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REJOINDER

The Congruence Myth Revisited

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In this article I note areas of agreement with Dawis (2000); Gati (2000); Hesketh (2000); Prediger (2000); Rounds, McKenna, Hubert, and Day (2000); and Tracey, Darcy, and Kovalski (2000); reply to points with which I disagree; and suggest priorities for future research.

My objectives when reviewing the research on the P–E fit model were to summarize what is currently known about the strengths and limitations of P–E fit theory as a model for understanding vocational behavior and to suggest potentially fruitful extensions of P–E fit research. Dawis (2000); Gati (2000); Hesketh (2000); Prediger (2000); Rounds, McKenna, Hubert, and Day (2000); and Tracey, Darcy, and Kovalski (2000) have written thought-provoking comments in which they agree with some of my conclusions, challenge others, and offer additional ideas for revitalizing research on the P–E fit model. The authors have raised far too many points for me to respond to them all; I counted more than 70 specific points that deserve comment. On the one hand, I feel an urge to correct some mistakes, but other issues of greater significance demand attention, so I will limit myself to urging readers to check the accuracy of the assertions made by the comment writers and by me against the original sources; we all can and do make mistakes. In this Rejoinder I note areas of consensus and I reply to points with which I disagree. My objective is not to say "who is right," but to highlight issues that merit further theoretical elaboration and empirical scrutiny.

After careful study of the PE fit literature it is my view that research on this model has stagnated in the past two decades as a consequence of the overwhelming appeal of Holland's (1997) model. With a few notable exceptions (e.g., Hesketh & Gardner, 1993; Gati, 1998; Gati, Fassa, & Mayer, 1998), P–E fit research has focused on Holland's (1997) model and even more narrowly on whether the six RIASEC types approximate a circular order when depicted in

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two-dimensional space. Holland (1997, p. 159) himself has criticized this myopia repeatedly, arguing that far more important issues are being ignored by vocational scholars. Holland and Gottfredson (1992) have criticized "the focus of research on the psychometric properties of inventories to the exclusion of research on the manner in which scores are used and interpreted and the effects of assessments on clients" (p. 169). Tinsley and Chu (1999) have documented the legacy of this neglect; we cannot even answer basic questions about the effectiveness of counseling with and without assessment or the cost effectiveness of assessment.

Despite this pessimistic assessment, I see reason for hope in the comments. Although Prediger (2000), Rounds et al. (2000), and Tracey et al. (2000) focus primarily on Holland's model, Gati (2000) and Hesketh (2000) suggest new directions for research on the P–E fit model and potentially fruitful areas for expansion or elaboration of P–E fit models, and Dawis (2000) offers a conceptual overview of the science of psychology from a P–E fit perspective. Therefore, in this Rejoinder I model the direction in which I hope to see the field move. I begin by focusing on issues pertaining to Holland's structural model, then move beyond that to consider the broader range of P–E fit issues that I believe hold great promise for expanding our understanding of human interactions.

RELATION OF HEXAGONAL CONGRUENCE TO VOCATIONAL OUTCOMES

Decision-Making Process

Rounds et al. (2000) rebut my conclusion that Holland's (1997) theory lacks validity by asserting that I based my conclusion on flawed "meta-analyses by Assouline and Meir (1987) and Tranberg, Slane, and Ekeberg (1993), as well as several studies published after those meta-analyses were completed" (Rounds et al., 2000, p. 206). They identify several "possible sources of error in meta-analysis" (Rounds et al., 2000, page 206), failing to note for the reader that I had considered those and additional possible sources of error before reaching my conclusion. Prediger (2000) and Tracey et al. (2000) also disagree with my conclusion and cite several factors that might have attenuated the relation between hexagonal congruence and vocational outcome, but in contrast to Rounds et al. (2000) they acknowledged that I considered those possible methodological weaknesses. Prediger takes the position that ". . . P–E fit studies involving interests and job satisfaction must undergo several refinements *before these results can say anything conclusive* . . ." (p. 197, italics added). While I acknowledge that significant weaknesses are apparent in P–E fit research, I believe that we can and should draw some conclusions from the research evidence that has been generated over the last 30 years.

Given this disagreement, it is instructive to reiterate my decision making process. My review of the literature revealed that hexagonal congruence was not significantly related to a variety of vocational outcomes such as job satisfaction,

college major satisfaction, tenure, achievement, mobility, commitment, career indecision, performance, stress, and strain. Prior to drawing a conclusion about the meaning of this body of research, however, I considered the possibility that several weaknesses in the research might account for the failure of Holland's model to predict theoretically relevant vocational outcomes. These included (a) restriction of range caused by sampling inadequacies, (b) flaws in the congruence indices used in these studies, and (c) the use of inadequate measures of personality, the environment, or the criterion. I was able to eliminate restriction of range and flawed congruence indices as possible explanations of the failure of hexagonal congruence to predict vocational outcomes. However, I was not able to reject the possibility that the failure of hexagonal congruence to predict theoretically relevant vocational outcomes is attributable to (a) the use of instruments to measure Holland's RIASEC constructs that confound theoretically distinct elements of the P–E fit model, or (b) the possibility that Holland's theory about the importance of hexagonal congruence on the RIASEC dimensions is invalid. The procedure I followed is fundamental to the scientific method; eliminate as many of the alternatives as possible and the correct explanation must be one of those remaining.

The potential confound that I was not able to dismiss was given little attention by Rounds et al. (2000). The measures used to derive Holland's codes for individuals, e.g., the Strong Interest Inventory (Harmon, Hansen, Borgen, & Hammer, 1994) and Self-Directed Search (Holland, 1985a) and those used to obtain RIASEC scores for environments use the mixing bowl approach in which all of the elements of the P–E fit model are tossed into the bowl (i.e., test) at the same time and thoroughly mixed. Hesketh (2000) notes that higher validity indices generally are obtained in selection research than in job choice research and attributes that disparity, in part, to the failure to use commensurate measures. It is possible that operationalizing Holland's theory using well-validated instruments that allow commensurate measurement would yield results that are more supportive of the theory.

Restriction of Range

My conclusion that the failure of the hexagonal congruence model to predict important vocational outcomes is not attributable to restriction of the range of congruence scores was based on my observation that the studies that had used the largest and most diverse samples (i.e., Hoeglund & Hansen, 1999; Tokar & Subich, 1997; Young, Tokar, & Subich, 1998) reported a weaker congruence–satisfaction relation than those that had used less adequate samples. Prediger (2000), Rounds et al. (2000), and Tracey et al. (2000) disagree, citing restriction of range as a likely cause of the nonsignificant congruence–outcome relation, but they provide no data to support their argument. Prediger suggests that in order to investigate P–E fit without restriction of range problems the random assignment of a general sample of persons to a cross-section of occupations would be necessary. Tracey et al. take the position that "anything short of random assign-

ment is attenuated due to selection effects and *no conclusions about the validity of P–E fit can or should be generated"* (Tracey et al., 2000, p. 218, italics added). Although I regard that position as too extreme, the possibility that selection effects may have attenuated the correlation between P–E fit and vocational outcomes should be considered.

Given this difference of opinion I attempted to investigate this issue empirically. I found that virtually none of the investigations reviewed by Assouline and Meir (1987) and Tranberg et al. (1993) reported the mean or variance in congruence scores, but that more recent research reports have provided the necessary data. Table 1 summarizes the data from those studies published since the Tranberg et al. (1993) meta-analysis in which the variance in congruence was reported. While the random assignment of persons to occupations is not feasible, Brown and Gore (1994) calculated the distribution of congruence scores that would result if individuals were assigned to occupations at random (see Table 1). Those data provide useful norms against which to compare the mean and standard deviation of congruence scores obtained in a specific study.

A consistent pattern emerges regardless of the congruence measure used; in most instances, the variance in congruence equals or exceeds that which would be expected if persons were assigned to jobs at random. Furthermore, the mean congruence score is often slightly higher than the expected population average, thereby providing weak support for the hypothesis that people tend to gravitate toward congruent occupations. It appears, therefore, that the supposition that restriction of range has attenuated the correlation between hexagonal congruence and vocational outcomes is unfounded. In contrast to the suggestion of Holland (1997), Prediger (2000), Rounds et al. (2000), and Tracey et al. (2000), the published literature supports the conclusion that, the better the research in terms of methodological considerations such as sample size, diversity of participants and occupations, and validity of instruments and congruence index, the less the support for Holland's (1997) theory.

Two Additional Methodological Weaknesses

Biserial correlations. During my review I discovered that some early researchers performed a median split (or a related procedure) to obtain groups of congruent and incongruent respondents. Procedures such as that discard information about the level of congruence and lead to the calculation of a biserial correlation between congruence and the vocational outcome. The biserial correlation can be interpreted as an estimate of the product moment correlation that would have been obtained if the continuous variable (i.e., congruence) had not been dichotomized. However, it yields an accurate estimate of the product moment correlation only when the population continuous variables are normally distributed. Since Prediger (2000), Rounds et al. (2000), and Tracey et al. (2000) all argue that people gravitate toward congruent occupations, there seems to be a consensus that the population of congruence scores is not distributed normally. The pattern of findings in Table 1 is consistent with that interpretation. Therefore,

TABLE 1
Comparison of the Means and Variances of Hexagonal Congruence Indices Reported in Five Studies with the Population Means and Variances of Those Indices

	Population		Range of possible congruence scores		Sample	
Index/authors	*M*	*SD*	Lowest	Highest	*M*	*SD*
C index	9.00	3.69	0	18		
Tokar & Subich (1997)					11.14	4.21
Young et al. (1998)					11.09	4.04
Oleski & Subich (1996)					10.09	3.52
Lent & Lopez (1996) Site 1, DHOC					10.56	3.55
Lent & Lopez (1996) Site 1, EAT					11.37	3.68
Lent & Lopez (1996) Site 2, DHOC					8.48	3.34
Lent & Lopez (1996) Site 2, EAT					11.08	3.29
Zenner–Schnuelle Index	1.39	1.34	0	6		
Young et al. (1998)					2.67	1.66
Camp & Chartrand (1962)					1.79	1.24
Kwak-Pulvino (based on SDS)	.33	.21	−1	1		
Tokar & Subich (1997)					.51	.29
Young et al. (1998)					.50	.28
Oleski & Subich (1996)					.41	.22
Lent & Lopez (1996) Site 1, DHOC					.52	.20
Lent & Lopez (1996) Site 1, EAT					.52	.25
Lent & Lopez (1996) Site 2, DHOC					.38	.15
Lent & Lopez (1996) Site 2, EAT					.51	.22
Camp & Chartrand (1962)					.37	.12
Gati Sb	2.02	.78	0	5		
Young et al. (1998)					2.49	1.16
Camp & Chartrand (1962)					2.45	1.18
Icahan M	10.00	7.82	0	25		
Young et al. (1998)					17.22	8.51
Camp & Chartrand (1962)					9.51	5.76
Ranked comparison index	4.10	2.54	0	10		
Young et al. (1998)					5.82	2.52
Camp & Chartrand (1962)					4.65	1.97
Compatibility index	3.39	1.07	0	8		
Young et al. (1998)					4.68	1.77
Camp & Chartrand (1962)					3.87	1.33
Three-level index	.45	.63	0	2		
Young et al. (1998)					1.01	.74
Camp & Chartrand (1962)					.94	.61
Two-letter agreement	1.67	.60	1	3		
Young et al. (1998)					2.11	.63
Camp & Chartrand (1962)					1.64	.54
First-letter based on hexagon	2.50	.96	1	4		
Young et al. (1998)					3.05	1.03
Camp & Chartrand (1962)					2.25	.66
Dichotomous first letter	.17	.37	0	1		
Young et al. (1998)					.46	.49
Camp & Chartrand (1962)					.06	.23

Note. Lent and Lopez (1996) calculated two measures of congruence for each of two different job sites using two different procedures for obtaining work environment codes. One procedure used the *Dictionary of Holland Occupational Codes* (Gottfredson, Holland, & Ogawa, 1982) and the second the Environmental Assessment Technique (Holland, 1985).

it is likely that the strength of the relation between hexagonal congruence and vocational outcome was overestimated in those earlier investigations in which a procedure such as the median split was used. This possible confound has been overlooked in prior considerations of this issue.

Meta-analysis flaws. Rounds et al. (2000) dismissed the previous meta-analyses, arguing that it was inappropriate for Assouline and Meir (1987) to include studies using high school and college samples and for Tranberg et al. (1993) to exclude unpublished studies. They argued that a more informative meta-analysis would be achieved by including all studies and coding for methodological weaknesses. I agree that it is preferable to include all relevant research, use dummy codes to distinguish among the studies on relevant dimensions (e.g., attributes of the sample, the instruments, and the analyses), and evaluate the effects of those factors on the effect size. Although that approach requires the expenditure of resources to code studies that are immedicably flawed, I have argued elsewhere that good science is not necessarily quick science (Tinsley, 1992).

Nevertheless, I found the arguments of Rounds et al. (2000) to be unpersuasive for three reasons. First, Tranberg et al. (1993) did perform secondary analyses to evaluate the possibility that gender, the congruence measure used and the quality of the instruments used moderated the congruence-satisfaction relation. They found that the better the quality of the study, the weaker the support for the hexagonal congruence hypothesis. Second, Rounds et al.'s (2000) argument that Assouline and Meir (1987) should have eliminated some studies from the analysis is inconsistent; those features should have been coded and the effects of variations on those attributes investigated, as was done by Tranberg et al. (1993).

Most telling, the logical implication of Rounds et al.'s (2000) criticisms is that the meta-analyses actually overstate the strength of support for Holland's theory. The typical criticism of meta-analyses in which unpublished studies have been excluded is that they overstate the effect size because most unpublished studies are unpublished because they are seriously flawed or failed to find significant results. Rosenthal (1979) referred to this as the "file drawer" problem, noting that studies supporting the null hypothesis of no significant results are more like to be buried away in file drawers. Kraemer and Andrews (1982, p. 405) argued that the published literature is biased toward positive findings because "nonsignificant findings are generally unpublishable even when they are replications of earlier studies reporting significant results." Assouline and Meir (1987) and Tranberg et al. (1993) found consistent effect sizes in the range of .20 which were not significantly greater than zero. It now seems likely that even these nonsignificant values may overstate the strength of support for Holland's theory.

CIRCUMPLEX MODEL

Who Proposed the Circumplex Model

Rounds et al. (2000) raise the issue of who proposed the circumplex model (i.e., the assumption of an equilateral hexagon). They argue that Hogan, Fouad,

Cudeck, Hansen, Rounds, Tracey, Hubert, and Prediger, among others, have "extended Holland's RIASEC order model to a equilateral hexagon" (p. 209). Rounds et al. (2000) claim that "To ascribe to Holland an equilateral hexagon is just false" (p. 208) and they repeated a quote from Holland that I had quoted in my original article (see p. 210) as partial support for their position. On the one hand this appears to be a rather trivial issue. If so many leading scholars have treated the hexagon as an equilateral hexagon, as Rounds et al. have asserted, then precisely who should have the credit for suggesting an invalid extension of Holland's model is not particularly important. The critical issue is that congruence indices based on the equilateral hexagon do not predict important vocational outcomes.

Nevertheless, it is reasonable to consider just how so many imminent scholars could have made such an error. In his most recent position on this issue, Holland (1997) wrote, "*The relationships* within and between personality types or environments *can be ordered according to a hexagonal model in which* the *distances* among the types or environments *are inversely proportional to the theoretical relationships between them*" (p. 5, italics added). He illustrated this statement with a figure depicting the now classic equilateral hexagon (p. 6) and stated, "This spatial relation provides *explicit definitions of* both consistency . . . and *congruence of person and environment* (four levels)" (p. 5, italics added). Given that this is Holland's statement of his theoretical position following decades of debate in the professional literature about whether he postulates an equilateral hexagon, it is reasonable to expect that his wording conveys his precise meaning. Holland did not include a qualifier such as "approximately"; he stated flatly that the distances are "inversely proportional" to the theoretical relationships. Furthermore, I am not aware of any instance in which Holland has used an "ugly polygon" (Holland, 1997, p. 159), as he calls it, to depict his model. Finally, only an equilateral hexagon would yield the four levels of congruence explicitly postulated by Holland; a misshapen polygon would yield more than four levels of congruence. These considerations lead to the inescapable conclusion that Holland has postulated an equilateral hexagon.

Advantages of Circumplex Model

Rounds et al. (2000) wrote, "Tinsley seems to believe there are advantages to the circumplex." That is close to the opposite of my position. Prediger (2000) concluded with a quotation from Holland: "The key characteristics of the hexagonal model are the RIASEC order and the implied distances or relationships among the types" (Holland, 1997, p. 159, italics in original). Again, I disagree. I think that the hexagon is a heuristic device that may well have some usefulness in simplifying the complexity of the world of work for lay persons but it is not useful as an operational system for distinguishing among individuals or occupations. It is the dimensions underlying the data that are important, not the heuristic model (i.e., the hexagon) that has been imposed on these dimensions. Tracey and Rounds (1996) have shown that a variety of alternative heuristic

models could be superimposed on these latent dimensions, and I have noted that procedures similar to those used by Tracey and Rounds could be used to create "a square, rectangular or circular model" (Tinsley, 1996, p. 1). Guilford's Structure of Intellect and Cattell's data box are useful three dimensional models (both cubes) that, thankfully, did not stimulate a lot of research on whether these models are "really cubes." In each of these instances the factors of critical importance are the latent dimensions underlying the data and not the heuristic model.

The data/ideas and things/people dimensions identified by Prediger (1982) as underlying Holland's hexagon have a long history in vocational psychology. Earlier versions of these dimensions were evident in the Dictionary of Occupational Titles (U.S. Department of Labor, 1977) and the things/people dimension was explicitly incorporated into Anne Roe's (1956) occupational classification system. I view these dimensions as critical to our understanding of the distinctions among people and occupational environments. Labeling individuals (or occupational environments) who are at the people end of the continuum as "social" and those at the things end of the continuum as "realistic" adds nothing to our understanding of the differences among people and occupations. Furthermore, the hexagon draws attention to the periphery of the circle that can be drawn around these dimensions; those extreme points on the underlying dimensions are not descriptive of any occupations and of only the most rare and unusual individual. Instead, all fall somewhere more toward the center of the two-dimensional display where their individuality is described most accurately by their status on the underlying dimensions. It is my view that the circumplex has little promise as a conceptual model for guiding research on the differences between individuals, their occupational environments, and their interactions.

PROCEDURES FOR TESTING HOLLAND'S MODEL

Randomization Test

Rounds et al. (2000) acknowledge that, "The randomization test is only intended for the 'non-zero evaluation' or the rejection of nullity . . . ." (p. 211). In plain language, the randomization test just shows that the data are not random. That is precisely the point I was making when I criticized, "the prevailing practice of testing the null hypothesis and making dichotomous pronouncements of statistical significance that ignore fundamental aspects of the data" (Tinsley, 2000). Performing the randomization test ritual to determine whether a matrix has significantly more structure than a random matrix addresses a scientifically uninteresting question that reveals nothing about the practical significance of the results. A correlation of .01 could be significantly greater than zero if the sample were large enough but it is challenging to imagine a situation under which a relation of that strength would be judged to be important.

Rounds et al. (2000) and Tracey et al. (2000) disagree with my conclusion that the randomization test does not provide a useful test of Holland's circular order

model. Tracey et al. (2000) advance the even stronger claim that "the specification of predictions is so complete . . . that testing for order relations approximates establishing constraints on equality." However, neither team addressed my criticisms that (a) the test is biased toward confirmation of the circular order hypothesis because it ignores ties, which represent clear disconfirmations of Holland's model; (b) testing the hypothesis that the ordering of the correlations in a RIASEC matrix is not "completely random" does not contribute significantly to our understanding of the meaning of the data; and (c) the finding that the model fits the observed correlation matrix better than most randomly generated correlation matrices provides no information about whether an alternative model would fit the data better than the hypothesized model.

Since Rounds and Tracey are internationally acclaimed scholars whose opinions deserve serious consideration, I entertained the thought that perhaps I had failed to understand some critical element of the logic underlying the randomization test. Therefore, I performed an empirical evaluation of the ability of the randomization test to reject matrices that are not completely random, but that also are inconsistent with Holland's theory. As I explained previously, (Tinsley, 2000, 147–179), the circular-order hypothesis can be stated in the form of the following predictions:

1. The pairwise correlations between adjacent types on Holland's hexagon (i.e., RI, IA, AS, SE, EC, and CR) will be larger than all other correlations in a RIASEC correlation matrix.
2. The correlations between alternate types (i.e., two places apart on the hexagon; RA, IS, AE, SC, ER, and CI) will be larger than the correlations between opposite types (i.e., types three places apart on the hexagon; RS, IE, and AC).

I evaluated two matrices in which the relative size of the correlations among the adjacent, alternate, and opposite types were in the direction predicted by the circular-order hypothesis, but in which no discriminant validity was present. In one the correlations were 1.00, .99, and .98 among all pairs of the adjacent, alternate, and opposite types, respectively. In the other matrix the three values were .02, .01, and .00. In both instances the randomization test indicated that all 72 of the predictions generated by the circular order hypothesis were met, that the correspondence index was 1.0, and that the p value was .0167. Therefore, the randomization test is insensitive to multicollinearity and to the almost complete absence of common variance and discriminant validity, so long as the matrix is not random.

Then I evaluated the sensitivity of the test to ties among the adjacent, alternate, and opposite types. First, I evaluated a matrix in which all of the correlations were 1.00 except those between the opposite types, which were .99. The test indicated that 36 of the 72 predictions had been met, the correspondence index was .50, and the p value was .0667. Although alarmingly close to "statistically significant," the test did return a verdict of "not significant." Next, I changed the

correlation between one pair of alternate types (R and E) to .99 and the randomization test indicated that 39 predictions had been met, the correspondence index was .54, and the *p* value was .033. This reveals that the randomization test is insensitive to ties.

In short, the randomization test is sensitive only to the presence of nonrandom circularity in the data. As such, it was not useful in identifying matrices that were clearly inconsistent with Holland's theory. The conclusion is inescapable; the randomization test does not provide an adequate test of Holland's theory.

MDS

I cited results reported by Fouad, Harmon, and Borgen (1997) as an illustration that the randomization test did not provide a valid test of Holland's theory (Tinsley, 2000). Fouad et al. found that a nonmetric multidimensional scaling analysis (MDS) revealed a triangular structure, whereas the randomization test indicated that the RIASEC ordering was significant. (Given the results cited above, this should no longer be surprising.) Tracey et al. (2000) challenged my interpretation, arguing that the Fouad et al. (1997) solution is degenerate. Had Fouad et al. (1997) performed a metric MDS analysis they would have "found not a triangular structure but a clear circular ordering of the variables supporting the results of the randomization test" (Tracey et al., 2000, p. 219).

This disagreement provides an excellent illustration of my point that procedures such as confirmatory factor analysis (CFA), structural equation modeling (SEM) and MDS lack an objective interpretation. Consequently their interpretation is influenced heavily by the investigator's preconceived notions, much like the interpretation of projective techniques. I consider this example in detail to highlight some of the points at which subjectivity and (often unrealized) assumptions shape the interpretation. Since Tracey et al. (2000) cited Kruskal and Wish (1978) as their authorities on MDS, I also limit my comments to that source so that the problems I identify cannot be discounted as mere differences of opinion among experts.

Degeneracy. First, there is the issue of degeneracy. Sounds awful! Degeneracy is a phenomenon that occurs primarily when nonmetric scaling is used in which the objects submitted to MDS "have a natural clustering, usually of three or less clusters, and the dissimilarities *between* objects in different clusters are all (or almost all) larger that the dissimilarities *within* each cluster" (Kruskal & Wish, 1978, p. 30, italics in original). When this occurs, almost all of the points for the objects in a single cluster will converge to a single location and stress will converge toward zero. However, theoretically, Holland's six RIASEC types should be equally distant from each other so there should be no natural clusters, and degeneracy should not occur. Thus, the presence of the clusters that Tracey et al. (2000) interpret as a sign of degeneracy could also be interpreted as a sign that the data are not consistent with Holland's theory.

The Tracey et al. (2000) interpretation is not without some basis, however. Kruskal and Wish (1978) assert that a degenerate solution should not be taken as

good fit because it violates the tacit assumption that "points should only lie in the same position if the corresponding objects function as virtually identical" (p. 30). Therefore, if your are willing to assume that the RIASEC types are all unique (i.e., to assume that Holland's theory is valid), the Fouad et al. (1997) solution can be interpreted as degenerate. If you adopt the theory neutral assumption that the relation among the RIASEC types is unknown, the Fouad et al. (1997) solution can be interpreted as indicating that some of the RIASEC types are very similar and that Holland's theory is not supported. There is no theory neutral way to resolve this conundrum using MDS. We cannot say whether the Fouad et al. (1997) solution or the Tracey et al. (2000) solution is correct without making a biasing assumption.

Hypothesis testing. Am I overstating the case? Not really. Kruskal and Wish (1978) explain that MDS is almost always used as a descriptive rather than as a hypothesis testing procedure. For example, there are no good statistical methods for determining the correct or true dimensionality of a set of data; the decision as to the dimensionality to use for a given set of data is as much a subjective as a statistical question. Kruskal and Wish argue that interpretability and ease of use are more important than the "correct" number of dimensions.

Interpreting MDS results. Tracey et al. (2000) reported a stress value of .14 that they interpreted as acceptable for metric MDS. This further illustrates the subjectivity that pervades the interpretation of MDS results. Kruskal and Wish (1978) advise that "one is reluctant to suggest a dimensionality (other than one) that yields stress much above 0.10" (p. 56). Davison and Sireci (2000) likewise advise users that stress values above .10 generally should not be accepted. In fairness to Tracey et al. (2000), however, it is important to recognize that the recommended stress values are purely arbitrary heuristics and they apply most directly to nonmetric MDS. Neither Kruskal and Wish (1978) nor Davison and Sireci (2000) offer a heuristic for interpreting metric MDS. Therefore, Tracey et al. (2000), like all other users of MDS, CFA, SEM, and similar procedures, are forced (and allowed) to pretty much set their own standards for determining what is significant.

Kruskal and Wish suggest an alternative approach to interpreting MDS results that might have led Tracey et al. (2000) to a different conclusion. They argue that "inferences should not be drawn that would change if several points were relocated by about 10% of the diameter of the configuration" (Kruskal & Wish, 1978, p. 59). I applied this heuristic to the metric MDS reported by Tracey et al. (2000) and found that moving R and I toward each other and C and S toward E resulted in a configuration in which R and I occupy essentially the same point, C and E occupy essentially the same point, and S is close to the CE point (i.e., the solution reported by Fouad et al.).

My recommendation is that researchers always cross-validate their MDS results (Tinsley & Brown, 2000), a position also taken by Kruskal and Wish (1978).

Compass Test

Two concerns led me to propose the compass test: my belief that the randomization test and MDS do not provide discriminating tests of Holland's circumplex hypothesis, and the absence of descriptive information to accompany these tests. Tracey et al. (2000) criticize the compass test as revealing only equal distances from the origin but not around the circle but that is even more true of the randomization test that they recommend. I see the compass test as the analog of descriptive statistics, designed to fill the need for a simple, easily interpretable visual depiction of the results of the type widely advocated by respected authorities such as Behrens (1997), Hallahan and Rosenthal (2000), and Tukey (1969). Tracey et al. argue that the adequacy of the plot depends on the adequacy with which it represents the data. I agree and join them in advocating that users of the compass test report the variance accounted for by the dimensions upon which the plot is based. It is now widely recognized that we can obtain significant *F* ratios and *t* scores even if the difference between means is trivial (Cohen, 1994; Estes, 1997; Gonzalez, 1994). In an analogous manner, I predict that experience will show that the randomization test and MDS often indicate support for Holland's model, but closer inspection using the compass test will reveal that those results are trivial.

ADVANCES IN PERSON–ENVIRONMENT FIT THEORY AND RESEARCH

Dawis (2000), Gati (2000), and Hesketh (2000) make important points about the extension of the P–E fit model and needed programs of research. Dawis provides an interesting conceptualization in which gaining an understanding of the person–environment interaction (PEI) is depicted as the all-encompassing objective of psychology. He interprets interests as expressed self-disclosures about the person and abilities as inferred information about the person. Environments become an aggregate of the people who form them. Both persons and environments have requirements for self and for other, both have capabilities, and the defining task of psychology is the identification and measurement of the critical perceptual, cognitive, affective, and behavioral attributes of persons, environments and their interactions.

Improving the P–E Matching Process

Dawis (2000) and Gati (2000) advocate the use of an expanded perspective in thinking about the PEI. According to Dawis (2000), the study of P–E fit should expand beyond its univariate focus on the relation between individuals and environments to a genuinely multivariate consideration of the relations of many to one (as in multiple regression), one to many (e.g., correlation of an individual with a component or factor) and many to many (e.g., canonical analysis). In contrast, much of what Gati (2000) suggests is reminiscent of the mid-century program of research that was instrumental in the development of the Theory of

Work Adjustment and the instrumentation used to operationalize the theory. Given the technological advances that have occurred in the last two decades, I view Dawis' and Gati's calls for an increased focus on to the individual, the specific environment, and their relations as highlighting where our research and conceptual attention should be focused for the next two decades. The emphasis they have placed on these issues is consistent with the positions taken by Hesketh (2000), Prediger (2000), and Tracey et al. (2000).

Gati (2000) suggests that it is time to move beyond our limited focus on the normatively desirable characteristics of environments to an expanded focus on specific characteristics of individual environments, which he calls work-related aspects. Many of the "normative" dimensions to which he refers were identified in research conducted by the team of researchers who staffed the Work Adjustment Project at the University of Minnesota in the 1960s and appear in instrumentation designed to measure person–environment fit. The Minnesota Importance Questionnaire (MIQ; Rounds, Henly, Dawis, Lofquist, & Weiss, 1981) and the Minnesota Job Description Questionnaire (MJDQ: Borgen, Weiss, Tinsley, Dawis, & Lofquist, 1968) were based on the findings of a program of research begun in the 1960s that investigated the importance of potential dimensions and selected those dimensions having the greatest relevance to distinguishing work adjustment. In Gati's terminology, these dimensions represent the modal (most critical or generally most important) work-related aspects. As such, they have had an important influence on the conceptualization of P–E fit for close to half a century.

A problem with P–E fit theory noted by several of the writers is that operational P–E fit models typically deal only with imaginary global environments (e.g., all elementary school teacher job settings), whereas the actual individual occupies an actual (specific or micro-) environment that may differ from the imaginary global environment in important respects. Technological and conceptual advances now make it possible to eliminate this disparity and match individuals to microenvironments. Advances in computer technology now make it feasible to store immense amounts of information on desktop computers. Advances in item response theory software and computer adaptive testing procedures suggest that it is now realistic to tailor the choice process to the individual and the microenvironment. However, application of these advances to issues of vocational choice and adjustment will require several changes in our present practices.

First, as Dawis (2000), Gati (2000), and Hesketh (2000) have noted, we must expand our focus beyond one to one relations in which only the modal dimensions that apply to all occupations receive attention. Instead, foundational research must be performed and assessment procedures must be developed that will permit the identification and quantification of those specific dimension that are uniquely relevant to a particular occupational environment. The approach used in developing the Occupational Aptitude Patterns (1979) might serve as a model for this effort.

Second, a more complex model than that offered by Holland's (1997) theory will be necessary. Holland's familiar RIASEC dimensions might still be used as a simple, understandable translator that strips away the underlying complexity and communicates to individuals in understandable terms, but the complexity anticipated by Dawis (2000), Gati (2000), and Hesketh (2000) will require a much more technically sophisticated model.

Third, the development of microtheories (see Hesketh & Gardner, 1993) will be necessary to model the complex and varied interactions that occur between persons and microenvironments. The use of a single function to model all of the important dimensions of the P–E interaction results in the loss of critical information about the microenvironments, the individual, and the work adjustment process. I interpret Gati's (2000) observation that the effects of P–E discrepancies depend largely on the unique reactions of the individual as reflecting an agreement with this premise.

Present Status Model

Gati (2000) and Tracey et al. (2000) discounted the present status model, and both seem inclined to dismiss it out of hand without investigation. Their comments seem to indicate a misunderstanding of the present status model that I attribute to my inadequate description of the model. Hesketh (2000) provided a clearer description and an informative elaboration of the present status model.

The general idea underlying the present status model is that some work environments are more broadly appealing to all individuals than other environments. Think of it as environmental charisma: some work environments have it, some do not. If you doubt that it exists, think of a prison work environment which undoubtedly is low in charisma. Enriched environments produce higher satisfaction, whereas environments devoid of amenities produce lower levels of satisfaction. Likewise, demanding work environments produce higher performance than laissez-faire work environments. The present status model does not suggest that everyone will be equally happy or productive in a given environment, but rather that the environment in and of itself plays an influential role in determining work attitudes and work behaviors, regardless of the personality of the person who is present in the environment.

The present status model is a plausible model that has been completely ignored in research on P–E fit. When compared to the P–E fit model, two critical tests are apparent. First, if you factored out the influence of personality type would information about the present work environment (i.e., the present status) predict important vocational outcomes? If so, then the qualities of the environment influence vocational outcomes independently of the personality of the individual. Second, if you factored out the influence of present status would information about P–E fit predict important vocational outcomes? If not, then the P–E fit model can be discarded in favor of the more parsimonious present status model. Although there seems to be some reluctance to consider the present status model seriously, I think that the focus on the local environment that will be necessary

to operationalize a microenvironment approach will stimulate a greater appreciation of the present status model as one potential explanation of vocational behavior.

Work Adjustment

Tracey et al. (2000) cite the static assessment of personality types as an important methodological concern, and Gati (2000) and Hesketh (2000) draw attention to the critical differences between choice and adjustment. Vocational choice does not equal vocational adjustment. It is for this reason that I called for longitudinal studies of the dynamic interaction between the person and the environment. Lofquist and Dawis (1969; Dawis & Lofquist, 1976, 1978) theorized over 30 years ago that the individual operates on and changes the environment while simultaneously the environment operates on and changes the individual. Both individuals and environments are relatively stable, but neither is static; both have the potential to change and only longitudinal research can open the process of this dynamic interaction to scrutiny.

I discussed this issue at some length (Tinsley, 2000), so I will not reiterate those points here. The personality styles and modes of adjustment postulated by Dawis and Lofquist (1976, 1978) provide a flexible model for studying the PEI that can accommodate the findings of mainstream personality research (e.g., the facets of the Big Five personality theory) and less formal observations (e. g., Gati's comments about the importance of readiness to compromise). Brown (1993), Hesketh (1993), Tenopyr (1993), and D. Tinsley (1993) have identified research issues pertaining to Dawis and Lofquist's personality styles and modes of adjustment that deserve attention.

Tiedeman and O'Hara (1963) proposed another long-neglected theory of the occupational decision-making process that describes both the process of occupational choice and the interaction between the individual and the occupation that occurs during the implementation of that decision. According to Tiedeman and O'Hara, implementation proceeds from induction (during which the individual enters the occupational environment and assimilates the occupational culture using primarily a reactive adjustment mode), through reformation (during which the individual and environment work to shape each other using primarily an active adjustment mode) to integration (during which the individual and environment experience tolerable discorrespondence). Harren (1979) extended this theory to explain the adjustment of students to universities. Promising findings were forthcoming from an early program of research (see Harren, Kass, Tinsley, & Moreland, 1978, 1979; Moreland, Harren, Krimsky-Montague, & Tinsley, 1979; Tinsley, Kass, Moreland, & Harren, 1983). Since that time this model has been ignored (see Blustein, Pauling, DeMania, & Faye, 1994, for a notable exception) sometimes at a loss of conceptual clarity (see Feij, Whitely, Peiro, & Taris, 1995, and the comments by Blustein, 1995, and Hesketh, 1995). The conceptual clarity of the emerging body of research on occupational socialization

would be greatly enhanced by the use of the models of Dawis and Lofquist (1976, 1978) and Tiedeman and O'Hara (1963).

CONCLUSION

Dawis (2000), Gati (2000), Hesketh (2000), Prediger (2000), Rounds et al. (2000), and Tracey et al. (2000) have raised numerous additional issues that deserve attention, but space and time limitations preclude my consideration of these points at this time. Prediger's (2000) raised the issue of the distinction between intrinsic and extrinsic satisfaction, a distinction my research on leisure has shown to be of great importance. Tracey et al. (2000) question whether P–E fit is optimal, a provocative question that so far as I know has not yet been considered? In my (perhaps biased) opinion, the exchange of views in this series of papers provides a rich intellectual resource. The community of scholars concerned with vocational behavior are now confronted with a choice. The easy path is to continue to focus on Holland's (1997) structural model, just as we have for the past two decades. The more challenging task is (a) to investigate the role of personality style and adjustment mode in the PEI, (b) to develop and test microtheories about the salient dimensions along which the PEI occurs, (c) to replace our focus on global environments with a more detailed description of microenvironments, (d) to undertake longitudinal research, (e) to replace our dependence on congruence measures with an informed use of hit rate analysis, and (f) to cross-validate our findings. If we chose the latter approach I predict that the next two decades will come to be viewed as another golden era of theory development, instrument development, and vocational research.

REFERENCES

Assouline, M., & Meir, E. (1987). Meta-analysis of the relationship between congruence and well-being measures. *Journal of Vocational Behavior,* **31,** 319–332.

Behrens, J. T. (1997). Principles and procedures of exploratory data analysis. *Psychological Methods,* **2,** 131–160.

Blustein, D. L. (1995). Toward a contextual perspective of the school-to-work transition: A reaction to Feij et al. *Journal of Vocational Behavior,* **46,** 257–265.

Blustein, D. L., Pauling, M. L., DeMania, M. E., & Faye, M. (1994). Relation between exploratory and choice factors and decisional progress. *Journal of Vocational Behavior,* **44,** 75–90.

Borgen, F. H., Weiss, D. J., Tinsley, H. E. A., Dawis, R. V., & Lofquist, L. H. (1968). *The measurement of occupational reinforcer patterns.* (Minnesota Studies in Vocational Rehabilitation No. 25.) Minneapolis, MN: University of Minnesota, Department of Psychology.

Brown, S. D. (1993). Contemporary psychological science and the Theory of Work Adjustment: A proposal for integration and a favor returned. *Journal of Vocational Behavior,* **43,** 58–66.

Brown, S. D., & Gore, P. A., Jr. (1994). An evaluation of interest congruence indices: Distribution characteristics and measurement properties. *Journal of Vocational Behavior,* **45,** 310–327.

Camp, C. C., & Chartrand, J. M. (1992). A comparison and evaluation of interest congruence indices. *Journal of Vocational Behavior,* **41,** 162–182.

Cohen, J. (1994). The earth is round ($p < .05$). *American Psychologist,* **49,** 997–1003.

Davison, M. L., & Sireci, S. G. (2000). Multidimensional scaling. In H. E. A. Tinsley & S. D. Brown (Eds.), *Handbook of applied multivariate statistics and mathematical modeling.* (pp. 325–352). San Diego, CA: Academic Press.

Dawis, R. V. (2000). P–E fit as paradigm: Comment on Tinsley (2000). *Journal of Vocational Behavior,* **56,** 180–183.

Dawis, R. V., & Lofquist, L. H. (1976). Personality style and the process of work adjustment. *Journal of Counseling Psychology,* **23,** 55–59.

Dawis, R. V., & Lofquist, L. H. (1978). A note on the dynamics of work adjustment. *Journal of Vocational Behavior,* **12,** 76–79.

Estes, W. K. (1997). Significance testing in psychological research: Some persisting issues. *Psychological Sciences,* **8,** 18–20.

Feij, J. A., Whitely, W. T., Peiro, J. M., & Taris, T. W. (1995). The development of career-enhancing strategies and content innovation: A longitudinal study of new workers. *Journal of Vocational Behavior,* **46,** 231–256.

Fouad, N. A., Harmon, L. W., & Borgen, F. H. (1997). Structure of interests in employed male and female members of U.S. Racial–ethnic minority and non-minority groups. *Journal of Counseling Psychology,* **44,** 339–345.

Gati, I. (1998). Using career-related aspects to elicit preferences and characterize occupations for a better person-environment fit. *Journal of Vocational Behavior,* **52,** 343–356.

Gati, I. (2000). Pitfalls of congruence research: A comment on Tinsley's "The congruence myth." *Journal of Vocational Behavior,* **56,** 184–189.

Gati, I., Fassa, N., & Mayer, Y. (1998). An aspect-based approach to person-environment fit: A comparison between the aspect structure derived from characteristics of occupations and that derived from counselees' preferences. *Journal of Vocational Behavior,* **53,** 28–43.

Gonzalez, R. (1994). The statistics ritual in psychological research. *Psychological Science,* **6,** 325–328.

Harmon, L. W., Hansen, J. C., Borgen, F. H., & Hammer, A. L. (1994). *Strong interest inventory: Applications and technical guide.* Palo Alto, CA: Consulting Psychologists Press.

Hallahan, M., & Rosenthal, R. (2000). Interpreting and reporting results. In H. E. A. Tinsley & S. D. Brown (Eds.), *Handbook of applied multivariate statistics and mathematical modeling* (pp. 125–149). San Diego, CA: Academic Press.

Harren, V. A. (1979). A model of career decision making for college students. *Journal of Vocational Behavior,* **14,** 119–133.

Harren, V. A., Kass, R. A., Tinsley, H. E. A., & Moreland, J. R. (1978). The influence of sex roles and cognitive styles on career decision making. *Journal of Counseling Psychology,* **25,** 390–398.

Harren, V. A., Kass, R. A., Tinsley, H. E. A., & Moreland, J. R. (1979). The influence of gender, sex attitudes and cognitive complexity on gender-dominant career choices. *Journal of Counseling Psychology,* **26,** 227–234.

Hesketh, B. (1993). Toward a better adjusted Theory of work adjustment. *Journal of Vocational Behavior,* **43,** 75–83.

Hesketh, B. (1995). Personality and adjustment styles: A theory of work adjustment approach to career enhancing strategies. *Journal of Vocational Behavior,* **46,** 274–282.

Hesketh, B. (2000). The next millennium of "fit" research: Comments on "The congruence myth: An analysis of the efficacy of the person–environment fit model" by H. E. A. Tinsley. *Journal of Vocational Behavior,* **56,** 190–196.

Hesketh, B., & Gardner, D. (1993). Person–environment fit models: A reconceptualization and empirical test. *Journal of Vocational Behavior,* **42,** 315–332.

Hoeglund, T., & Hansen, J. C. (1999). Holland-style measures of congruence: Are complex indices more effective predictors of satisfaction? *Journal of Vocational Behavior,* **54,** 471–482.

Holland, J. L. (1985a). *The self-directed search professional manual—1985 edition.* Odessa, FL: Psychological Assessment Resources.

Holland, J. L. (1997). *Making vocational choices: A theory of vocational personalities and work environments (3rd ed.).* Odessa, FL: Psychological Assessment Resources, Inc.

Holland, J. L., & Gottfredson, G. D. (1992). Studies of the hexagonal model: An evaluation (or, the perils of stalking the perfect hexagon). *Journal of Vocational Behavior,* **40,** 159–170.

Kraemer, H. C., & Andrews, G. (1982). A nonparametric technique for meta-analysis effect size calculation. *Psychological Bulletin,* **91,** 404–412.

Kruskal, J. B., & Wish, M. (1978). *Multidimensional scaling.* Newbury Park, CA: Sage.

Lent, E. B., & Lopez, F. G. (1996). Congruence from many angles: Relations of multiple congruence indices to job satisfaction among adult workers. *Journal of Vocational Behavior,* **49,** 24–37.

Moreland, J. R., Harren, V. A., Krimsky-Montague, E., & Tinsley, H. E. A. (1979). Sex role self-concept and career decision making. *Journal of Counseling Psychology,* **26,** 329–336.

Oleski, D., & Subich, L. M. (1996). Congruence and career change in employed adults. *Journal of Vocational Behavior,* **49,** 221–229.

Rosenthal, R. (1979). The 'file drawer' problem and tolerance for null results. *Psychological Bulletin,* **86,** 638–641.

Prediger, D. J. (1982). Dimensions underlying Holland's hexagon: Missing link between interests and occupations. *Journal of Vocational Behavior,* **21,** 259–287.

Prediger, D. J. (2000). Holland's hexagon is alive and well—though somewhat out of shape: Response to Tinsley. *Journal of Vocational Behavior,* **56,** 197–204.

Roe, A. (1956). *The psychology of occupations.* New York: Wiley.

Rounds, J. B., Henly, G. A., Dawis, R. V., Lofquist, L. H., & Weiss, D. J. (1981). *Manual for the Minnesota Importance Questionnaire: A measure of needs and values.* Minneapolis, MN: Vocational Psychology Research, Department of Psychology, University of Minnesota.

Rounds, J., McKenna, M. C., Hubert, L., & Day, S. X. (2000). Tinsley on Holland: A misshapen argument. *Journal of Vocational Behavior,* **56,** 205–215.

Tenopyr, M. L. (1993). Construct validation needs in vocational behavior theories. *Journal of Vocational Behavior,* **43,** 84–89.

Tiedeman, D. V., & O'Hara, R. P. (1963). *Career development: Choice and adjustment.* New York: College Entrance Examination Board.

Tinsley, D. J. (1993). Extensions, elaborations, and construct validation of the Theory of Work Adjustment. *Journal of Vocational Behavior,* **43,** 67–74.

Tinsley, H. E. A. (1992). Am I the fifth horseman of the Apocalypse? Comment on Galassi, Crase, Martin, James and Wallace (1992) and comments on research concerning expectations about counseling. *Journal of Counseling Psychology,* **39,** 59–65.

Tinsley, H. E. A. (1996). The structure of vocational interests. *Journal of Vocational Behavior,* **48,** 1–2.

Tinsley, H. E. A. (2000). The congruence myth: An analysis of the efficacy of the person–environment fit model. *Journal of Vocational Behavior,* **56,** 147–179.

Tinsley, H. E. A., & Brown, S. D. (2000). Multivariate statistics and mathematical modeling. In H. E. A. Tinsley & S. D. Brown (Eds.), *Handbook of applied multivariate statistics and mathematical modeling* (pp. 3–36). San Diego, CA: Academic Press.

Tinsley, H. E. A., & Chu, S. (1999). Research on test and interest inventory interpretation outcomes. *Vocational interests: Meaning, measurement, and counseling use.* Palo Alto, CA: Davies-Black.

Tinsley, H. E. A., Kass, R. A., Moreland, J. R., & Harren, V. A. (1983). A longitudinal study of occupational decision making among female college students. *Vocational Guidance Quarterly,* **32,** 160–167.

Tokar, D. M., & Subich, L. M. (1997). Relative contributions of congruence and personality dimensions to job satisfaction. *Journal of Vocational Behavior,* **50,** 482–491.

Tracey, T. J. G., Darcy, M., & Kovalski, T. M. (2000). A closer look at person–environment fit. *Journal of Vocational Behavior,* **56,** 216–224.

Tracey, T. J., & Rounds, J. (1996). The spherical representation of vocational interests. *Journal of Vocational Behavior,* **48,** 3–41.

Tranberg, M., Slane, S., & Ekeberg, S. E. (1993). The relation between interest congruence and satisfaction: A meta analysis. *Journal of Vocational Behavior,* **42,** 253–264.

Tukey, J. W. (1969). Analyzing data: Sanctification or detective work? *American Psychologist*, **24,** 83–91.

U. S. Department of Labor (1977). *Dictionary of occupational titles (fourth edition).* Washington, D.C.: U.S. Government Printing Office.

U. S. Department of Labor (1979). *Manual for the USES General Aptitude Test Battery, Section II: Occupational aptitude pattern structure.* Washington, D.C.: U.S. Government Printing Office.

Young, G., Tokar, D. M., & Subich, L. M. (1998). Congruence revisited: Do 11 indices differentially predict job satisfaction and is the relation moderated by person and situation variables? *Journal of Vocational Behavior*, **42,** 208–223.

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