

## Ten Difference Score Myths

JEFFREY R. EDWARDS  
*University of North Carolina*

*Difference scores have been widely used in studies of fit, similarity, and agreement. Despite their widespread use, difference scores suffer from numerous methodological problems. These problems can be mitigated or avoided with polynomial regression analysis, and this method has become increasingly prevalent during the past decade. Unfortunately, a number of potentially damaging myths have begun to spread regarding the drawbacks of difference scores and the advantages of polynomial regression. If these myths go unchecked, difference scores and the problems they create are likely to persist in studies of fit, similarity, and agreement. This article reviews 10 difference score myths and attempts to dispel these myths, focusing on studies conducted since polynomial regression was formally introduced as an alternative to difference scores.*

For decades, difference scores have been ubiquitous in organizational behavior research (Edwards, 1994). Typically, difference scores are used to represent the congruence between two constructs, which is then treated as a concept in its own right. Difference scores are prevalent in studies of the fit between the person and job (Edwards, 1991; Spokane, Meir, & Catalano, 2000), the similarity between employee and organizational values (Chatman, 1991; Kristof, 1996), the match between employee expectations and experiences (Wanous, Poland, Premack, & Davis, 1992), and the agreement between performance ratings (Church & Waclawski, 1999; Godshalk & Sosik, 2000; London & Wohlers, 1991; Mersman & Donaldson, 2000).

Despite their widespread use, difference scores suffer from numerous methodological problems (Cronbach, 1958; Edwards, 1994; Johns, 1981; Wall & Payne, 1973). These problems can be ameliorated or avoided with polynomial regression analysis, which uses components of difference scores supplemented by higher-order terms to represent relationships of interest in congruence research. Polynomial regression retains the conceptual integrity of the components and treats difference scores as statements of hypotheses to be tested empirically. The genesis of this approach can be

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traced to Cronbach (1958), and formal treatments have been available for approximately a decade (Edwards, 1991, 1993, 1994, 1995; Edwards & Cooper, 1990; Edwards & Parry, 1993).

The passage of 10 years provides an opportune juncture to review the use of polynomial regression in congruence research. This review was prompted by an invitation to write a chapter on difference scores and polynomial regression for *Advances in Measurement and Data Analysis* (Drasgow & Schmitt, 2001). The review examined approximately 200 articles that have cited the aforementioned formal treatments of polynomial regression. Overall, these articles fell into three broad categories. One category consisted of studies that applied polynomial regression as it was intended (for examples of such studies, along with a comprehensive treatment of polynomial regression, see Edwards, 2001). A second category comprised studies that acknowledged difference scores as problematic but used them nonetheless. A third category contained articles that demonstrated basic misconceptions regarding problems with difference scores and the effectiveness of alternative analytical procedures, including polynomial regression analysis. These misunderstandings constitute a set of myths that have the potential to wreak havoc on future congruence research.

The objective of this article is to dispel 10 difference score myths evident in recent congruence research. This objective is important, given that published articles promulgating difference score myths are becoming sources for other studies, which risks propagating these myths throughout congruence research. Indeed, the citation patterns revealed by the review indicate that this propagation has already begun. If left unchecked, the spread of difference score myths is likely to lead astray otherwise well-intentioned researchers, encourage futile attempts to resurrect difference scores from the methodological dustbin, and hinder the accumulation of knowledge regarding the broad range of phenomena encompassed by congruence research.

### Difference Score Myths

The following myths are based on research published during the past decade. The first 2 myths represent misconceptions regarding problems with difference scores, the next 4 myths propose alternatives to difference scores that are themselves problematic, and the final 4 myths constitute misunderstandings or misguided criticisms of polynomial regression. These 10 myths are followed by a general myth regarding the myths themselves. For each myth, supporting arguments are summarized, and flaws in these arguments are identified.

#### Myth 1: The Problem With Difference Scores Is Low Reliability

One prevalent myth is that the only serious problem with difference scores is low internal consistency reliability. This myth is evidenced in various ways. For instance, some studies have reported reliabilities of difference scores and, if conventional reliability standards are reached (e.g., .70; Nunnally, 1978), have used difference scores without further reservation (Bauer & Green, 1996; Earley, 1994; Jehn & Chatman, 2000; Martocchio & Judge, 1995; McFarland & Ryan, 2000; Smith & Tisak, 1993). Other studies have reported null findings for difference scores and pointed to low reliability as the culprit (Cordes, Dougherty, & Blum, 1997). Still other studies have

attempted to justify the use of difference scores by arguing that concerns over their reliability have been exaggerated (Stewart, Carson, & Cardy, 1996).

The reliability of any measure, including a difference score, is ultimately an empirical matter. In some cases, difference scores may exhibit reliabilities greater than the .70 threshold suggested by Nunnally (1978). However, at issue is not merely whether difference scores are reliable in an absolute sense but also whether they are more reliable than are viable alternatives, such as their component measures analyzed jointly (Johns, 1981). When component measures are positively correlated, as in most congruence research, then the reliability of their difference is usually less than the reliability of either component measure. Moreover, adequate reliabilities do not absolve difference scores of their other methodological problems, and these problems are sufficient to proscribe the use of difference scores regardless of the reliabilities they exhibit.

### **Myth 2: Difference Scores Provide Conservative Statistical Tests**

Statistical tests based on difference scores have been framed as conservative, insinuating that any finding that emerges from the morass of problems with difference scores is truly robust. For example, some authors have argued that because difference scores tend to have low reliabilities, hypothesis tests based on difference scores are conservative (Jehn & Chatman, 2000). Other authors have acknowledged that difference scores could be discarded in favor of alternative methods, such as polynomial regression, but deem such methods unnecessary when results obtained using difference scores are consistent with hypotheses (Christiansen, Villanova, & Mikulay, 1997). In some instances, difference scores are regarded as appropriate for exploratory research (M. Lubatkin & Powell, 1998; A. H. Lubatkin, Vengroff, Ndiaye, & Veiga, 1999), implying that results found using difference scores have survived an initial screening and may merit further research using more sophisticated analytical methods.

Some problems with difference scores reduce effect sizes and therefore may produce conservative statistical tests. For example, difference scores that suffer from low reliability yield attenuated bivariate relationships with other variables. Likewise, difference scores used as independent variables impose constraints that reduce the explained variance (Edwards, 1994, 1996; Edwards & Harrison, 1993). However, difference scores are also likely to invite conclusions that signify false positives, such that statistical tests effectively become liberal. For instance, studies that operationalize met expectations as the difference between expectations and experiences often report a positive correlation between this difference and satisfaction as support for the met expectations hypothesis (Wanous et al., 1992). However, studies using polynomial regression have found that satisfaction is related to experiences without regard to expectations (Hom, Griffeth, Palich, & Bracker, 1999; Irving & Meyer, 1994). Thus, using difference scores to represent met expectations has produced liberal conclusions that have not survived the closer scrutiny afforded by polynomial regression. Likewise, studies of person-environment fit have used correlations between difference scores and outcomes as evidence that fit is beneficial and misfit is harmful, regardless of the direction of misfit (Edwards, 1991; Spokane et al., 2000). This interpretation is rarely supported by methods that test the assumptions embedded in difference scores

(Edwards, 1996; Edwards & Harrison, 1993). Finally, although conservative statistical procedures are often portrayed as virtuous, such conservatism usually corresponds to effect sizes that are biased downward and Type I error rates that are minimized at the expense of Type II error. Assuming the goal of empirical research is to draw accurate conclusions regarding population parameters, researchers should seek statistical procedures that are neither too liberal nor too conservative. Unfortunately, difference scores increase errors of both types.

### **Myth 3: Measures That Elicit Direct Comparisons Avoid Problems With Difference Scores**

Some researchers have attempted to circumvent problems with difference scores by using measures that elicit direct comparisons of two components. For instance, some studies of the effects of discrepancies between actual and desired job attributes have asked respondents to directly report the degree to which actual amounts deviate from desired amounts (e.g., Lance, Mallard, & Michalos, 1995; Mallard, Lance, & Michalos, 1997) or desired amounts deviate from actual amounts (e.g., Rice, Peirce, Moyer, & McFarlin, 1991). Likewise, studies of met expectations have directly assessed the degree to which job experiences exceed or fall short of expectations (Ashforth & Saks, 2000). Similar measures have been used in studies of person-organization fit (Saks & Ashforth, 1997) and psychological contract fulfillment (Coyle-Shapiro & Kessler, 2000; Robinson & Morrison, 2000). The use of such measures is often rationalized by citing Johns (1981), who stated that “if respondents can describe existing organizational conditions and preferred organizational conditions, they can surely report directly whatever it is we think we measure when we calculate the difference between these descriptions” (p. 459).

Measures that elicit direct comparisons merely shift the onus of creating a difference score from the researcher to the respondent. Some direct measures use response scales ranging from negative to positive numbers, thereby priming the respondent to mentally subtract the components on some metric and report the difference. Other direct measures ask respondents to report the degree to which the components are similar, which mimics the effect of calculating an absolute or squared difference. In each case, these direct measures prompt respondents to compare the components and explicitly or implicitly compute their difference. The resulting scores are prone to the same problems that plague difference scores because these problems do not depend on whether the respondent or the researcher calculates the difference. Moreover, an item that elicits a direct comparison is double-barreled, given that it combines two distinct concepts into a single score (DeVellis, 1991). Therefore, admonishments regarding the use of double-barreled items also pertain to direct comparison items.

To complicate problems with direct comparison measures, available evidence has suggested that respondents do not systematically combine components when reporting their difference. For example, Rice, McFarlin, and Bennett (1989) measured actual amount, wanted amount, and the perceived discrepancy (ranging from  $-2$  to  $+2$ ) between actual and wanted amounts for 13 job dimensions. If respondents mentally subtracted actual and wanted amounts to derive perceived discrepancies, then regressions of perceived discrepancies on actual and wanted amounts should yield coefficients that are equal in magnitude but opposite in sign and  $R^2$  values near unity. Regression analyses using data from Rice et al. (1989) yielded coefficients that were opposite

in sign and not significantly different in magnitude for only 5 of the 13 job dimensions, and  $R^2$  values ranged from .07 to .70 with a median of .28. This evidence provides reason to question the construct validity of direct comparison measures as indicators of the difference between components. This evidence also has suggested that asking respondents to compare components may invoke cognitive processes other than the simple comparisons presumed in much congruence research. These processes may be worth studying in their own right, but they should not be considered proxies for the models that difference scores are intended to capture.

#### **Myth 4: Categorized Comparisons Avoid Problems With Difference Scores**

To avoid problems with difference scores, some researchers have created subgroups based on the congruence between two component measures. This approach has been used in studies of self-other agreement that classify respondents as overestimators, underestimators, or accurate, depending on where self-ratings fall relative to ratings from others. Some researchers have advocated this approach as an antidote for reliability problems created by difference scores (Mersman & Donaldson, 2000). Other researchers have recommended this approach because it provides directional information and corrects for level differences in self- and other ratings without relying on difference scores (Church & Waclawski, 1996). Still other researchers have adopted this approach as a response to the general call to replace difference scores with other analytical procedures (Godshalk & Sosik, 2000).

Although the subgrouping approach may create the illusion that problems with difference scores are avoided, it inherits these problems in full force and supplements them with problems created by categorizing continuous measures. With the subgrouping approach, respondents with self-scores above or below other scores by some threshold (e.g., one-half standard deviation) are classified as overestimators and underestimators, respectively, and the remaining respondents are classified as accurate. This procedure is tantamount to subtracting other scores from self-scores and trichotomizing the resulting difference score. Hence, classifications created by the subgrouping approach are difference scores in disguise, and trichotomizing these scores merely accentuates the loss of information and reduction in explained variance that plague difference scores. For instance, if a normally distributed difference score is trichotomized into three groups of equal size (corresponding roughly to cutoff values at one-half standard deviation above and below the mean), the variance explained by the trichotomized score is approximately 26% less than that explained by the continuous score (Peters & Van Voorhis, 1940), resulting in a loss of statistical power equivalent to dropping about 28% of the sample (Cohen, 1983). Thus, when compared with using difference scores, the subgrouping approach simply makes matters worse.

#### **Myth 5: Product Terms Are Viable Substitutes for Difference Scores**

When confronted by problems with difference scores, some researchers have resorted to product terms tested hierarchically in multiple regression analysis. This approach has been applied in studies of demographic similarity (Riordan & Shore, 1997), person-organization fit (Bretz & Judge, 1994), supervisor-subordinate agree-

ment (Deluga, 1998; Furnham & Stringfield, 1994), and the congruence between job requirements and employee skills (Arnold, 1994). The use of product terms as a substitute for difference scores is alluring, given that product terms analyzed hierarchically capture the interaction between two variables (Cohen, 1978), and the terms *interaction* and *fit* often have been used jointly, if not interchangeably, in congruence research (Chatman, 1989; Joyce, Slocum, & Von Glinow, 1982; Kahana, 1982; Kulka, 1979; Patsfall & Feimer, 1985; Pervin, 1989; Tinsley, 2000). The allure of product terms as substitutes for difference scores is bolstered by Cronbach (1958), who stated,

Any data fitted by the relation  $Z = k(|X - Y|)$  will also be fitted by a function such as  $Z = c(-XY)$ . Quite precise and extensive data are required to determine which function best fits the data in a given study. Absolute scores may be regarded as approximations of the more conventional measures of interaction, and vice versa. (p. 356)

Some researchers have cited Cronbach's (1958) statement as justification for using product terms in place of difference scores (Deluga, 1998).

For two dichotomous variables (e.g., supervisor and subordinate gender), the effects of congruence can be estimated using the two variables and their product, supplemented by analyses to determine whether the pattern of means conforms to a contrast of the two cells on the principal diagonal (i.e., both variables low or high) versus the cells off the diagonal (i.e., one variable low and the other variable high). However, for continuous measures, a product term does not represent the effects of congruence. This fallacy is demonstrated by the following regression equations, the first of which uses  $X$ ,  $Y$ , and their product as predictors:

$$Z = b_0 + b_1X + b_2Y + b_3XY + e. \quad (1)$$

This equation may be compared with the following equation, which uses the absolute difference between  $X$  and  $Y$  as a predictor of  $Z$ :

$$Z = b_0 + b_1|X - Y| + e. \quad (2)$$

The absolute value transformation in Equation (2) is a logical operation that can be rewritten as an algebraic expression using the following piecewise linear regression equation (Edwards, 1994):

$$Z = b_0 + b_1(1 - 2W)(X - Y) + e, \quad (3)$$

where  $W = 0$  if  $X > Y$ ,  $W = 1$  if  $X < Y$ , and  $W$  is randomly set to 0 or 1 when  $X = Y$ . Thus, when  $(X - Y)$  is positive, the term  $(1 - 2W)$  reduces to 1 and leaves the difference unaltered, whereas when  $(X - Y)$  is negative,  $(1 - 2W)$  becomes  $-1$ , reversing the sign of the difference, yielding the same effect as the absolute value transformation in Equation (2). Expanding Equation (3) yields

$$Z = b_0 + b_1X - b_1Y - 2b_1WX + 2b_1WY + e. \quad (4)$$

Equation (4) is a special case of a general piecewise linear equation that uses a separate coefficient on each term and adds  $W$  to appropriately estimate the coefficients on  $WX$  and  $WY$  (Cohen, 1978):

$$Z = b_0 + b_1X + b_2Y + b_3W + b_4WX + b_5WY + e. \quad (5)$$

Note that Equation (5) captures the interactions of  $X$  and  $Y$  with  $W$ , such that the slopes of  $X$  and  $Y$  differ depending on whether the  $(X - Y)$  difference is positive or negative. In contrast, Equation (1) captures the interaction between  $X$  and  $Y$ , such that the slope of  $X$  differs depending on the level of  $Y$  and vice versa. The conditional relationships indicative of congruence that Equation (5) captures cannot be detected by Equation (1).

The inadequacy of Equation (1) is further demonstrated by comparing it with the following equation, which uses the squared difference between  $X$  and  $Y$  to predict  $Z$ :

$$Z = b_0 + b_1(X - Y)^2 + e. \quad (6)$$

Expanding Equation (6) yields

$$Z = b_0 + b_1X^2 - 2b_1XY + b_1Y^2 + e. \quad (7)$$

Equation (7) is a special case of the following general quadratic equation (Edwards, 1994):

$$Z = b_0 + b_1X + b_2Y + b_3X^2 + b_4XY + b_5Y^2 + e. \quad (8)$$

Comparing Equation (8) to Equation (1) shows that the latter omits  $X^2$  and  $Y^2$ , meaning that it cannot capture curvatures in the relationships of  $X$  and  $Y$  with  $Z$  that characterize congruence effects.

The inadequacies of Equation (1) for estimating congruence effects are depicted graphically in Figures 1 and 2. Figure 1a shows a congruence effect corresponding to an absolute difference. Using Equation (1) to estimate this effect yields Figure 1b. Although Figures 1a and 1b show that  $Z$  decreases as  $X$  and  $Y$  deviate from one another in either direction, Figure 1b further indicates that  $Z$  increases along the  $Y = X$  line (i.e., the line of perfect fit) as  $X$  and  $Y$  deviate from their scale midpoints. This upward curvature along the  $Y = X$  line is inconsistent with the congruence effect depicted by Figure 1a. Likewise, Figure 2a shows a congruence effect following a squared difference. When Equation (1) is used to estimate this effect, the result is Figure 2b. Again, Equation (1) forces an upward curvature along the  $Y = X$  line that is inconsistent with the effect shown in Figure 2a. These graphical comparisons reinforce the fallacy of attempting to test congruence effects using Equation (1).

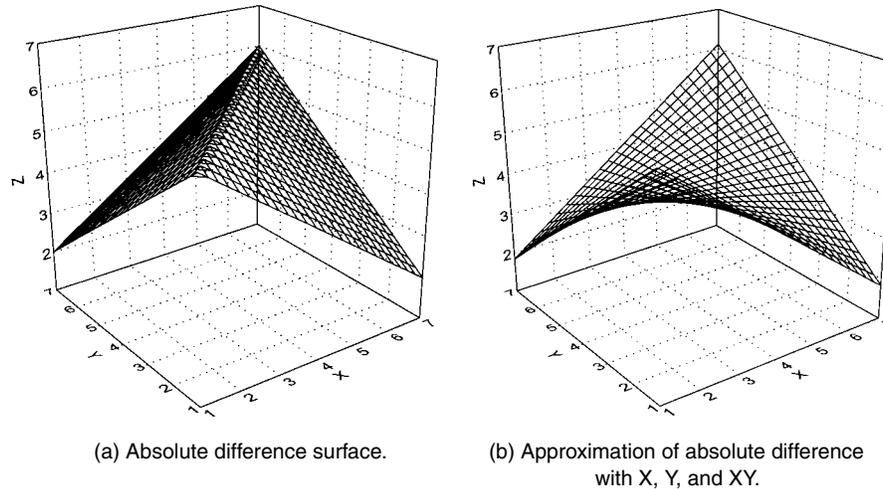


Figure 1: Congruence Effects for Absolute Difference and Product Term

### Myth 6: Hierarchical Analysis Provides Conservative Tests of Difference Scores

Some studies statistically control for component measures before estimating the effects of difference scores. This approach is characterized as conservative, based on the premise that controlling for component measures yields a stringent, purified test of congruence. Examples of this approach are provided by studies controlling for component measures when using absolute difference scores to test similarity effects for cognitive style (Tierney, 1997), values (Ashkanasy & O'Connor, 1997), affect (Bauer & Green, 1996), and demography (Glaman, Jones, & Rozelle, 1996). Other studies advocate tests of squared difference scores controlling for their components (Smith & Tisak, 1993) or the algebraic difference between their components (Tisak & Smith, 1994a). Controlling for difference score components may seem intuitively appealing, given that components are controlled when testing interactions using product terms (Cohen, 1978), and product terms have been treated as viable substitutes for difference scores. Some researchers have taken this analogy literally, arguing that analyzing difference scores without controlling for their component measures is essentially equivalent to testing an interaction term without lower-order terms in the equation (Hesketh, 2000).

Despite its appeal, controlling for component measures does not yield conservative tests of difference scores. Instead, this approach alters the relationships difference scores are intended to capture, such that a coefficient on a difference score that seems to support a congruence hypothesis may represent a relationship that is quite different, depending on the coefficients on the component measures. For illustration, consider the congruence relationship shown in Figure 1a, which corresponds to Equation (2). Adding  $X$  and  $Y$  to Equation (2) as control variables yields

$$Z = b_0 + b_1X_1 + b_2Y_2 + b_3|X - Y| + e. \quad (9)$$

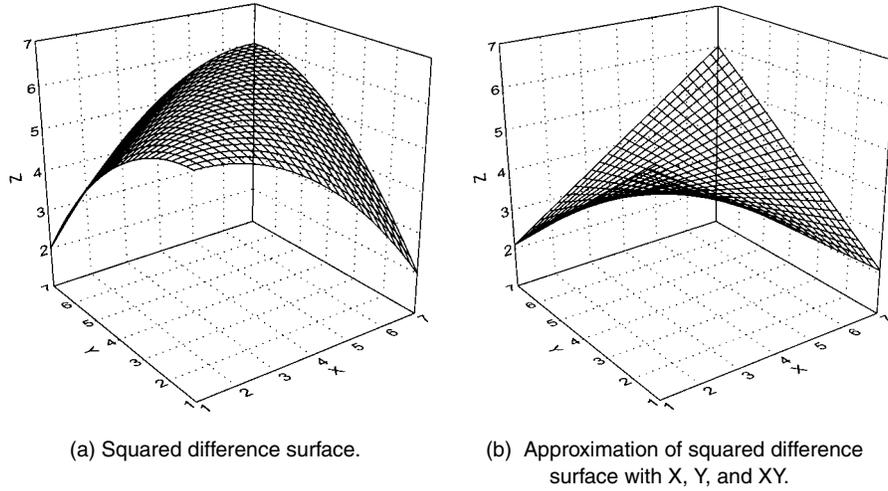


Figure 2: Congruence Effects for Squared Difference and Product Term

Following Equations (3) and (4), Equation (9) may be rewritten as

$$\begin{aligned} Z &= b_0 + b_1X + b_2Y + b_3X - b_3Y - 2b_3WX + 2b_3WY + e \\ &= b_0 + (b_1 + b_3)X + (b_2 - b_3)Y - 2b_3WX + 2b_3WY + e. \end{aligned} \quad (10)$$

Comparing Equation (10) to Equation (2) shows that controlling for  $X$  and  $Y$  relaxes the constraint on the coefficients on  $X$  and  $Y$ , such that these coefficients need not be equal in magnitude and opposite in sign. However, the changes in the coefficients for  $X$  and  $Y$  as  $W$  changes from 0 to 1 remain constrained to  $-2b_3$  for  $X$  and  $+2b_3$  for  $Y$ . As a result, Equation (10) represents a class of asymmetric relationships of  $X$  and  $Y$  with  $Z$  that may deviate substantially from the relationship shown in Figure 1a, depending on the values obtained for  $b_1$  and  $b_2$ . This phenomenon can be seen by comparing the surface in Figure 1a with surfaces in Figure 3 for which (a)  $b_1$  and  $b_2$  are positive, (b)  $b_1$  is positive and  $b_2$  is negative, and (c)  $b_1$  is negative and  $b_2$  is positive. Figure 3a shows that  $Z$  increases as  $X$  and  $Y$  increase along the  $Y = X$  line, and Figures 3b and 3c show that the surface is flat when  $X > Y$  or  $X < Y$ , respectively. For all three surfaces, the coefficient on  $|X - Y|$  is the same value as that used to plot the surface in Figure 1a. If  $b_1$  and  $b_2$  are disregarded (as is usually the case when  $X$  and  $Y$  are treated as control variables), the pronounced differences among these surfaces would be overlooked.

Similar results are produced by controlling for  $X$  and  $Y$  in an equation using a squared difference as a predictor:

$$Z = b_0 + b_1X + b_2Y + b_3(X - Y)^2 + e. \quad (11)$$

Expanding Equation (11) yields

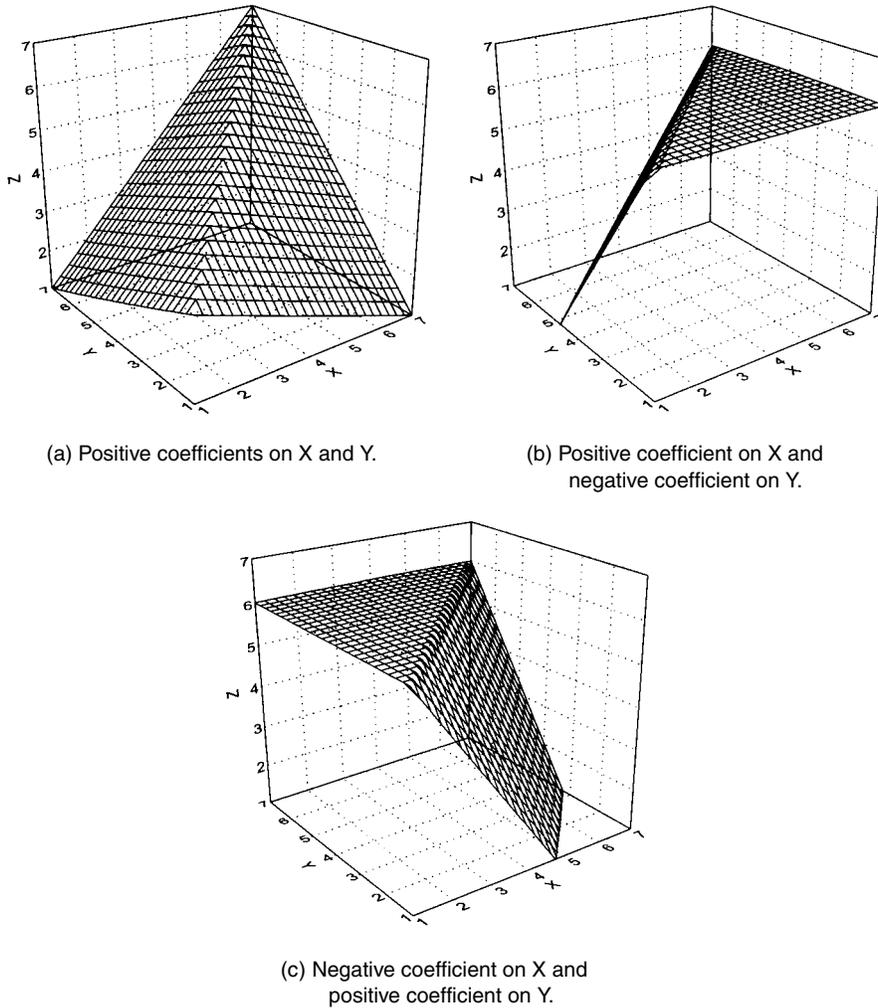


Figure 3: Effects of Controlling for X and Y When Estimating Absolute Difference Effect

$$Z = b_0 + b_1X + b_2Y + b_3X^2 - 2b_3XY + b_3Y^2 + e. \quad (12)$$

Comparing Equation (12) with Equation (7) shows that controlling for X and Y relaxes the constraints that force their coefficients to zero but maintains the constraints on the coefficients for  $X^2$ ,  $XY$ , and  $Y^2$ . Consequently, Equation (12) corresponds to a class of quadratic relationships that may differ markedly from the relationship shown in Figure 2a, depending on the estimated values of  $b_1$  and  $b_2$ . For illustration, Figure 2a may be compared with Figure 4, which shows surfaces where (a)  $b_1$  and  $b_2$  are positive, (b)  $b_1$  is positive and  $b_2$  is negative, and (c)  $b_1$  is negative and  $b_2$  is positive. Figure 4a shows that along the  $Y = X$  line, Z increases as X and Y increase. Figures 4b and 4c show that Z

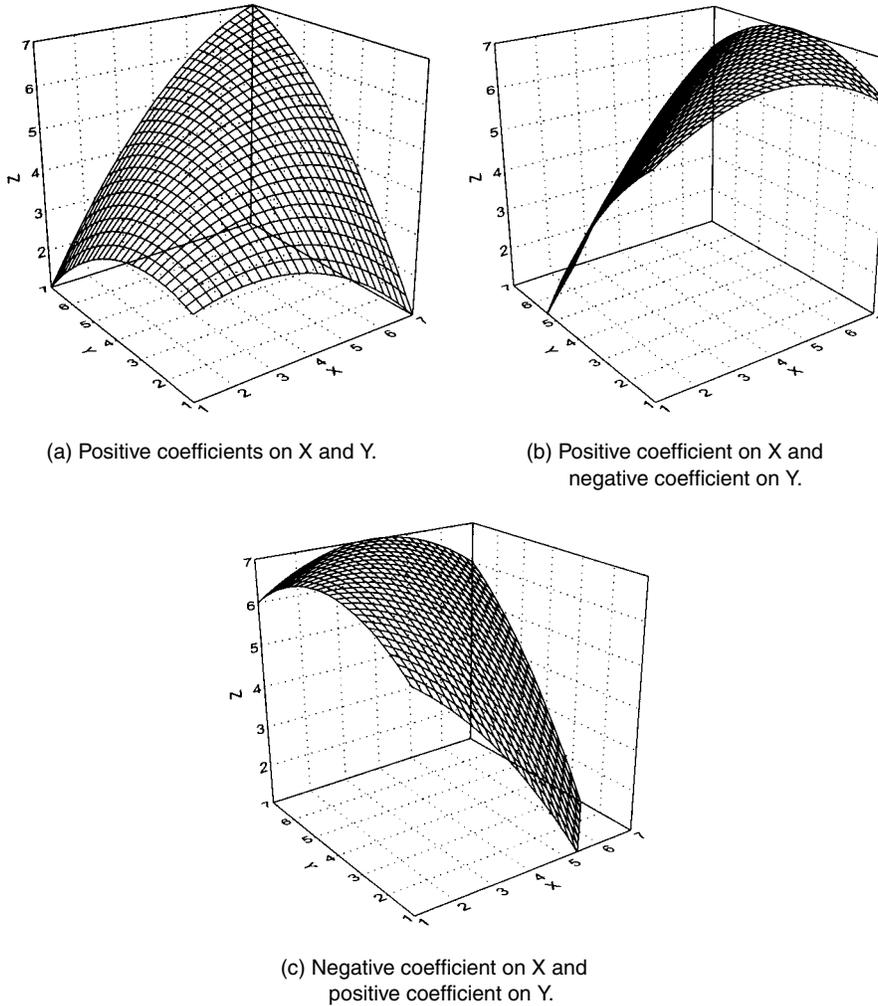


Figure 4: Effects of Controlling for X and Y When Estimating Squared Difference Effect

is maximized not along the  $Y = X$  line but instead where  $X > Y$  or  $X < Y$ , respectively. Again, for all three surfaces, the coefficients on  $(X - Y)^2$  are the same as that for the surface in Figure 2a, but the interpretations of the surfaces differ substantially depending on the values of  $b_1$  and  $b_2$ .

### Myth 7: Polynomial Regression Is an Exploratory, Empirically-Driven Procedure

One particularly pernicious myth is that polynomial regression is an exploratory, empirically driven procedure. This myth was promulgated by Tisak and Smith (1994b), who argued in favor of difference scores over polynomial regression by stat-

ing that “the fundamental issue . . . is whether one should develop models that depict predetermined theoretical concerns . . . or use general purpose models that attempt to fit data” (p. 693). A similar position was advanced by Tinsley (2000), who claimed that polynomial regression “capitalizes on sample specific variance to maximize the amount of variance explained” (p. 171). Bedeian and Day (1994) also characterized polynomial regression as empirically driven, stating that it compels researchers “to formulate more complex theories to account for the empirical relations” and “may be letting the empirical tail wag the theoretical dog” (pp. 695-696). These sentiments have also surfaced in empirical studies in which researchers acknowledge problems with difference scores (Witt, 1998) and report evidence that rejects difference score assumptions (Porter, Pearce, Tripoli, & Lewis, 1998) but nonetheless argue that difference scores were required to test a priori hypotheses based on theory.

Despite claims to the contrary, the first and foremost goal of polynomial regression is to test hypotheses derived from theories of congruence. This goal is evident in the development and initial applications of polynomial regression (Edwards, 1991, 1994, 1996; Edwards & Harrison, 1993), which treated constraints imposed by difference scores as a priori hypotheses to be tested empirically. For instance, using an algebraic difference score as a predictor embodies the hypothesis that the components of the difference have equal but opposite effects (Edwards, 1994). Polynomial regression provides an explicit test of this hypothesis whereas using an algebraic difference score incorporates this hypothesis as an untested assumption. Likewise, the constraints imposed by using an absolute or squared difference score as a predictor are tantamount to hypothesizing that (a) the dependent variable is minimized (or maximized) at some constant value when the two components are equal, and (b) the dependent variable increases (or decreases) symmetrically as the components deviate from one another in either direction. Again, these hypotheses can be explicitly tested with polynomial regression but are taken for granted when absolute or squared difference scores are used. Thus, polynomial regression provides comprehensive tests of a priori hypotheses derived from theories of congruence, whereas difference scores allow congruence hypotheses to evade empirical scrutiny.

When coupled with response surface methodology (Edwards & Parry, 1993), polynomial regression permits tests of hypotheses that go well beyond the narrow range of relationships that correspond to difference scores. These benefits are illustrated by Edwards and Rothbard (1999), who translated hypotheses derived from person-environment fit theory (Edwards, Caplan, & Harrison, 1998) into three-dimensional surfaces that are substantially more complex than are the simplified surfaces implied by difference scores. Other theories of congruence (e.g., Kulka, 1979; Naylor, Pritchard, & Ilgen, 1980; Rice, McFarlin, Hunt, & Near, 1985) also postulate relationships that outstrip the simplistic associations implied by difference scores. Thus, although difference scores have been touted as theory driven, they limit attention to a small subset of hypotheses that follow from theories of congruence and fail to provide evidence necessary to confirm or disconfirm these hypotheses.

Like any analytical procedure, polynomial regression can also be applied in an exploratory manner. However, exploratory applications are justified only when a priori hypotheses are rejected or cannot be derived from available theory. Moreover, as Edwards (1994) stated unequivocally, results from exploratory analyses “are subject to cross-validation and conceptual scrutiny. . . it is folly to construct elaborate post-hoc interpretations of complex surfaces that are not both generalizable and conceptu-

ally meaningful” (p. 74). These guidelines have been followed in exploratory applications of polynomial regression, which were prompted by the wholesale rejection of hypotheses implied by difference scores (Edwards, 1996; Edwards & Harrison, 1993). Thus, the treatment of polynomial regression as exploratory or confirmatory reflects the choices of the researcher, not the attributes of the method itself, and any exploratory application should follow accepted guidelines for exploratory research (Behrens, 1997).

### **Myth 8: Polynomial Regression Suffers From Multicollinearity**

Some researchers have claimed that polynomial regression suffers from multicollinearity. For instance, Kristof (1996) cautioned that the terms  $X^2$ ,  $XY$ , and  $Y^2$  in Equation (8) will exhibit some multicollinearity with the  $X$  and  $Y$  terms. A similar warning was issued by Tinsley (2000), who further argued that multicollinearity between lower-order and higher-order terms in polynomial regression creates problems that “are critical in research designed to increase our understanding of the P-E interaction [*sic*]” (p. 171). Warnings such as these sound a formidable alarm, given that multicollinearity can wreak havoc on parameter estimates and standard errors in multiple regression analysis (Belsley, Kuh, & Welsch, 1980).

Concerns regarding multicollinearity between lower-order and higher-order terms are unfounded. This form of multicollinearity represents nonessential ill-conditioning (Marquardt, 1980) and can be eliminated by rescaling  $X$  and  $Y$  prior to calculating  $X^2$ ,  $XY$ , and  $Y^2$ . For instance, if  $X$  and  $Y$  have a bivariate normal distribution, then centering  $X$  and  $Y$  at their means reduces their covariances with  $X^2$ ,  $XY$ , and  $Y^2$  to zero. (Aiken & West, 1991; Bohrnstedt & Goldberger, 1969). If  $X$  and  $Y$  are not distributed bivariate normal, they may be centered at alternative values near their respective means to reduce or eliminate their covariances with  $X^2$ ,  $XY$ , and  $Y^2$ . Although such rescaling may yield some peace of mind, it is entirely unnecessary, because altering the covariances of  $X$  and  $Y$  with  $X^2$ ,  $XY$ , and  $Y^2$  has no bearing on the substantive interpretation of the joint relationship of  $X$  and  $Y$  with  $Z$ . Regardless of the values at which  $X$  and  $Y$  are centered,  $X^2$ ,  $XY$ , and  $Y^2$  yield the same coefficient estimates and standard errors, and the variance explained by these terms and the equation as a whole remain unchanged (Cohen, 1978). In contrast, the coefficients on  $X$  and  $Y$  vary depending on the values at which  $X$  and  $Y$  are centered. However, this variability should be expected, given that the coefficients on  $X$  and  $Y$  represent the slope of the surface at the point  $X = 0$ ,  $Y = 0$ . Rescaling  $X$  and  $Y$  shifts this point to different regions of the surface, which logically should yield different coefficients on  $X$  and  $Y$  (provided the coefficients on  $X^2$ ,  $XY$ , and  $Y^2$  are not zero). Thus, although the covariances of  $X$  and  $Y$  with  $X^2$ ,  $XY$ , and  $Y^2$  may be manipulated by rescaling  $X$  and  $Y$ , the conclusions yielded by polynomial regression analysis are unaffected.

The covariances among  $X^2$ ,  $XY$ , and  $Y^2$  are also affected by rescaling  $X$  and  $Y$ , but rescalings that minimize these covariances generally do not minimize the covariances of  $X$  and  $Y$  with  $X^2$ ,  $XY$ , and  $Y^2$ , and vice versa. However, rescaling  $X$  and  $Y$  to minimize covariances among  $X^2$ ,  $XY$ , and  $Y^2$  is pointless because the substantive interpretation of the analysis remains the same regardless of how  $X$  and  $Y$  are rescaled. In contrast, the covariance between  $X$  and  $Y$  is not affected by rescaling, and a high covariance raises multicollinearity concerns irrespective of the inclusion of  $X^2$ ,  $XY$ , and  $Y^2$  in the regres-

sion equation. Moreover, a high covariance between  $X$  and  $Y$  creates problems for congruence research regardless of the analytical procedure used because a high covariance indicates that the joint distribution of  $X$  and  $Y$  is narrow and elliptical, which in turn signifies range restriction along the axis of the distribution that represents variation in congruence. Without adequate dispersion along this axis, congruence effects are difficult to detect, regardless of how they are analyzed.

### **Myth 9: Higher-Order Terms Do Not Enhance the Understanding of Congruence**

Researchers have questioned whether higher-order terms used in polynomial regression equations shed light on the meaning and effects of congruence. For instance, some researchers acknowledge that higher-order terms may explain significant amounts of variance but assert that such terms do not contribute to the understanding of congruence (Hesketh, 2000; Kristof, 1996; Tinsley, 2000). These sentiments hearken back to reservations expressed by Bedeian and Day (1994), who questioned whether current theories explain higher-order terms and their associated response surfaces. In a similar vein, Tisak and Smith (1994b) asserted that the need to select and interpret appropriate higher-order terms constitutes a liability of polynomial regression analysis.

Admittedly, the interpretation of higher-order terms can be difficult, at least initially. Difficulties may arise from attempts to interpret coefficients on higher-order terms individually (e.g., the effect of  $X^2$  on  $Z$  controlling for the effects of  $X$ ,  $Y$ ,  $XY$ , and  $Y^2$ ) or efforts to translate congruence hypotheses into expected coefficients on  $X^2$ ,  $XY$ , and  $Y^2$ . These difficulties may be ameliorated by using response surfaces as the intermediary between congruence hypotheses and polynomial regression coefficients. In this manner, congruence hypotheses are translated into three-dimensional surfaces that stipulate patterns of regression coefficients, and the obtained coefficient estimates are used to determine whether the hypothesized surface is supported empirically. For example, studies of person-organization fit often predict that fit leads to positive outcomes (e.g., satisfaction, commitment, performance). This prediction translates into a surface that is flat along the  $Y = X$  line and decreases as  $X$  and  $Y$  deviate from one another in either direction, as in Figure 2a. In turn, this surface corresponds to the quadratic regression equation in Equation (8) in which  $b_1$  and  $b_2$  are zero,  $b_3$  and  $b_5$  are negative and equal, and  $b_4$  is positive and twice as large in absolute magnitude as  $b_3$  and  $b_5$ . Moreover, combinations of coefficients can be used to test specific aspects of the surface. For example, linear combinations of  $b_1$ ,  $b_2$ ,  $b_3$ ,  $b_4$ , and  $b_5$  can be used to determine whether the surface is flat along the  $Y = X$  line and curved downward along lines perpendicular to the  $Y = X$  line (Edwards & Parry, 1993). These procedures do not isolate individual coefficients from Equation (8) but instead treat these coefficients as a set, given that all five coefficients are required to understand the shape of the surface relating  $X$  and  $Y$  to  $Z$ . In short, coefficients from polynomial regression analysis provide considerable conceptual insight when congruence hypotheses are mapped onto surfaces that in turn are translated into regression coefficients analyzed collectively.

### Myth 10: Polynomial Regression Eliminates the Concept of Congruence

Finally, some researchers have argued that replacing difference scores with polynomial regression eliminates the concept of congruence. This myth derives from the assumption that a difference score represents a concept that is distinct from its components. This assumption underlies arguments advanced by Tisak and Smith (1994a, 1994b), who asserted that difference scores and their component measures are not conceptually interchangeable. As argued by Tisak and Smith (1994b), "The concept of role conflict obtained from the differences between subordinate and supervisor job ratings is not the same as conceptualizations of subordinate and supervisor ratings" (p. 691). Citing these arguments, Kristof (1996) and Tinsley (2000) questioned whether difference scores and polynomial regression address the same concept, ultimately concluding that this question should be resolved empirically. Arguments such as these have also been used to justify the use of difference scores in empirical studies. For instance, Jehn and Chatman (2000) stated that their decision to use difference scores rather than component measures "was dictated by our theoretical perspective, which required that we use a combination of the two components (self and group perception) rather than two single component measures" (p. 65).

Although the concept represented by a difference score is distinct from its components taken separately, it is not distinct from its components considered jointly. This point is illustrated by Figure 5, which shows lines representing differences between  $X$  and  $Y$  ranging from  $-5$  to  $+5$ . The solid line represents all combinations of  $X$  and  $Y$  for which  $X - Y = 0$ . The dashed lines toward the upper left indicate combinations of  $X$  and  $Y$  that yield  $X - Y$  values of  $-1$ ,  $-2$ ,  $-3$ ,  $-4$ , and  $-5$ , and the dashed lines toward the lower right indicate  $X$  and  $Y$  combinations that yield  $X - Y$  values of  $+1$ ,  $+2$ ,  $+3$ ,  $+4$ , and  $+5$ . As Figure 5 makes evident, any given value of the  $X - Y$  difference represents nothing more than the combination of  $X$  and  $Y$  values, and a value of the difference is fully determined by the  $X$  and  $Y$  values taken jointly. Therefore, it is illogical to argue that a concept captured by the  $X - Y$  difference is not likewise captured jointly by  $X$  and  $Y$ . Figure 5 also shows that a difference score discards information regarding the absolute levels of its components. For instance, the difference  $X - Y = 0$  conceals whether  $X$  and  $Y$  are low or high in an absolute sense, as indicated by movement along the solid line in Figure 5. Although congruence hypotheses often imply that the absolute levels of components are irrelevant, this implication cannot be tested unless absolute component levels are preserved in data analysis. In short, because a difference score is calculated from its components, it cannot represent a construct that is conceptually or operationally distinct from its components taken jointly.

Some researchers might argue that squared or absolute difference scores represent constructs that are not captured by their components, given that these difference scores are not simple linear combinations of component measures. However, squared and absolute difference scores are merely mathematical transformations intended to represent a particular functional form relating the components to the outcome. These transformations do not create information beyond that contained in the components of the difference, nor do they represent concepts that are distinct from the component measures supplemented by higher-order terms calculated from these measures.

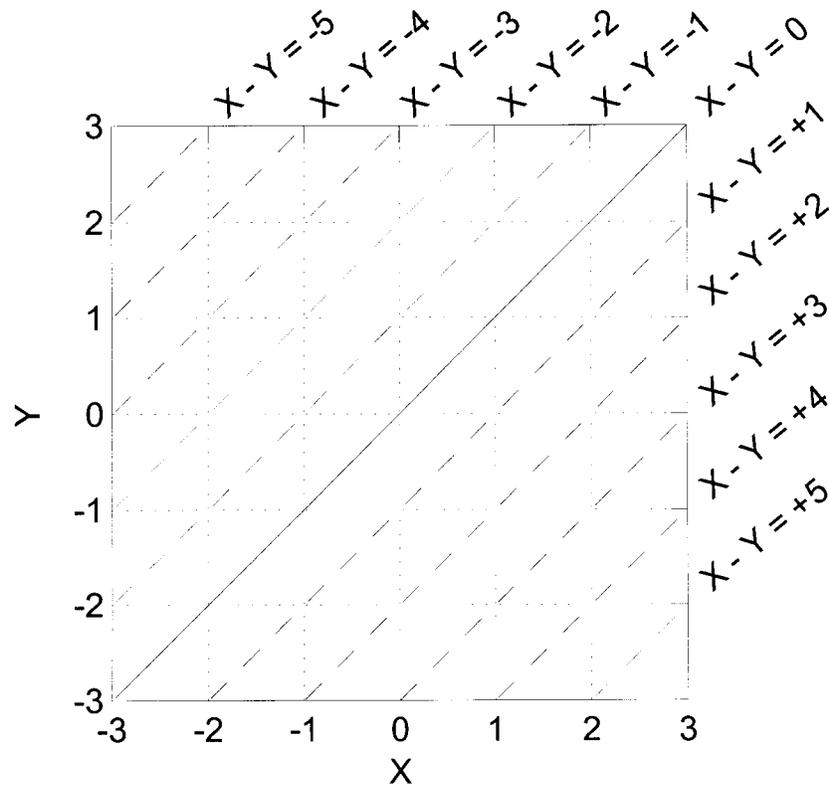


Figure 5: Mapping  $X - Y$  Differences Onto Combinations of  $X$  and  $Y$

At a more fundamental level, the calculation of a difference score implies that two constructs exist, as represented by the component measures. The difference between these constructs does not itself represent a construct but instead refers to the proximity between two constructs. To paraphrase Cronbach and Gleser (1953), congruence is not an entity unto itself but instead can be conceptualized only in reference to its components. It is these components taken jointly, not some ephemeral entity created by calculating their difference, that signify the meaning of congruence.

### Myth About Myths

The preceding myths have themselves kindled a broader myth that the use of difference scores versus polynomial regression is a matter of debate. Although such a debate was staged (Bedeian, Day, Edwards, Tisak, & Smith, 1994), it was replete with misrepresentations of polynomial regression and offered no resolution to the opposing views. In the end, this debate did little more than promote the spread of difference score myths and provide a convenient citation for researchers to frame the choice between difference scores and polynomial regression as a judgment call, as illustrated by one instigator of the debate (Phillips & Bedeian, 1994):

Despite their popularity, the appropriateness of deviation scores for estimating differences between measurement units continues to be a source of much debate (Bedeian, Day, Edwards, Tisak, & Smith, in press). Some authors have advised that the statistical and psychometric properties of deviation scores are so problematic that their use should be discontinued (e.g., Edwards, in press). Others have concluded that such criticisms are unfounded, declaring deviation scores to be both reliable and unbiased (e.g., Smith & Tisak, 1993). Weighing arguments favoring both positions and recognizing the grounding of our research in Graen's leader-member exchange perspective, we judged deviation scores appropriate for use. (p. 994)

Other researchers have followed suit. For example, M. Lubatkin and Powell (1998), referring to the debate staged by Bedeian et al. (1994), claimed that "the verdict is still out" (p. 1018) regarding the relative merits of difference scores and polynomial regression and used absolute difference scores to represent person-environment fit (see also A. H. Lubatkin et al., 1999). Likewise, Deluga (1998) cited Bedeian et al. (1994) to indicate that the use of difference scores is a source of debate but then used a product term to capture supervisor-subordinate similarity, citing Cronbach (1958) as justification.

The use of difference scores versus polynomial regression does not provide grounds for a productive debate, for several reasons. First, algebraic expansion shows that most difference scores are special cases of polynomial regression equations, as illustrated by comparing Equations (3) and (6) with Equations (5) and (8), respectively. There is little basis for arguing that a difference score is superior to polynomial regression when the former is subsumed by the latter. Difference scores that cannot be expanded into polynomial regression equations nonetheless refer to congruence hypotheses that can be tested with polynomial regression. For instance, the Euclidean distance (i.e., the square root of the sum of squared differences) has been used to represent nondirectional congruence hypotheses in which some outcome is minimized or maximized when component measures are equal across several dimensions (e.g., Basu & Green, 1995; Church, 1997; Church & Waclawski, 1999; Sutcliffe, 1994; Young, Smith, Grimm, & Simonc, 2000). Such hypotheses can be tested using augmented versions of Equations (5) and (8) containing multiple  $X_i$  and  $Y_i$  and their associated higher-order terms (Edwards, 1993). Hence, from a conceptual standpoint, these equations subsume Euclidean distances and the hypotheses they are intended to represent.

Second, the assumptions embedded in difference scores can be tested empirically using polynomial regression (Edwards, 1994). It seems pointless to argue in favor of difference scores over polynomial regression when the argument can be settled empirically. Empirical tests have soundly rejected difference score assumptions (e.g., Edwards, 1991, 1993, 1994; Edwards & Harrison, 1993), but it is nonetheless prudent to test these assumptions on a study-by-study basis. If the assumptions are not supported, then the hypothesis that motivated the use of the difference score is rejected. Further interpretation of results enters the realm of exploratory analyses and, as emphasized here and elsewhere (e.g., Edwards, 1994), such interpretations should be considered tentative, pending cross-validation. If the assumptions underlying the difference score are supported, then the hypothesis it represents may be considered tenable. At that point, further analyses using the difference score itself would be superfluous because coefficients from the polynomial regression equation provide all the information necessary to determine the nature and magnitude of the congruence effect.

Finally, the authors positioned as advocates of difference scores concluded that they “are not necessarily advocating a different statistical approach than Edwards, but rather a different order in which the approach should be applied” (Tisak & Smith, 1994b, p. 694). Specifically, they suggested testing intermediate models in which constraints imposed by difference scores are progressively relaxed. This approach is reasonable, assuming the intermediate models capture meaningful hypotheses from congruence theories. However, testing these models requires the use of polynomial regression itself, given that the models embody congruence hypotheses and polynomial regression provides tests of these hypotheses. Moreover, many criticisms advanced by Tisak and Smith (1994a, 1994b) did not admonish polynomial regression per se but instead argued against exploratory data analysis divorced from theory. These criticisms are specious because they denounce an approach to research rather than polynomial regression itself, and this approach was never advocated in the development or application of polynomial regression. Finally, one opponent in the debate has subsequently advocated polynomial regression in favor of difference scores (Brutus, Fleenor, & Tisak, 1999). Thus, any pretense of a debate has all but vanished.

### **Limitations of Polynomial Regression**

Like any analytical technique, polynomial regression has its shortcomings. For instance, polynomial regression inherits the assumptions of multiple regression analysis that independent variables are measured without error (Pedhazur, 1997). Measurement error biases coefficient estimates and reduces statistical power for tests of squared and product terms (Busemeyer & Jones, 1983; Dunlap & Kemery, 1988). Measurement error can be accommodated by structural equation modeling with latent variables (Bollen, 1989), and applications to quadratic equations used to test congruence hypotheses have been promising (Edwards & Bagozzi, 1999). Second, when testing congruence hypotheses that pertain to multiple dimensions, polynomial regression equations can contain many terms (Edwards, 1993). Such equations may require large samples to provide adequate statistical power. This shortcoming is not unique to polynomial regression but instead applies to any regression equation with many independent variables. This shortcoming should be addressed not by resorting to difference scores but instead by collecting enough data to obtain the desired level of statistical power.

### **Summary and Conclusion**

Polynomial regression is progressively supplanting difference scores in congruence research. Results from polynomial regression are more comprehensive and conclusive than those obtained from difference scores, and these results are beginning to reshape basic postulates of congruence theories. These advancements are threatened by the current spread of myths regarding problems with difference scores and the advantages of polynomial regression. This article has attempted to dispel these myths, thereby encouraging researchers to set aside vacuous methodological debates and instead pursue important theoretical questions regarding the nature, causes, and consequences of congruence.

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*Jeffrey R. Edwards (Ph.D., Carnegie Mellon University) is the Belk Distinguished Professor of Management at the Kenan-Flagler Business School at the University of North Carolina. He studies stress, coping and well-being, person-organization fit, and methodological issues. His work has appeared in the Academy of Management Review, the Academy of Management Journal, the Journal of Applied Psychology, Organizational Behavior and Human Decision Processes, Personnel Psychology, Psychological Methods, and Organizational Research Methods. He is past chair of the Research Methods Division of the Academy of Management.*