001$	−1.09
Attitudinal form × researcher's discipline interactions, $F(3,31) = 3.43, P \leq .029$:		
$\bar{\omega}_1$	$1.80, P \leq .19$	−.11
$\bar{\omega}_2$	$.13, P \leq .72$	−.53
R	$9.05, P \leq .005$	1.08

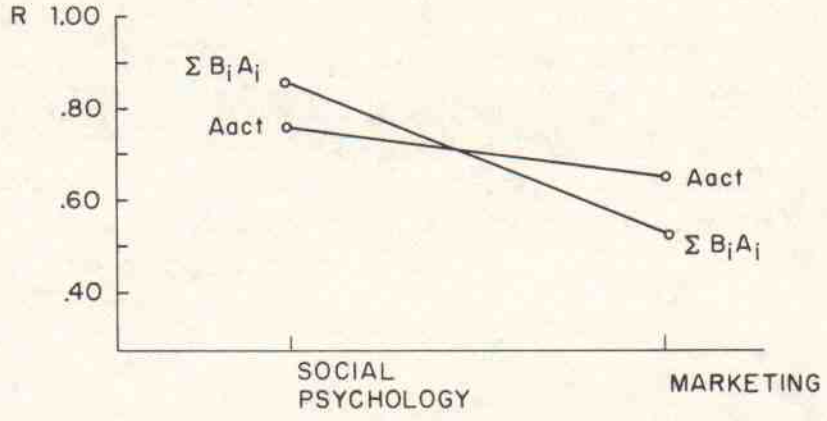

FIG. 1.—Attitudinal form × researcher's discipline means for dependent variable R.

marketing researchers often use lists of attributes predetermined on the basis of prior research or manager's experience, while social psychologists more often elicit outcomes from the respondents before belief and evaluation statements are constructed (Ryan and Etzel 1976), social psychologists may be using more salient outcomes. Unfortunately, the methodology reported in the original studies does not provide an explanation of how outcomes were determined for 19 of the studies, so a formal test of this issue is not possible. The main attitudinal effect means, shown in table 2 for ω_2, indicate that the use of the $\Sigma B_i a_i$ attitudinal form shifted predictive power from attitudinal to the normative variable (SN or $\Sigma NB_j MC_j$); ω_2 was thus larger when the $\Sigma B_i a_i$ form of attitude was employed. This result may also be due to the inherent unreliability in multiplied scale scores.

In terms of overall patterns, the 37 studies using the Fishbein model are more nearly replicative than might initially appear in the sense that model parameters showed little variation systematic with the five main effects used in MANOVA. The magnitude of the statistically significant effect of researcher discipline was small, and the differences that were found were chiefly in the measures of goodness of fit.

The insignificant effect of four potentially major sources of variation (attitudinal form, normative form, research method, and type of sample) has useful implications. For example, the traditional concern that parameter estimates obtained with student samples do not represent the "real world" (e.g., Ferber 1977) does not appear to apply to the Fishbein results. Guidance may also be available for needs for future research that would not normally result from conventional qualitative literature review. For example, results on attitudinal form (table 2) are nearly significant. Since the cells contain a disproportionate distribution of observations (9 vs. 28), more work using the multiplicative attitudinal form may be needed to provide closure on this measurement-related issue.

The approach also provides a richer set of "null hypotheses" for comparison of work extending the basic design used in this analysis. For example, in the only test employing an unobtrusive measure of real world behavior, Ryan and Bonfield (1980) produced parameter estimates significantly greater than zero but smaller than the averages for these 37 studies, all of which used more obtrusive data collection procedures. Bentler and Speckart (1979) also have suggested an expanded model with additional variables which can use these results as a starting point for evaluating the expanded parameter set.

Conclusion

This approach to synthesizing results from imperfect replication makes constructive use of potentially salient differences among studies that

can be classified at least so that a given feature is not unique to a single study. It would be useful to have systematically designed differences in a set of studies prespecified as experimental variates or covariates in extensions of this approach. Even if a particular researcher does not wish to engage in an entire "designed" research program of this sort, it would be useful for each new application of a given model to consider the role that it might play in filling in the quasi-experimental design—for example, is a new piece of work a replication of a cell which already has multiple entries but which has disproportionately few cases of one particular treatment, is it a replication of a single-entry cell, or is it the first entry in an experimental cell heretofore empty?

Of course, the application of the approach to the Fishbein literature involves nearly ideal conditions in terms of comparability over studies of measurement methods, estimation procedures, and model structures. Slightly different results were obtained by researchers in different fields, especially in terms of goodness of fit, but we would predict average multiple correlations of future studies will be about .7 and that ω_1 will be larger than ω_2 (about .45 vs. .3).

We expect that the approach presented here will prove useful in generalizing for many sets of studies, including those less well structured from the point of view of the quasi-experimental design. In fact, the approach is likely to prove useful here in situations in which maximal information must be extracted from a minimal set of replications per cell and where those replications are not perfect. While "perfect" replication is clearly useful, imperfect replication, when systematically analyzed, can also be useful for advancing knowledge. Since imperfect replication can separate model from method, discovering how method differences affect results may be as helpful as estimating model parameters under a given set of conditions in an individual study.

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