DOCUMENT RESUME

ED 128 361 TM 005 483

AUTHOR Cronbach, Lee J.; And Others
TITLE Analysis of Covariance: Angel of Salvation, or Temptress and Deluder?
INSTITUTION Stanford Univ., Calif. Stanford Evaluation Consortium.
PUB DATE Feb 76
NOTE 47p.
AVAILABLE FROM Stanford Evaluation Consortium, School of Education, Stanford University, Stanford, California 94305 ($1.00)

EDRS PRICE MF-$0.83 HC-$2.06 Plus Postage.
DESCRIPTORS *Analysis of Covariance; Error Patterns; Mathematical Models; Multiple Regression Analysis; Predictive Validity; *Research Design; Sampling; *Statistical Bias

ABSTRACT

The unbiased estimate of a "treatment effect" reached by analysis of covariance in a nonrandomized experiment would often require that a different covariate be used in each treatment. A sufficient but unlikely condition for an unbiased estimate is that the covariate for each treatment is (1) the complete covariate that predicts the outcome as fully as possible from initial characteristics of the case, or (2) the complete discriminant that fully represents differences between group means on the initial characteristics. When the covariate for a treatment is a weighted composite of (1) and (2), the adjusted outcome mean may have positive or negative basis, the bias being a nonlinear function of the weights. If (1) and (2) do not wholly account for the covariate, the adjustment to the outcome mean is reduced in absolute magnitude. A procedure is offered to correct for error of measurement when there are two or more fallible covariables. Specification errors as well as errors of measurement have an attenuating effect, however. Since the parameters of the specification errors are unknown, no correction procedures can be counted on to provide an unbiased estimate of the treatment effect. (Author/BW)

OCCASIONAL PAPERS OF THE STANFORD

Evaluation Consortium

Stanford University, Stanford, California, 94305

ANALYSIS OF COVARIANCE:

ANGEL OF SALVATION, OR TEMPTRESS AND DELUDER?

Lee J. Cronbach, David R. Rogosa,

Robert E. Floden and Gary G. Price

February, 1976

Analysis of covariance -- Angel of salvation, or temptress and deluder?

Lee J. Cronbach, David R. Rogosa, Robert E. Floden and Gary G. Price

Stanford Evaluation Consortium, Stanford University

In comparing social and educational programs, treatment effects are confounded with effects of subject characteristics when self-selection or a nonrandom assignment rule determines who enters (or who completes) a treatment.

In an attempt to remove confounding, the investigator might confine his comparison to subsets of cases whose distributions on one or more initial measures are the same within all treatments. It was established long ago (Rulon, 1941; Thorndike, 1942) that such *post hoc* matching of cases produces a biased conclusion. Analysis of covariance (ancova), the usual current method of coping with nonequivalence, uses the within-groups regression of outcome on a characteristic measured prior to treatment to adjust the outcome measure. This purports to estimate the treatment difference for persons who were alike at the outset. It is in effect a matching procedure; but few users of ancova have been aware that it too is biased.

Two decades ago Cochran (1953, esp. p. 689) drew attention to the bias, but he was almost unique among statisticians in making this point. If the covariate does not fully reflect the difference between experimental and control groups, he suggested, analysis of co 'ance underadjusts. More forcefully, Campbell and Erlebacher (1970) charged that statistical bias may have produced the negative conclusion in the Westinghouse evaluation of Head Start. They asserted that whenever the treatment groups differ initially, ancova will not adjust adequately for the initial group differences. The adjusted outcome mean of the "more able" group will be unduly high, relative to the adjusted outcome mean of the "less able" group. Much the same argument appears in Campbell and Boruch (1975).

Their assertion is rapidly becoming part of the conventional wisdom of social scientists. A recent authoritative statement on policy research (Riecken and Boruch, 1974, p. 109) not only asserts that ancova and similar methods "underadjust for the latent group differences" but recommends that the person analyzing posttests "should live with such pretest differences rather than try to adjust them away . . ." As other examples of this pessimism in social science, we note Lord's statement (1967, p. 305; see also Lord, 1969) " . . . that with the data usually available for such studies there simply is no logical or statistical procedure that can be counted on to make proper allowances for uncontrolled preexisting differences between groups," and of Meehl's statement (1970, p. 402) that a comparison made in an <u>ex post facto</u> experiment is "in most instances so radically defective in its logical structure that it is in principle incapable of answering the kinds of theoretical questions which typically give rise to its use."

When cases are not assigned at random to treatments, the samples receiving the treatments are best thought of as coming from separate populations (Thorndike, 1942). These multi-population studies are

Analysis of covariance -- Angel of salvation, or temptress and deluder?

Lee J. Cronbach, David R. Rogosa, Robert E. Floden and Gary G. Price

Stanford Evaluation Consortium, Stanford University*

Abstract

The estimate of a "treatment effect" reached by analysis of covariance in a nonrandomized experiment depends on the covariate chosen. An unbiased estimate of the treatment effect would often require that a different covariate be used in each treatment. A sufficient but unlikely condition for an unbiased estimate is that the covariate for each treatment is (a) the complete covariate that predicts the outcome in that treatment as fully as possible from initial characteristics of the case, or (b) the complete discriminant that fully represents differences between the group means on the initial characteristics. When the covariate for a treatment is a weighted composite of (a) and (b), the adjusted outcome mean for a treatment may have positive or negative basis, the bias being a nonlinear function of the weights. To the extent that (a) and (b) do not wholly account for the covariate, the adjustment to the outcome mean is reduced in absolute magnitude. A procedure is offered to correct for error of measurement when there are two or more fallible covariables. Specification errors as well as errors of measurement have an attenuating effect, however. Since the parameters of the specification errors are unknown, no correction procedures can be counted on to provide an unbiased estimate of the treatment effect.

* The Stanford Evaluation Consortium is supported by a grant from the Russell Sage Foundation.

called quasiexperiments in the social-science literature, observational studies in the statistical literature. For a recent review of the statistical literature on such studies, see McKinlay (1975). Because of selection or self-selection, the populations have different distributions of initial characteristics.

The most direct problem of inference is to describe the relation of outcome to initial characteristics in one population at a time. As Cronbach and Furby (1970, p. 78) suggested, the within-treatment regression equation relating outcome to true initial status[1] on relevant covariates describes the effect in a suitable way. This inference, made separately in each population, seems to present no insuperable difficulties. It is the comparison of treatments that encounters serious difficulties.

This paper concentrates on the estimate of the intercept α_A within Population A. The bias in the estimate is described as a function of the underlying parameters of a linear model for quasiexperiments. Once the bias arising from a particular covariate is characterized within each treatment, it is a simple matter to characterize the bias in the estimated difference $\hat{\alpha}_A - \hat{\alpha}_B$, arising from applying that covariate in each treatment as in ancova. It is also possible to evaluate $\hat{\alpha}_A$ and $\hat{\alpha}_B$ each with a different (optimal) covariate; the bias in this difference is another function of the same parameters. The conditions affecting bias also affect statistical efficiency, not necessarily in the same way; we do not pursue questions of efficiency here.

---

[1]"True score" is an ambiguous concept. For present purposes the reader can think in terms of classical test theory (Lord & Novick, 1968), or of theory for congeric tests (Jöreskog, 1971).

As Cochran and Rubin (1973) implied, ancova does not always underadjust. The conditions under which the treatment given the less able group will be underrated or overrated can be specified; some of these conditions are indicated by Weisberg and Bryk (1974). We shall show that an adequate adjustment or evaluation of bias requires information that is unlikely to be obtainable.

Our thinking grows out of exchanges with other investigators, and is influenced by their published papers and drafts and by their reactions to preliminary versions of our own thinking. In particular, we have received help from conversations with Robert Boruch, Donald T. Campbell, Albert Erlebacher, and their associates, with David E. Wiley and Ward Keesling, and with Janet D. Elashoff, Arthur S. Goldberger, Frederic M. Lord, and Donald B. Rubin. We have benefited also from interchanges with James W. Bush and his group studying the methodological problems as seen in research on delivery of health services. Rosedith Sitgreaves and other colleagues in the Technical Seminar of the Stanford Evaluation Consortium have made helpful comments. We hope that responses to this preliminary report will extend our list of creditors.

Definitions, notation and model

Assume that an investigation is intended to appraise the effects of Treatments A and B on outcome Y. To consider here more than two treatments, more than one outcome, etc., would obscure what is novel in our argument. To appraise the treatment is to determine the expected value of Y in that treatment, for cases whose initial characteristics are known.

Notation and sampling assumptions

We speak of Populations A and B, and of their union Population G, the grand population. This two-population model reduces to the one-population model, the true experiment, when corresponding parameters of the two populations are equal.

Population membership reflects eligibility rules, readiness to volunteer, etc. In any subgroup of G having uniform initial characteristics, one can specify the probability of belonging to Population A conditional on initial characteristics, and its complement the probability of belonging to B. Integrating these over the density distribution of initial characteristics in Population G, we arrive at unconditional probabilities: A member of Population G falls into Population A with probability π_A. It is convenient to write κ for $\pi_A/\pi_B = \pi_A/(1-\pi_A)$. When an after-school science program attracts only a select group of students, π_A is perhaps 0.05 and κ is 0.05/0.95=0.053.

As Weisberg and Bryk emphasized, variables may correlate differently in Populations A, B, and G. Score composites that are uncorrelated in A (or B, or G) may be correlated in the other two populations. Geometric intuitions about dimensions, angular separations, etc. cannot be trusted, unless one is careful to specify and keep in mind one particular population.

Formally, it is assumed here that cases have been drawn at random from Population A to receive Treatment A, etc. Cases may usually be thought of as persons. An investigator who applies a treatment to groups (e.g., to schools, classes, or communities) should take this larger aggregation as the case, unless he is prepared to assume that persons or other units within an aggregation are treated independently.

Every case p is described at the outset of the experiment by a vector of true scores $\xi_{pi}(i = 1, 2, \ldots)$. A ξ_i may be combined with an error variable ε to generate a X_i. Values of X_j are random, not fixed. W will represent any variable (any X or ξ or combination of them) chosen as a covariate. To distinguish variables we write W_1 and W_2 or ξ_i and ξ_j, etc. ξ_{W_1} will refer to the true score corresponding to W_1, etc.

We use subscripts where necessary to identify the distribution considered. E.g., $\mu_{X_1(A)}$ and $\overline{X}_{1(A)}$ are the means of X_1 in Population A and in a sample from Population A, respectively. The code is as follows:

(A), (B)	Calculated within Treatments A and B, respectively.
(T)	Calculated within Treatment T, where T may be A or B.
(A,B)	Calculated from the pooled distribution of scores expressed as deviations from the respective treatment means. Population A,B is an artificial, but statistically valid, population. In Sample A,B the numbers of cases in A must be κ times the number in B or else weighting must be used.
(G)	Calculated on the grand population or on a pooled sample with A and B cases in the ratio, κ:1.

We make an important scaling assumption, without loss of generality: for any ξ_i, $\mu_{\xi_i(G)} = 0$. Also, $\mu_{\varepsilon(G)} = 0$, hence $\mu_{X_i(G)} = 0$.

Consequently, $\mu_{X_i(B)} = -\kappa\mu_{X_i(A)}$.

For any ε and T, it is assumed that $\mu_{\varepsilon(T)} = 0$. This denies that fallible information (X_i as distinct from the corresponding ξ_i) influences which treatment a person gets. (But see Note 4, p. 17). It will <u>not</u> be assumed that, in general, $\sigma_{\xi_i(A)} = \sigma_{\xi_i(B)}$ or $\sigma_{\xi_i\xi_j(A)} = \sigma_{\xi_i\xi_j(B)}$.

Indeed, all homogeneity assumptions will be avoided.

or observable in practice) are included in the set of ξ_i; any combination of the ξ_i is itself a ξ_i. The set is to be thought of as indefinitely large. An investigator would have exhaustive information on case p if he had a set of scores for p on all the ξ_i.

A formal restriction is needed if the matrix of $\rho_{\xi_i \xi_j (T)}$ is not of full rank. Components of the ξ_i, orthogonal within T, can be identified, and any ξ_i can be expressed as a weighted sum of these components, the weights possibly differing with the treatment. If any linear combination of ξ_j has a vector of weights in T that are proportional to the weights of ξ_i, we assume it to have the same absolute coefficient of variation ($|\sigma/\mu|$) as ξ_i. (If the ratio of the vectors is negative then the coefficients of variation are opposite in sign.) This requirement is imposed in Population A and Population B, separately. Although hypothetical cases can be constructed in which the assumption is violated, it appears impossible for a violation to arise in practice.

ξ^* is defined as the ξ_i that best accounts for the pooled within-treatments variance, and ξ_T^* is defined as the ξ_i that best predicts Y within Population T. When the chosen covariate W is the best predictor of Y in Population A,B--that is, when ξ^* is chosen as the covariate--

The investigator cannot be sure of the direction of bias in his $\hat{\alpha}_{AW}$. Even if (hypothetically) he knows that his covariate lies in Ω, the investigator who selects a single arbitrary W_Ω has no knowledge of ϕ

the residual variance $\sigma^2_{Y \cdot W(A,B)}$ is minimized. To complete the definitions: $\rho_{Y\xi^*(A,B)} \geq 0$ and $\rho_{Y\xi^*_T(T)} \geq 0$. Unless ξ^* and ξ^*_T are the same, $\rho_{Y\xi^*_T(T)} > \rho_{Y\xi^*(T)}$. It should be noted that $\rho_{Y\xi^*(T)}$ can be negative in one treatment. $\rho_{\xi^*_A\xi^*_B(T)}$ may be negative also (for example, if one treatment capitalizes on extroversion and one on introversion).

Arbitrarily[2] we designate the treatment for which $\mu_{\xi^*(T)} > 0$ as Treatment A. Defining A on the basis of ξ^* makes A "the more able group," insofar as that label can be given a priori meaning. The investigator who has less than exhaustive information on his sample is not able to identify ξ^*, hence he cannot directly identify which treatment is A.

He will presumably think of the group whose sample mean on W is higher as "the more able group", if the sign of $\rho_{YW(T)}$ is positive in both treatments. Where $\rho_{YW(T)}$ does have the same sign in both treatments, we polarize W to make the sign positive. Where the $\rho_{YW(T)}$ are opposite in sign, we polarize W to make the sign positive in A. The treatment for which $\mu_{W(T)} > 0$ is designated T_W.

Model accounting for Y

ζ is the error of observing Y. υ is a disturbance taking into account any unpredictable event that affects the true score on Y; it includes variations in the delivery of the treatment, and events such as illness that could not be predicted from initial status. $\mu_{\upsilon(T)} = \mu_{\zeta(T)} = 0$.

We assume ξ^*_T, υ_T, and ζ linearly independent. For case p in Population T,

$$Y_{Tp} = \alpha_T + \beta_{Y\xi^*_T(T)}\, \xi^*_{Tp} + \upsilon_{Tp} + \zeta_p \quad (1)$$

[2] If $\mu_{\xi^*(T)} = 0$, the assignment of the label A is a matter of indifference. There will be other statements where at a boundary a description or polarization is equivocal. We do not comment on other special cases of this kind.

Having set $\mu_{Y(G)}$ at zero, we can identify α_T as the expected value of Y for cases in Population T whose $\xi^*_{Tp} = \mu_{\xi^*_T(G)}$. When $\rho_{\xi^*_A\xi^*(A)} = \rho_{\xi^*_B\xi^*(B)} = 1$ and $\beta_{Y\xi^*(A)} = \beta_{Y\xi^*(B)}$, $\alpha_A - \alpha_B$ describes the treatment effect at every value of ξ^*.

Interest in the α_T is diminished when the regressions of Y on any ξ_i are heterogeneous (as will occur when any of the three conditions just stated is violated). That parameter is emphasized in this paper, however, because it has been the focus in previous uses of ancova. Even with heterogeneous regressions, α_T is relevant for decisions where one proposes to apply the same treatment to all cases in the future. Suppose an investigator comparing Textbooks A and B believes that $\rho_{\xi^*_A\xi^*_B} = 1$, $\beta_{Y\xi^*(A)} > \beta_{Y\xi^*(B)}$, and $\alpha_A > \alpha_B$. A disordinal interaction of this kind might suggest assigning students above a certain level of ξ^* to A, and the others to B. The investigator may, however, recommend uniform adoption of Textbook A hereafter. Such a policy would be reasonable whenever the costs of keeping two treatments in place outweigh the benefits from differentiated assignment.

The complete discriminant ξ^o

In general, Populations A and B will be differentiated in several respects. We introduce the construct of a "complete discriminant" (which will coincide with the linear discriminant function under restrictive assumptions). Consider a set of orthogonal components Z_1, Z_2 . . . derived from the matrix of $\rho_{\xi_i\xi_j(G)}$.

The complete discriminant ξ^o is defined so that in either T, for all k, ℓ pairs, $\rho_{Z_k\xi^o(G)} : \rho_{Z_\ell\xi^o(G)} = \mu_{Z_k(T)} : \mu_{Z_\ell(T)}$. ξ^o is polarized to make $\mu_{\xi^o(A)} = -\mu_{\xi^o(B)} > 0$.

Group membership depends on the characteristics of the case that influence the decision to enter and remain in a treatment and/or the characteristics considered in assignment to treatments. (An error ε can function as a discriminant; e.g., a student who by chance earns a higher SAT score than his true score may as a consequence apply to and be accepted by a more selective college. To make our argument as direct as possible we have ruled this out by specifying $\mu_{\varepsilon(T)} = 0$, hence $\rho_{\xi^o \varepsilon(G)} = 0$ for all ε. But see Note 4, p. 17.)

Adjusted outcome

By "adjusted outcome" we mean $Y - \beta_{YW(T)}W$. If ξ_T^* is the chosen covariate, we have the ideally adjusted outcome:

(2) $$Y_{Tp} \cdot \xi^*_{Tp} = Y_{Tp} - \beta_{Y\xi^*_T(T)}\xi^*_{Tp} = \alpha_T + \upsilon_{Tp} + \zeta_p \, .$$

Since the expected values of υ and ζ over persons within T are zero, the mean of this adjusted outcome within Sample T is an unbiased estimate of α_T. The central question in this paper has to do with the bias in $\hat{\alpha}_T$ when the chosen covariate W is not ξ^*_T.

Traditional formulations and procedures in the light of this model

The constructs represented by ξ^*_T and ξ^o do not appear explicitly in the literature on ancova. The usual model is expressed in terms of a single covariate, more or less as follows:

(3) $$Y_{Tp} = \alpha_T + \beta_{YW(A,B)}W_{Tp} + \upsilon_{Tp} + \zeta_p \, .$$

Homogeneity of regressions for the particular W -- $\beta_{YW(A)} = \beta_{YW(B)} = \beta_{YW(A,B)}$ -- is explicitly assumed. To apply ancova, an investigator collects data on one or more X_i that he considers relevant. If he observes more than one X he forms the composite W that maximizes $r^2_{YW(A,B)}$. Otherwise, his arbitrarily chosen X_i becomes W.

Using a sample estimate of $\beta_{YW(A,B)}$, the investigator can estimate α_A and α_B or he can estimate $\alpha_A - \alpha_B$ in one step. He usually tests the null hypothesis $\alpha_A - \alpha_B = 0$, assuming that values of W are fixed. Taking W as random is appropriate in a two-population experiment; but statistical theory for random covariates is limited.

The traditional assumption of homogeneous regressions can be weakened by adding a term in $W_{Tp} \times T$ to (3), T being a dummy variable. The analysis proceeds much as before (Atiqullah, 1964; Cramer, 1972; Goldberger, 1972b), the treatment effect being described as the difference between the adjusted treatment means evaluated at the grand mean on X .

Since the 1950s, statisticians have been conscious that error of measurement makes the usual estimate of $\alpha_A - \alpha_B$ inadequate. Lord (1960) developed a procedure for use in a two-population experiment when two fallible, equivalent measures W and W' have been made, the corresponding ξ_W being fixed. Lord estimates what $\alpha_A - \alpha_B$ would be if ξ_W were known and chosen as the covariate. Lord's formulation and the work of Porter (1967) and DeGracie (1968) seem implicitly to assume that one variable is the best predictor of Y in Populations A, B, and A,B, and that this variable has been chosen as the covariate. Unpublished notes by various authors have touched on the problem of correcting for error of measurement when the X_i represent two or more ξ_i . At best, a solution along the lines of those notes must assume that these ξ_i can be combined into a variable ξ_W such that $\rho_{\xi_A^* \xi_W (A)} = \rho_{\xi_B^* \xi_W (B)} = 1$. Some theoreticians have been aware that an error-free but incomplete covariate adjusts the treatment effect inadequately, the most complete discussions being those of Cochran and Rubin (1973) and Weisberg and Bryk (1974).

Although we have not seen any formal use of the construct ξ^* , the concept of ξ^* appears in considerations of model specification (e.g., DeGracie and Fuller, 1972, p. 930). The literature on structural models of social and economic processes refers to a model as being completely specified if all causal influences are included. That is, model specification is complete if a set of ξ_i included in the model can be combined to evaluate ξ_T^* .

A person who (hypothetically) had ideal information in a two-population or a one-population experiment should compute $\beta_{Y\xi_A^*(A)}$ and $\beta_{Y\xi_B^*(B)}$. Entering these in (2) and averaging over cases would lead to unbiased estimates of α_A and α_B. This procedure makes no assumption of homogeneity, and it does not require using the same covariate in each treatment.

Notes on heterogeneity of regression

Before turning to the analysis of bias, we comment on aspects of the homogeneity assumption.

(1) The investigator whose data fail to reject the null hypothesis $\beta_{YW(A)} - \beta_{YW(B)} = 0$ cannot confidently conclude that the regressions are homogeneous. With samples of the usual size, the test on regression slopes is not powerful enough to detect important differences (Cronbach and Snow, in press, Chapter 4). Having regarded the regressions onto W as homogeneous, the investigator usually interprets $\alpha_A - \alpha_B$ as describing the expected effect in Population G for any value of other initial characteristics--but this may not be valid even when $\beta_{YW(A)} = \beta_{YW(B)}$.

(2) It does not appear likely that the same variable will be the best predictor of outcome in both populations, as different treatments can be expected to call upon aptitudes in different proportions. Unless both the $\rho_{\xi^*_A \xi^*_B (T)}$ are 1, the regressions of Y onto not more than one linear combination of ξ^*_A and ξ^*_B are homogeneous. If both $\rho_{\xi^*_A \xi^*_B (T)} = 1$, the regressions may or may not be homogeneous. A similar statement can be made about observed scores. Hence it is a priori unlikely that whatever W the investigator chooses will yield homogeneous regressions.

(3) Procedures suggested for taking error of measurement into account have assumed that W has the same reliability in each treatment. This seems not to be an appropriate assumption in a two-population experiment, since $\sigma^2_{\xi_W (A)}$ usually will not equal $\sigma^2_{\xi_W (B)}$. Unless one does assume equal reliabilities, homogeneous regressions onto W imply nonhomogeneous regressions onto ξ_W.

$\underline{\alpha}_T$ estimated with an arbitrary, wholly relevant covariate

Assumptions and dimensionalization

In this section we confine attention to relationships in Population A (not the sample), and to a covariate that within A is a linear function of ξ_A^* and ξ^o. The initial argument will be developed under the following assumptions:

1. $0 < \rho_{\xi^o \xi_A^*(A)} < 1$.

2. Dimensions I and II define the plane Ω_A, in which ξ_A^* and ξ^o lie. $\rho_{I\xi_A^*(A)} = 1$, $\rho_{I\ II(A)} = 0$, and $R^2_{\xi^o \cdot I\ II(A)} = 1$.

3. $\mu_{II(A)} > 0$. (From Assumption 1, $\mu_{I(A)} > 0$.)

4. $\sigma^2_{I(A)} = \sigma^2_{II(A)} = 1$.

5. The covariate W_{Ω_A} is an arbitrary linear combination of ξ_A^* and ξ^o ; i.e., W_{Ω_A} lies in Ω_A .

Consistent with a polarization rule made earlier, $\rho_{\xi_A^* W_{\Omega_A}(A)} > 0$. We now set out to relate the bias in $\hat{\alpha}_A$ to the following set of parameters (all within Population A): μ_I , $\rho_{Y\xi_A^*}$, σ_Y^2 , $\mu_I^2 + \mu_{II}^2$, and $\rho_{\xi_A^* W_{\Omega_A}}$. Note that $\mu_I = \mu_{\xi_A^*}/\sigma_{\xi_A^*}$. Figure 1 displays the variables. Because I is at the top center of Figure 1 to conform to the scheme of

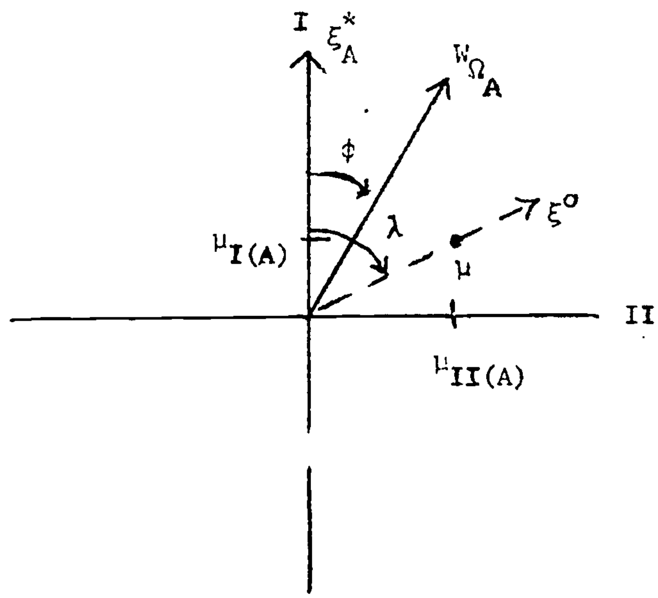

Figure 1. A configuration of variables in plane Ω_A .

later figures, we measure angles in the clockwise direction (contrary to convention). The coordinates of the centroid are $\{\mu_{I(A)} ; \mu_{II(A)}\}$. Assumptions 1 and 3 locate the centroid in Quadrant 1 of the plane. We denote the angle from ξ_A^* to ξ^o by λ ; $0^o < \lambda < 90^o$. (The earlier assumption that $\mu_{\xi^*(A)} > 0$ will restrict λ within this range when $\rho_{\xi_A^* \xi_B^*(A)} << 0$ and $\pi_A << \pi_B$.) The angle ϕ between W_{Ω_A} and ξ_A^* may range between -90^o and $+90^o$; $\cos\phi = \rho_{\xi_A^* W_{\Omega_A}(A)}$. Hereafter we omit subscripts (A), as all parameters are defined within Population A. We write β_A for $\beta_{Y\xi_A^*(A)}$, and we simplify Ω_A to Ω . The covariances of the variables with Dimensions I and II are given in Table 1.

TABLE 1

Covariances of Variables with the Dimensions

	I	II	σ^2
ξ_A^*	$\sigma_{\xi_A^*}$	0	$\sigma^2_{\xi_A^*}$
ξ^o	$\sigma_{\xi^o} \dfrac{\mu_I}{\sqrt{\mu_I^2 + \mu_{II}^2}}$ $= \sigma_{\xi^o}\cos\lambda$	$\sigma_{\xi^o} \dfrac{\mu_{II}}{\sqrt{\mu_I^2 + \mu_{II}^2}}$ $= \sigma_{\xi^o}\sin\lambda$	$\sigma^2_{\xi^o}$
W_Ω	$\sigma_{W_\Omega}\cos\phi$	$\sigma_{W_\Omega}\sin\phi$	$\sigma^2_{W_\Omega}$

Analysis

(4) $$W_\Omega = (\cos\phi)\,\sigma_{W_\Omega}\,\mathrm{I} + (\sin\phi)\,\sigma_{W_\Omega}\,\mathrm{II}\ .$$

From (1),

(5) $$Y = \alpha_A + \beta_A\,\sigma_{\xi^*_A}\,\mathrm{I} + \upsilon_A + \zeta\ .$$

(6) $$\mu_{W_\Omega} = (\cos\phi)\,\sigma_{W_\Omega}\mu_{\mathrm{I}} + (\sin\phi)\sigma_{W_\Omega}\,\mu_{\mathrm{II}}$$

and

(7) $$\mu_Y = \alpha_A + \beta_A\,\sigma_{\xi^*_A}\,\mu_{\mathrm{I}}\ .$$

The regression equation for predicting Y can be written as

(8) $$E(Y|W_\Omega) = \mu_Y + (\cos\phi)\,\beta_A\sigma_{\xi^*_A}\,(W_\Omega - \mu_{W_\Omega})/\sigma_{W_\Omega}\ .$$

Except when $\cos\phi = 1$, the regression slope in (8) is smaller than the slope in (5).

Equation (8) is not biased, as a description of the relation of outcome to W_Ω. Bias enters when $\hat{\alpha}_{AW_\Omega}$, estimated by entering $W_\Omega = 0$ in (8), is taken as a measure of α_A.

Bias as a function of ϕ

The choice of covariate has a critical influence on the estimate of the treatment effect. Inserting $W_\Omega = 0$ into (8),

(9) $$\hat{\alpha}_{AW_\Omega} = \mu_Y - (\cos\phi)\,\beta_A\sigma_{\xi^*_A}\mu_{W_\Omega} / \sigma_{W_\Omega}\ .$$

The last term in (9) is the "adjustment" that would be made to $\mu_{Y(A)}$ with W_Ω as the covariate in ancova. According to (7), $\beta_A \sigma_{\xi_A^*} \mu_I$ is the ideal adjustment that changes $\mu_{Y(A)}$ to α_A.

Substituting from (6),

$$(10) \qquad \hat{\alpha}_{AW_\Omega} = \mu_Y - (\cos\phi)\, \beta_A \sigma_{\xi_A^*} \left[(\cos\phi)\, \mu_I + (\sin\phi)\, \mu_{II}\right] .$$

The bias $\Delta_{AW_\Omega} = \hat{\alpha}_{AW_\Omega} - \alpha_A$. Because the centroid lies on ξ^o, $\mu_I : \mu_{II} = \cos\lambda : \sin\lambda$. Using this fact we can reach

$$(11) \qquad \Delta_{AW_\Omega} = \beta_A \sigma_{\xi_A^*} \left[\cos\lambda - \cos\phi \cos(\lambda - \phi)\right] \left[\mu_I^2 + \mu_{II}^2\right]^{\frac{1}{2}} .$$

This equation is one of the three main results in this paper.

$\Delta = 0$ when $\phi = 0$ or $\phi = \lambda$. I.e., if ξ_A^* is used as the covariate, bias in $\hat{\alpha}_{AW}$ is eliminated. Also if the complete discriminant ξ^o is used as the covariate, bias in $\hat{\alpha}_{AW}$ is eliminated.[3] So far as we know, this latter observation has not been made previously.[4]

Figure 2 plots $\cos\phi$ and μ_{W_Ω} as a function of ϕ and shows how the

---

[3]Indeed, if an incomplete discriminant that satisfies $\rho_{\xi_A^* W \cdot \xi^o (T)} = 0$ is used as the covariate, there is zero bias in $\hat{\alpha}_{AW}$. In our mathematical development such instances do not appear explicitly; they enter as instances where an attenuation effect exactly offsets an overestimate.

[4]Weisberg and Bryk, who formulate the problem differently and have no variable in their model that maps into ξ^o, nonetheless arrive at a somewhat similar finding. Likewise, Arthur Goldberger (1972a,b and personal communication) has established that if one forms the groups by explicitly dividing Population G on a certain variable -- which then becomes ξ^o -- taking that variable as a covariate estimates the treatment effect without bias. In the literature on educational and psychological field studies,

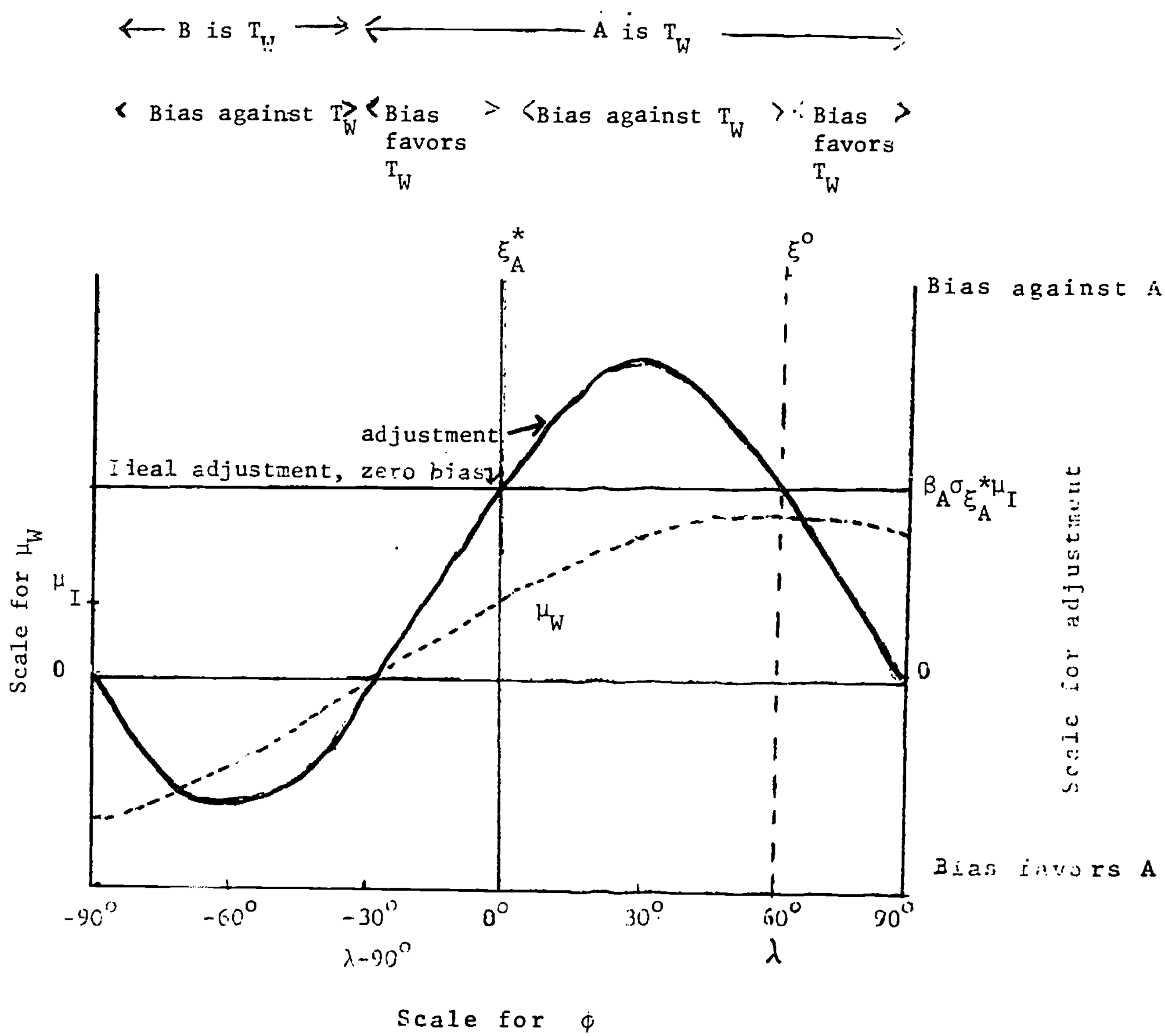

Figure 2. Adjustment to be subtracted from mean outcome for Treatment A as a function of $\phi_{W_\Omega}(A)$, where the covariate W lies in Ω.

adjustment and the bias vary. In the region $0^\circ < \phi < \lambda$, the bias is negative; that is to say, ancova overadjusts and $\mu_{Y(A)}$ and Treatment A is made to look less effective for persons at the grand mean than it is. In the regions $-90^\circ < \phi < 0^\circ$ and $\lambda < \phi < 90^\circ$, the bias makes Treatment A look better than it is. If $-90^\circ < \phi < \lambda - 90^\circ$, the estimate of α_A is greater (more seriously biased in favor of A) than when no covariance adjustment is made; that is, $\hat{\alpha}_A > \mu_{Y(A)}$. Conclusions about the net bias in $\hat{\alpha}_A - \hat{\alpha}_B$ will be derived from this information in a later section.

The shape of Figure 2 changes with λ . When $\lambda = 0^\circ$ the adjustment is always nonnegative and the adjustment curve is symmetric about $\phi = 0$. When $\lambda = 90^\circ$ the ideal adjustment is zero and the adjustment curve encloses equal areas above and below the zero adjustment axis. For any λ the maximum of the adjustment occurs at $\phi = \lambda/2$ and is proportional to $1 + \cos\lambda$. The minimum occurs at $\phi = (\lambda/2) - 90^\circ$ and is proportional to $-1 + \cos\lambda$. Figure 2 is coded at the top to show where $\mu_W > 0$ and hence where A is T_W . Also, it is coded to show where the bias makes T_W look better than it should.

---

4 (continued)
explicit assignment to one treatment of persons in a certain range of the intended covariate is known as a "regression discontinuity" design. Explicit assignment is almost invariably based on a fallible X rather than a ξ , as Goldberger comments. Goldberger's proof implicitly assumes that any loss of subjects subsequent to assignment depends only on ξ^o and/or random events.

Goldberger has pointed out (personal communication) that our argument becomes more inclusive if we allow a fallible measure to enter into the complete discriminant. We have ruled this out for ease of exposition. So long as all variables lying in Ω are free of measurement error, any measurement error can be identified with a variable III or IV (below). This permits comparison of our results with those of previous papers on disattenuation in ancova. It would be possible to restate our argument, writing X^o for the complete discriminant, bearing in mind that the mean of the measurement error in Population A is not zero. Then Ω would be defined by ξ_A^* and X^o, and various W_Ω would replace the ξ_i of our argument. (Paradoxically, if W_Ω is a variable lying in the plane defined by ξ_A^* and the fallible X^o, using the true ξ_{W_Ω} as covariate would have an attenuating effect because $R^2_{\xi_{W_\Omega} \cdot \xi_A^* X^o} < 1$.)

The investigator cannot be sure of the direction of bias in his $\hat{\alpha}_{AW}$. Even if (hypothetically) he knows that his covariate lies in Ω, the investigator who selects a single arbitrary W_{Ω} has no knowledge of ϕ beyond the fact that $0 < | \cos \phi | \leq | \rho_{YW_{\Omega}} |$. Nor does he have knowledge of λ. With <u>two</u> covariates known to lie in Ω, he would be in an ideal position. If W_1 and W_2 lie in Ω, the multiple-regression equation for predicting Y within A yields ξ_A^*, which produces a bias-free $\hat{\alpha}_A$. (See pages 27-30.)

It takes some imagination to construct a plausible substantive example of a covariate where $-90^o < \phi < \lambda - 90^o$.
For the sake of example, let Treatment A be training in assertiveness and let the dependent variable be some kind of verbal performance. Assume that verbal aptitude ξ^* is the best predictor of the dependent variable in Populations A and B as well as A,B. For purposes of the example, the discriminant ξ^o is a measure of gentleness; persons above a certain level of gentleness (opposite of assertiveness) tend to opt into the experimental treatment. Suppose also that on the average gentler persons have superior ξ^* scores. Say, $\lambda = 60^o$. Now let the investigator choose as covariate a role-playing pretest in which the person is to assert justifiable counter-arguments in response to some communication. Assume that this W lies in Ω_A. Role playing may well correlate substantially but negatively with ξ^o, and have a small positive correlation with ξ^*. Say, $\phi = -80^o$. If so, the covariance adjustment would make $\hat{\alpha}_A > \mu_{Y(A)} > \alpha_A$. That is to say, the attempt at adjustment worsens the bias.

With regard to the bias in Treatment B, one important change is required. We can distinguish ϕ_A and ϕ_B, λ_A and λ_B, Ω_A and Ω_B. ($\phi_B = \cos^{-1}\rho_{\xi^*_B W_{\Omega_B}}(B)$, $\lambda_B = \cos^{-1}\rho_{\xi^*_B \xi^o}(B)$, $\beta_B = \beta_{Y\xi^*_B}(B)$.)

$$(10') \qquad \hat{\alpha}_{BW_{\Omega_B}} = \mu_{Y(B)} - (\cos\phi_B)\,\beta_B\sigma_{\xi^*_B}\left[(\cos\phi_B)\,\mu_{I(B)} + (\sin\phi_B)\,\mu_{II(B)}\right].$$

Changes here are in notation only. In (11), however, the fact that $\mu_{I(B)}$ and $\mu_{II(B)}$ are negative requires a change of sign:

$$(11') \qquad \Delta_{BW_{\Omega_B}} = -\,\beta_B\sigma_{\xi^*_B}\left[\cos\lambda_B - \cos\phi_B\cos(\lambda_B - \phi_B)\right]\left[\mu^2_{I(B)} + \mu^2_{II(B)}\right]^{1/2}.$$

All preceding statements about the bias in $\hat{\alpha}_A$ can be suitably modified to apply to $\hat{\alpha}_B$.

Alternative assumptions

We now consider the alternative assumption $-1 < \rho_{\xi^o \xi^*_A (A)} < 0$, hence $\mu_{I(A)} < 0$. (To keep $\mu_{II(A)} > 0$ is an arbitrary polarization that does not lose generality.) Under this assumption $90^o < \lambda < 180^o$. Equations (9) to (11) remain valid. We proceed without subscripts (A).

The figure tracing the adjustment as a function of ϕ would be a transformation of Figure 2. Since $\mu_I < 0$, the ideal adjustment is negative. If $\lambda = 120^o$ instead of 60^o as in Figure 2, the new curve can be obtained by inverting the mirror image of the old one. The scale for the adjustment is inverted, with $\beta_A \sigma_{\xi^*_A} \mu_I$ now below zero. There is a small hump above zero where $\lambda - 90^o < \phi < 90^o$, and a larger half-cycle swinging below zero to the left of $\lambda - 90^o$. Again, $\hat{\alpha}_A$ is unbiased when either ξ^*_A or ξ^o is used as the covariate. (If ϕ is plotted over the range -90^o to 90^o, $-\xi^o$ is located at $\phi = \lambda - 180^o$.) The discussion on pages 17-19 is valid under the alternative assumption considered here, save that certain statements about ranges of ϕ and λ must be reworded.

Estimate of $\alpha_A - \alpha_B$ with wholly relevant covariates

This section uses the results within treatments to examine the bias in the difference $\hat{\alpha}_A - \hat{\alpha}_B$. In analysis of covariance, the same covariate is applied to each treatment. The following discussion considers first the bias resulting when a covariate is chosen separately in each treatment. This section is limited to wholly relevant covariates; that is, the covariate chosen in A satisfies $R^2_{W \cdot \xi^*_A \xi^o (A)} = 1$ and the covariate chosen in B satisfies $R^2_{W \cdot \xi^*_B \xi^o (B)} = 1$.

Separate covariates

Figure 2 depicted the bias in $\hat{\alpha}_A$ with covariate W_{Ω_A}. Precisely the same function applies to B, with ξ^*_B, W_{Ω_B}, etc. replacing their counterparts in A. In general, $\phi_A \neq \phi_B$ and $\lambda_A \neq \lambda_B$; the ideal adjustment in A equals that in B only in special cases.

The bias in $\hat{\alpha}_A - \hat{\alpha}_B$ equals $(\hat{\alpha}_A - \alpha_A) - (\hat{\alpha}_B - \alpha_B)$ or $\Delta_{AW_{\Omega_A}} - \Delta_{BW_{\Omega_B}}$ -- hereafter, $\Delta_A - \Delta_B$. If W_{Ω_A} is ξ^*_A or ξ^o <u>and</u> W_{Ω_B} is ξ^*_B or ξ^o, $\Delta_A = \Delta_B = 0$, and consequently the estimated difference has zero bias. With certain choices of covariates, $\Delta_A > \Delta_B$ so that the net bias makes A appear more advantageous then it is. The reverse occurs with other choices of covariates. We proceed to examine how the net bias relates to the parameters of our model.

Holding λ_A and λ_B fixed, the net bias (or the net adjustment) can be represented by a function of ϕ_A and ϕ_B. The contour map in Figure 3 is generated with $\lambda_A = 60^o$, $\lambda_B = 30^o$, $\kappa = 1$, and $\beta_A \sigma_{\xi^*_A(A)} = \beta_B \sigma_{\xi^*_B(B)}$. Since $\mu_{\xi^*_A(A)} = 0.5\mu_{\xi^o(A)}$ and $\mu_{\xi^*_B(B)} = 0.866\ \mu_{\xi^o(B)} = -0.866\ \mu_{\xi^o(A)}$, these parameters cause the ideal adjustment for B to be larger in absolute value than that for A. The curves in Figure 3 are contours of the net bias, $\Delta_A - \Delta_B$. Where "Bias favors A" appears, there is a hill representing a positive bias; and where "Bias favors B" appears, there is a depression representing negative bias. If the range were extended in any direction the contour map would repeat. (The net bias differs from the net adjustment by a change of signs and by addition of a constant. Note the curve labelled "zero adjustment".)

The configuration would be much the same for other values of λ_A and λ_B. In general, the bias favors A over most of the joint range of the ϕ_A and ϕ_B. As the investigator does not know the ϕ_T and $\rho_{\xi^o \xi^*_A(A)}$, however, it is impossible for him to specify the direction of the net bias arising from a particular covariate.

A single arbitrary covariate

Ordinarily the user of ancova chooses a single covariate or forms a single composite covariate. Suppose that the chosen covariate W is wholly relevant in both treatments, i.e., $R^2_{W\cdot\xi^*_A\xi^o(A)} = R^2_{W\cdot\xi^*_B\xi^o(B)} = 1$. We can simplify the results of the preceding section by placing a restriction on the relation of Ω_B to Ω_A.

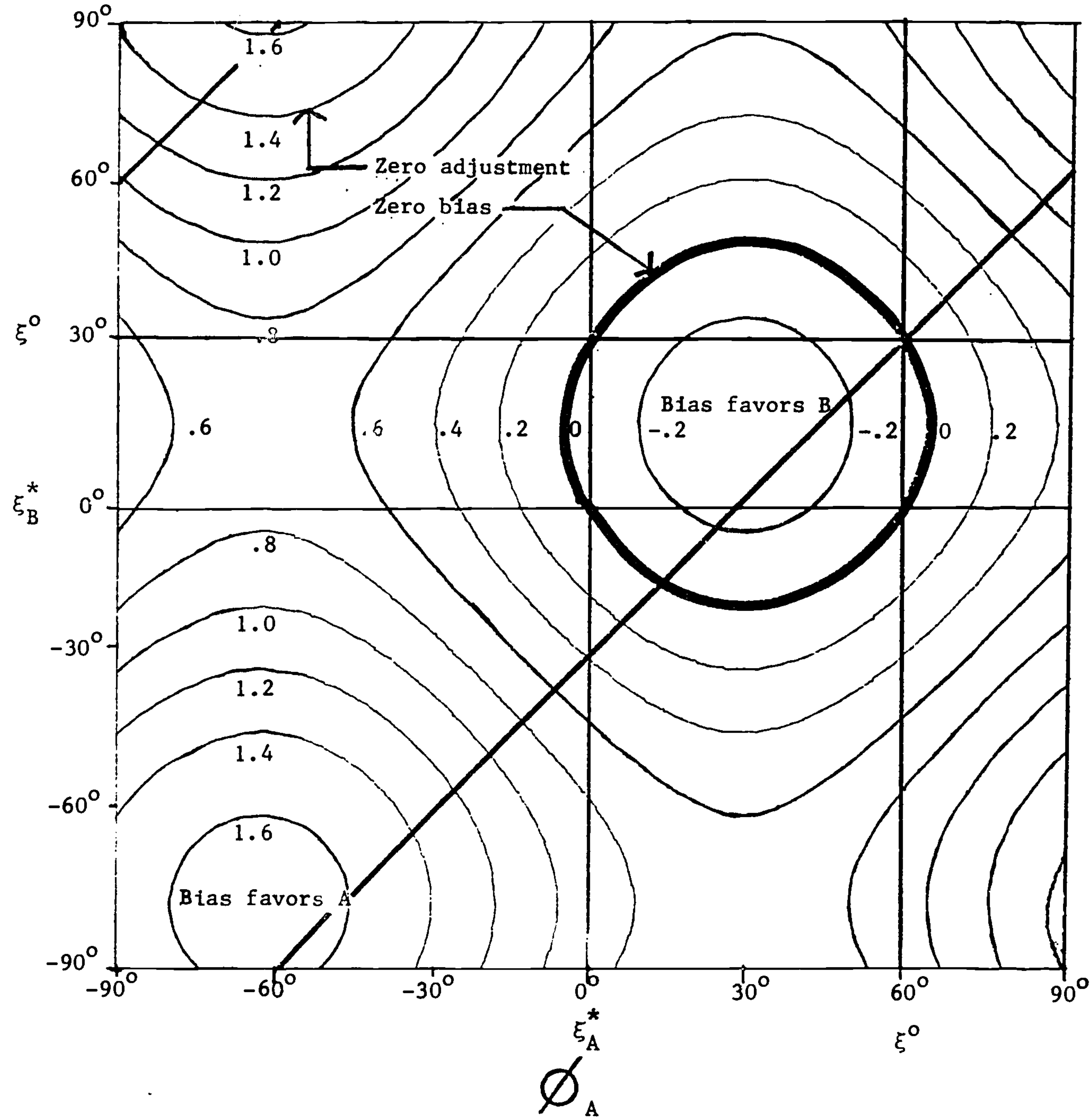

Figure 3. Net bias with a separate, wholly relevant covariate in each treatment. $\lambda_A = 60^o$, $\lambda_B = 30^o$. The diagonal line represents the coordinates for which $\phi_A - \lambda_B = \phi_B - \lambda_B$.

One such restriction is to specify $R^2_{W\cdot\xi^*_B\xi^o(A)} = R^2_{W\cdot\xi^*_A\xi^o(B)} = 1$. Then Ω_B can be mapped into Ω_A , and for any W_Ω $\phi_A - \lambda_B = \phi_B - \lambda_B$. We align the two planes to make ξ^o a common reference line. In Figure 3 a straight line identifies all points such that $\phi_A - \lambda_A = \phi_B - \lambda_B$. When a single W_Ω is used in both treatments, the bias indicated at the point on the line that corresponds to ϕ_A and ϕ_B results. In general, the bias favors A over most of the length of the line. The investigator who tries to pick a relevant covariate will try to pick one close to ξ^* or ξ^o or both; this will locate his study in the center of the chart. For the parameters in Figure 3 the resulting net bias is moderate, but may favor either A or B.

Attenuating effect of irrelevant information and error of measurement

An arbitrary covariate

Now consider a general covariate W such that $R^2_{W\cdot I\ II(A)} < 1$. Assumptions 1-4 of page 14 are retained. We define a variable III_W that is uncorrelated with I and II within A, and set $\sigma^2_{III_W(A)} = 1$. It is convenient to keep $\rho_{\xi^*_A W(A)} > 0$. In this section we again drop most subscripts (A).

The covariate W is a combination of a W_Ω and a residual identified with III_W. (Ordinarily, each W is associated with a different III_W, and any given W may be associated with one III_W in A and another in B .) This residual may represent error of measurement, or it may represent true information that neither predicts Y within A nor discriminates between groups. In place of (4) we have

$$(12) \qquad W = (\cos\phi)\,\sigma_{W_\Omega} I + (\sin\phi)\sigma_{W_\Omega} II + h\ III_W = W_\Omega + h\ III_W .$$

Thus $\sigma^2_W > \sigma^2_{W_\Omega}$. Since III_W is a residual, $\mu_{III_W(A)}$ is zero, and $\mu_W = \mu_{W_\Omega}$ is the same as in (6).

Now

$$(13) \qquad \mu_W = (\cos\phi)\sigma_{W_\Omega}\mu_I + (\sin\phi)\sigma_{W_\Omega}\mu_{II} .$$

We write $\rho^2_{W(A)}$ for $R^2_{W\cdot I\ II(A)} = \sigma^2_{W_\Omega}/\sigma^2_W$. It should be noted that

$\rho^2_{W(A)}$ is not a reliability coefficient unless $R^2_{\xi_W \cdot I\ II} = 1$; that is, unless the true score ξ_W lies in Ω_A. We do not assume $\rho^2_{W(A)} = \rho^2_{W(B)}$.

From (5) and (12), $\sigma_{YW} = \cos\phi\ \beta_A \sigma_{\xi^*_A}\ \sigma_{W_\Omega}$. Then $\beta_{YW} = \sigma_{YW} / \sigma^2_W =$ $(\cos\phi)\beta_A \sigma_{\xi^*_A} \sigma_{W_\Omega} / \sigma^2_W$, and the adjustment term $\beta_{YW}\mu_W$ is $(\cos\phi)\ \beta_A \sigma_{\xi^*_A}\ [(\cos\ \phi)\mu_I + (\sin\phi)\ \mu_{II}]\ \rho^2_W$. Comparing this with the adjustment term in (10), we see that including irrelevant information in the covariate reduces the adjustment. The adjustment to the outcome mean in A, given by W, is $\rho^2_{W(A)}$ times that given by W_{Ω_A}. This is our second main result.

Figure 4 plots the adjustment, tracing the attenuating effect for specified values of $\rho^2_{W(A)}$. Ordinarily, however, when one changes the balance of I and II within W, a change in $\rho^2_{W(A)}$ results, as will be seen below. Unless $\rho^2_{W(A)}$ is high, ancova with <u>any</u> W will underadjust $(\hat{\alpha}_A > \alpha_A)$ provided that $\rho_{\xi^*_A \xi^o(A)}$ is positive. But when this correlation is negative, the opposite is true $(\hat{\alpha}_A < \alpha_A)$.

The adjustment described above generalizes a result in classical test theory: If ξ_X is the true score corresponding to X, $\beta_{YX} = \rho^2_{\xi_X X}\beta_{Y\xi_X}$. This equation describes the attenuating effect of error of measurement on a regression coefficient, the multiplier on the right being the reliability coefficient. Several writers, starting with Lord (1960), set out to correct the covariance adjustment for error of measurement, retaining the assumption $\beta_{YW(A)} = \beta_{YW(B)}$. They applied in various ways the principle that dividing $\beta_{YW(A,B)}$ by $\rho^2_{\xi_W W(A,B)}$ gives the slope of pooled within-groups regression of Y on ξ_W. This is true

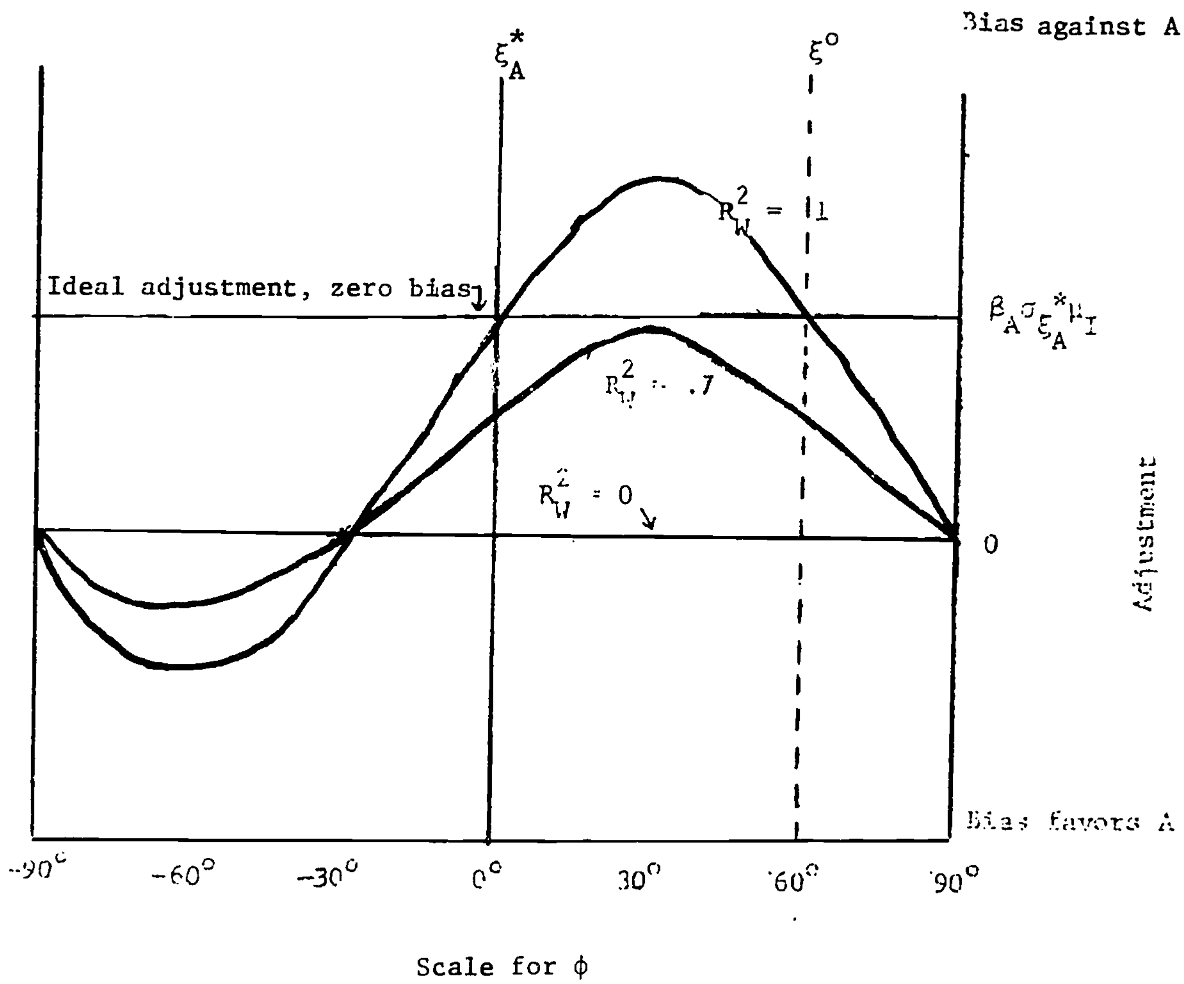

Figure 4. Adjustment as a function of R_W^2 and ϕ, where W is an arbitrary covariate.

only if within-group reliabilities are equal, which is not necessarily the case with nonrandom assignment.

We now find that irrelevant information (i.e., reflecting dimensions other than I and II) has the same effect as measurement error. Since $\rho^2_W \leq \rho^2_{\xi_W W}$, dividing β_{YW} by the reliability coefficient is not an adequate correction when irrelevant information enters W.[5]

Bias in $\hat{\alpha}_A - \hat{\alpha}_B$. To consider the net adjustment a new figure similar to Figure 3 could be produced for any specified values of $\rho^2_{W(A)}$ and $\rho^2_{W(B)}$. Figure 3 itself can be reinterpreted when $\kappa = 1$ and $\rho^2_{W(A)} = \rho^2_{W(B)}$. The curve labelled "zero adjustment" retains that label. In the region enclosed by that curve the net bias is reduced in absolute value. The large positive adjustments are reduced, and if $\rho^2_{W(T)}$ is sufficiently low there is no longer a region where bias favors B. This indicates a set of conditions making the Campbell-Erlebacher generalization valid (if A , not T_W is considered to be the treatment given the "abler group").

---

[5] Campbell and Erlebacher suggested using ρ^2_{YW} as a divisor. A memorandum circulated privately by Campbell in 1972 withdrew the proposal, but a similar idea appears with a new rationale in Campbell and Boruch (1975). Since $\rho^2_{YW(T)} < \rho^2_{\xi^*_T W(T)} < \rho^2_{W(T)}$, using ρ^2_{YW} as divisor overcorrects. As can be seen by considering the possibility that ξ^o is the chosen covariate, such a correction would sometimes make matters worse.

Two arbitrary pretest measures

We noted earlier that when two W_{Ω_T} have been observed, it is possible to combine them into a covariate proportional to ξ_T^* which provides a bias-free adjustment of $\mu_{Y(T)}$. With measures that lie outside Ω_T, the relationships are less simple.

When two or more X_i have been observed, the usual procedure in analysis of covariance is to form the composite that best predicts Y within treatments pooled. To analyze within treatments, an analog would be to form the composites $W_{Y(A)}$ and $W_{Y(B)}$ that best predict Y within the respective treatments. Even if $\xi_{W_{Y(T)}}$ were to lie in Ω_T, however, it would be unlikely that $\rho_{\xi_T^* \xi_{W_{Y(T)}}}(T) = 1$ or that $\rho_{\xi^o \xi_{W_{Y(T)}}} \cdot (T) = 1$.

Thus the true score on whatever combination of observed scores best predicts Y gives an $\hat{\alpha}_T$ with an unknown bias. Disattenuation of the regression coefficient in T by the reliability coefficient $\rho^2_{\xi_{W_{Y(T)}} W_{Y(T)}}$ or by $\rho^2_{W_{Y(T)}}$ would not correct adequately.

The ideal adjustment could be determined if certain quantities were known. Assume that W_1 and W_2 have been observed, and write III and IV to replace the III_W. Again, consider relations within A only.

(14)
$$W_1 = (\cos \phi_1)\, \sigma_{W_{1\Omega}}\, I + (\sin \phi_1)\, \sigma_{W_{1\Omega}}\, II + h_1 III$$

$$W_2 = (\cos \phi_2)\, \sigma_{W_{2\Omega}}\, I + (\sin \phi_2)\, \sigma_{W_{2\Omega}}\, II + h_2 III + g_2 IV .$$

Here
$$\phi_1 = \cos^{-1} \rho_{W_{1\Omega} \xi_A^*},\ \phi_2 = \cos^{-1} \rho_{W_{2\Omega} \xi_A^*} ,$$

$$\sigma_I^2 = \ldots \sigma_{IV}^2 = 1 \text{ and } \mu_{III} = \mu_{IV} = 0.$$

If errors are independent, $h_2 = 0$. But errors of observation can be correlated. (See Cronbach et al., 1972, Chapter 9.) The covariance matrix for certain variables of interest takes the form of Table 2, which reflects the basic logic of Cronbach et al. on disattenuation when errors are correlated. In the covariance matrix we have substituted

$u_1 = (\cos \phi_1)\sigma_{W_{1\Omega}}$, $v_1 = (\sin \phi_1)\sigma_{W_{1\Omega}}$, $u_2 = (\cos \phi_2)\sigma_{W_{2\Omega}}$, $v_2 = (\sin \phi_2)\sigma_{W_{2\Omega}}$

and $q = u_1u_2 + v_1v_2$. The quantities whose sample values are directly observable are underlined in the table.

We note that

$$\rho^2_{W_1} = \frac{\sigma^2_{W_{1\Omega}}}{\sigma^2_{W_1}} = \frac{u_1^{\,2} + v_1^{\,2}}{u_1^{\,2} + v_1^{\,2} + h_1^{\,2}} , \quad \text{and} \quad \rho^2_{W_2} = \frac{\sigma^2_{W_{2\Omega}}}{\sigma^2_{W_2}} = \frac{u_2^{\,2} + v_2^{\,2}}{u_2^{\,2} + v_2^{\,2} + h_2^{\,2} + g_2^{\,2}} .$$

If $\rho^2_{W_1}$, $\rho^2_{W_2}$, and h_1h_2 were known, the investigator could estimate Y from

$$\hat{Y} = \beta_{YW_{1\Omega}\cdot W_{2\Omega}} W_{1\Omega} + \beta_{YW_{2\Omega}\cdot W_{1\Omega}} W_{2\Omega} . \tag{15}$$

Call this regression estimate W^*_A . Since W^*_A correlates perfectly with ξ^*_A , the composite serves as a complete covariate.

From Table 2,
$$\beta_{YW_{1\Omega}\cdot W_{2\Omega}} = \frac{(u_2^2 + v_2^2)\ (u_1\beta_A\sigma_{\xi^*_A}) - (u_2\beta_A\sigma_{\xi^*_A})q}{(u_1^2 + v_1^2)(u_2^2 + v_2^2) - q^2}$$

and a similar formula holds for $\beta_{YW_{2\Omega}\cdot W_{1\Omega}}$.

TABLE 2

Covariance Matrix

	W_1	W_2	$W_{1\Omega}$	$W_{2\Omega}$	Y
W_1	$\underline{u_1^2+v_1^2+h_1^2}$	$\underline{q + h_1h_2}$	$u_1^2+v_1^2$	q	$\underline{u_1\sigma_{Y\xi^*_A(A)}/\sigma_{\xi^*_A(A)}}$
W_2		$\underline{u_2^2+v_2^2+h_2^2+g_2^2}$	q	$u_2^2+v_2^2$	$\underline{u_2\sigma_{Y\xi^*_A(A)}/\sigma_{\xi^*_A(A)}}$
$W_{1\Omega}$			$u_1^2+v_1^2$	q	$u_1\sigma_{Y\xi^*_A(A)}/\sigma_{\xi^*_A(A)}$
$W_{2\Omega}$				$u_2^2+v_2^2$	$u_2\sigma_{Y\xi^*_A(A)}/\sigma_{\xi^*_A(A)}$
Y					$\underline{\sigma_Y^2}$

The ideal adjustment (see p.17) is $\beta_{Y\xi^*_A}\mu_{\xi^*_A} = \beta_{Y\xi^*_A}\mu_I\sigma_{\xi^*_A}$. Since $\mu_{III} = \mu_{IV} = 0$,

(16) $$\mu_{W^*_A} = \beta_{YW_{1\Omega}\cdot W_{2\Omega}}\mu_{W_1} + \beta_{YW_{2\Omega}\cdot W_{1\Omega}}\mu_{W_2} = \mu_1\sigma_{W^*_A} .$$

Thus,

(17) $$\mu_{\xi^*_A} = \mu_{W^*_A}\sigma_{\xi^*_A}/\sigma_{W^*_\Omega} .$$

Since $\rho_{YW^*_A} = \rho_{Y\xi^*_A}$, $\beta_{Y\xi^*_A} = \beta_{YW^*_A}\dfrac{\sigma_{W^*_A}}{\sigma_{\xi^*_A}}$.

Then from (17) $\beta_{YW^*_A}\mu_{W^*_A} = \beta_{Y\xi^*_A}\mu_{\xi^*_A}$, and the ideal adjustment can be expressed in terms of the parameters in (14).

This procedure for disattenuation is our third main result. If III and IV consist of uncorrelated measurement errors only, the procedure is equivalent to that of Keesling and Wiley (1975). They assumed $h_2 = 0$ and interpreted III and IV in this way; in general we find it necessary to allow for specification errors and for correlated errors. $\rho^2_{W_1}$, $\rho^2_{W_2}$, and $h_1 h_2$ are unobservable. If reliability coefficients are used and an assumption is made about $h_1 h_2$, a partial disattenuation is achieved.

Conclusions and implications for one- and two-population experiments

In the two-population experiment three variables are critical: the complete covariates ξ^*_A and ξ^*_B and the complete discriminant ξ^o. In principle, analysis of covariance procedures give a less satisfactory description of treatment effects than determination of the regression equation within each treatment separately. Ideally one could regress the outcome in Treatment T onto ξ^*_T, getting an unbiased estimate of α_T, the expected outcome averaged over all persons in the grand population. $\hat{\alpha}_T$ is also unbiased when ξ^o is used as a predictor. Calculation of a single adjustment in the usual manner is warranted only under much stronger homogeneity assumptions than are usually recognized.

An arbitrarily chosen covariate will in general lead to a biased estimate of α_T (or $\alpha_A - \alpha_B$). Bias also results from the usual practice of combining two or more predictors with the weights that best predict the outcome within treatments pooled, and using the composite as a covariate. The adjustment for initial differences made in analysis of covariance may overadjust or it may underadjust. With some configurations of data the covariance adjustment gives an estimate more biased than the unadjusted difference in outcome means. This is "underadjustment" carried to an extreme.

As has often been noted, error in measuring the covariate reduces the absolute magnitude of the adjustments for A and B. Disattenuation on the basis of a reliability coefficient is inadequate to remove the bias. Two kinds of specification error also produce bias. When the covariate W is perfectly predicted by ξ^*_T and ξ^o

together ($R^2_{W\cdot\xi^*_T\xi^o(T)} = 1$), but not by either of them alone, the bias is a sinusoidal function of $\cos^{-1}\rho_{\xi^*_T W(T)}$. When the true score ξ_W contains a component that is not perfectly predicted by ξ^*_T and/or ξ^o, that specification error has the same adjustment-reducing effect as an error of measurement.

In the one-population ("true") experiment, adjustment is needed only to overcome differences between the A and B samples. The argument of this paper regarding the bias resulting from using a covariate other than ξ^*_T or ξ^o could be restated to apply to sample data in the one-population experiment. ξ^o would be replaced by an X^o (see footnote 4 , p. 17), and a wave function similar to Figure 2 would be found. The ideal adjustment will ordinarily be smaller than the ideal adjustment in a two-population experiment, since $\overline{\xi^o_{(T)}}$, arising only from sampling error, is likely to be small and therefore the $\overline{\xi^*_T}$ are also small.

Disattenuation in the one-population study changes the slope of the regression line, but does not change the estimated intercept. This distinctive aspect of the one-population experiment has not been explicit in previous warnings about the consequences of using a fallible covariate.

The investigator working with actual data from a two-population experiment does not know how his chosen covariate relates to ξ^*_T and ξ^o. Therefore, in interpreting an adjusted treatment mean or a within-treatment regression equation , he cannot assert that he has taken initial characteristics of the treatment population fully into account.

Nor can he state that whatever treatment difference he finds, after adjustment by analysis of covariance with or without correcting for unreliability, is biased in a known direction.

The sufficient conditions for an accurate description of the treatment effect are these: The investigator has observed at least two initial characteristics of the cases, W_1 and W_2, and an outcome Y. He also knows the proportion of variance in W_1 and in W_2 not predicted by a linear combination of the complete covariate and complete discriminant. And he knows the covariance between the unpredicted portions of the two measures.

These conditions will not be met in practice. Perhaps it will be possible to set plausible upper and lower bounds on the unobservable parameters, and from them to derive bounds on the estimated treatment effect or, better, on the regression equation relating outcome to the ideal covariate. An alternative is simply to describe the function relating outcome within a treatment to a particular set of covariates. ($\xi_{W_{Y(A)}}$, $\xi_{W_{Y(B)}}$, and the true score on the sample discriminant function would be a good choice.) Such a function for Treatment A could be interpreted as describing relations in Population A, with no implication that the result generalizes to Populations B and G. If functions for the two treatments are compared, it must be realized that specification errors have not been allowed for.

conditions. Moreover, what is planned as a one-population experiment is subject in some degree to the same difficulties of interpretation as a two-population experiment, once subjects in the initial sample withdraw or fail to provide data. Even the one-population experiment is subject to serious problems of external validity (Rubin, 1974; Cronbach, 1975), so that the inferences made from it are not necessarily more dependable than those from a two-population experiment covering a wider range of conditions.

Our final position is not so pessimistic as those of Riecken and Boruch, Lord, and Meehl. We do favor pinning down results within populations separately rather than trying to adjust initial differences away. Regression onto covariates is needed in that process, and the fact that covariates are incomplete or imprecise does not argue against their use. Comparison of populations is certain to be wanted, and the problems encountered in ancova are no more than one instance of the ubiquitous problem in social science of incomplete specification. The solution is not to abandon realistic social science but to make less presumptuous claims regarding the result.

BIBLIOGRAPHY

Atiqullah, M. The robustness of the covariance analysis of a one-way classification. Biometrika, 1964, 51, 365-372.

Campbell, D. T. & Boruch, R. F. Making the case for randomized assignment to treatments by considering the alternatives: Six ways in which quasi-experimental evaluations in compensatory education tend to underestimate effects. In C. A. Bennett and A. A. Lumsdaine (Eds.) Evaluation and experiment. New York: Academic Press, 1975, 195-285.

Campbell, D. T. & Erlebacher, A. How regression artifacts in quasi-experimental evaluations can mistakenly make compensatory education look harmful. In J. Hellmuth (Ed.), Compensatory education: A national debate. Vol. III. The disadvantaged child. New York: Brunner/Mazel, 1970, 185-210.

Cochran, W. G. Matching in analytical studies. American Journal of Public Health, 1953, Part I, 43 (6), 684-691.

Cochran, W. G. & Rubin, D. B. Controlling bias in observational studies: A review. Sankhya-B, 1973, 35, 417-446.

Cramer, E. M. Significance tests and tests of models in multiple regression. American Statistician, 1972, 26 (4), 26-30.

Cronbach, L. J. & Furby, L. How we should measure "change" -- or should we? Psychological Bulletin, 1970, 74, 68-80.

Cronbach, L. J.; Gleser, G. C.; Nanda, H. & Rajaratnam, N. The dependability of behavioral measurements: Theory of generalizability for scores and profiles. New York: Wiley, 1972.

Cronbach, L. J. & Snow, R. E. Aptitudes and instructional methods. New York: Irvington Publishers, in press.

DeGracie, J. Analysis of covariance when the concomitant variable is measured with error. Unpublished doctoral dissertation, Iowa State University, 1968.

DeGracie, J.S., and Fuller, W.A. Estimation of the slope and analysis of covariance when the concomitant variable is measured with error. Journal of the American Statistical Association, 1972, 67, 930-937.

Goldberger, A. S. Selection bias in evaluating treatment effects: Some formal illustrations. University of Wisconsin, Institute for Research on Poverty: Discussion Paper 123-72, April 1972 (a).

Goldberger, A. S. Selection bias in evaluating treatment effects: The case of interaction. University of Wisconsin, Institute for Research on Poverty: Discussion Paper 129-72, June 1972 (b).

Jöreskog, K. G. Statistical analysis of sets of congeneric tests. Psychometrika, 1971, 36, 109-133.

Keesling, J. W. & Wiley, D. E. Measurement error and the analysis of quasi-experimental data. Unpublished Draft Manuscript, UCLA, 1975.

Lord, F. M. Large-scale covariance analysis when the control variable is fallible. Journal of the American Statistical Association, 1960, 55, 307-321.

Lord, F. M. A paradox in the interpretation of group comparisons. Psychological Bulletin, 1967, 68, 304-305.

Lord, F. M. Statistical adjustments when comparing preexisting groups. Psychological Bulletin, 1969, 72, 336-337.

McKinlay, S. M. The design and analysis of the observational study -- a review. Journal of the American Statistical Association, 1975, 70, 503-520.

Meehl, P. E. Nuisance variables and the ex post facto design. In Radner & S. Winokur (Eds.), Analysis of theories and methods of physics and psychology, Vol. IV. Minnesota Studies in the Philosophy of Science. Minneapolis: University of Minnesota Press, 1970, 373-402.

Porter, A. C. The effects of using fallible variables in the analysis of covariance. Ph.D. dissertation, University of Wisconsin, 1967.

Riecken, H. W. & Boruch, R. F. (Eds.) Social experimentation, New York: Academic Press, 1974.

Rubin, D. B. Estimating causal effects of treatments in randomized and nonrandomized studies. Journal of Educational Psychology, 1974, 66, 688-701.

Rulon, P. J. Problems of regression. Harvard Educational Review, 1941, 11, 213-223.

Thorndike, R. L. Regression fallacies in the matched groups experiment. Psychometrika, 1942, 7 (2), 85-102.

Weisberg, H. I. & Bryk, A. S. On the use of the analysis of covariance with nonrandom assignment of experimental units to treatment groups. Unpublished Draft Report [Huron Institute, Cambridge, Mass.], 1974.