

The Future of Person-Organization Fit Research: Problems, Opportunities, and a Few Suggestions

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This is both an exciting – and somewhat troubling – time to be doing fit research. It is exciting because one sees a literature that has established a solid base in organizational psychology, and is both deepening its roots and branching out into new directions. It is troubling because some of the contributions in the past decade or so have suggested a certain *methodological stalemate* (Larson & Csikszentmihalyi, 1983) in fit research. As with any area of academic study, researchers will differ in whether they see various problems as serious ones, and, indeed, whether they see the purported problems as problems at all. In this paper, I offer my own view discussing 10 issues, quandaries, questions, and future directions that I believe deserve attention by fit researchers.

Before beginning, I wish to express thanks for contributors to a book I edited. Their views have informed mine, though of course what is to follow should not be seen as necessarily reflecting their views. In particular, I want to acknowledge: Cheri Ostroff (co-editor of the book), Amy Kristof-Brown, Jeff Edwards, Dan Cable, Dave Harrison, Scott DeRue, John Hollenbeck, Barry Gerhart, John Kammeyer-Mueller, Tory Higgins, Karen Jansen, and Anne Tsui.

One final note: I make no allusions to the comprehensiveness of my polemical comments. These are merely 10 issues that are on my mind as I think about fit. To the extent that papers can have moods, I intentionally start out dark – focusing on problems and challenges – and end on brighter notes.

Issue #1: Terminology Confusion

As even a causal reader will have noted, there is inconsistency in labels to describe fit – person-environment fit, person-organization fit, person-job fit, person-group fit, person-individual fit, dyadic fit, and so forth. Some use the broad term person-environment fit, others use person-organization fit as an umbrella term, and still others distinguish person-organization from other foci like person-job, person-group, person-person, and so on. Alternatively, one could argue that person-organization fit is a more specific instantiation of the more general person-environment fit. Fifteen years ago Jerry Ferris and I complained that the literature on fit was confusing and plagued by conceptual ambiguities (Judge & Ferris, 1992). I am not sure the situation has improved. We are swimming in terms and concepts. I think, rather obviously, the key is not to stop conceptualizing, but to emphasize works that will integrate the terms/concepts and to encourage “ideational consolidation” (Felps, 2006). The recent Edwards, Cable, Williamson, Lambert, and Shipp (2006) paper, I believe, is one step in the right direction. We need more, lest we enact the Chinese proverb, “One step forward, two steps back.”

Issue #2: Is Fit A Weak Paradigm?

The literature on fit, it seems to me, is an exemplar of a *weak paradigm*. In Kuhn's (1996) way of thinking, stronger paradigms have fewer debates "over legitimate methods, problems, and standards of solution" (p. 48). To Kuhn, the consequences of a weak paradigm are high rejection rates, heavy emphasis placed on particularism versus universalism (in short, favoring pedigree, status, and networks over the meritocracy of ideas) and, ultimately, stunted scientific progress. Although one may well disagree with Kuhn's causes and consequences of paradigm development, surely most would agree that fit as a field is so weakly developed that we see disagreements even as to the scope of the field. Moreover, the area has been dominated by methodological debates, a plethora of labels, various levels of analysis, fuzzy boundaries, etc. It is true that, to some degree, these arguments could be made about most any OB topic. As Schneider (2001) concludes, "There is considerable ambiguity over what is appropriate research from a person-environment fit perspective" (p. 150).

One might argue that if fit research is to advance, we should do more than celebrate the diversity of ideas and approaches. Intellectual debate is interesting and undoubtedly at some level healthy. However, I also think we are fooling ourselves if we think a field so wracked with dissensus does not circumscribe the contributions of the field to intellectual thought in organizational behavior. I am not arguing for a forced consensus, nor am I really suggesting any "solution." However, we must try to see things as they are rather than how we might wish them to be. To some intrepid researcher, problems become opportunities. But, today, problems are problems.

To develop a stronger paradigm, we need to better delineate what is, and as importantly, what is not, fit. One might argue that any conceptualization of fit must include commensurability is critical fit, as Harrison (2007) has argued. From this assumption, Harrison and other fit researchers would then reject alternative conceptions of fit that do not meet the commensurability test. The problem, I think, then lies in what one defines as commensurate. If one were to conduct a study showing that individuals high in need for achievement were attracted to organizations that based pay on performance (see Turban & Keon, 1993), this would appear to fail the commensurability test. Would this, then, not be a study of person-organization fit? "Sticky questions" such as these need to be tackled head-on if the field is to develop a stronger paradigm and realize the consequent advantages as suggested by Kuhn.

Issue #3: Endogeneity and Selectivity Biases

In my doctoral education at Illinois, labor economists were in abundance. I at times felt if I heard the terms "endogeneity bias" and "selectivity bias" one more time I was going to scream. Still, such biases pose a perverse problem in the fit literature because the very concept we are studying is itself manifested in selectivity bias (both employee self-selection and employer selection), as shown by Schneider (1987, 2001) and supported by Schneider et al. (1998). As Schneider (2001) aptly notes, "I think the point cannot be overemphasized: When either the person variable or the environmental variable (or both) are restricted in range, finding a significant effect for fit (or a significant effect for an algebraic interaction) will be constrained" (p. 146).

To review, selectivity (or selection) bias occurs when the dependent variable is measured for only a select portion of the sample. In a classic example from labor economics, if we use a dummy variable (union vs. nonunion) to predict wages for the pooled sample of workers, and conclude that the coefficient on this variable represents the union-nonunion

wage differential, we have a selectivity bias because we observe an individual's union wage only if he or she belongs to a union. This problem becomes magnified if we consider the endogeneity bias – individuals who join unions may be different for unobserved reasons that are correlated with earnings and thus the coefficient estimate does not accurately represent the causal impact of unionization on wages. For example, if lower ability individuals are more likely to join a union, then the coefficient on the union dummy variable will be downwardly biased because it is confounding two effects (the effects of joining a union on earnings and the effects of ability on earnings).

As this example suggests, such processes may wreak havoc with the fit literature. If we link a measure of person-organization fit to an employment outcome (e.g., a direct measure of perceived fit on job performance), we very likely do so without regard to possibility selectivity and endogeneity biases. For example, since individuals may self-select, and be selected, into organizations or jobs based on perceived similarity, we have a selectivity problem in that poorly fitting people never joined the organization. Moreover, we also have likely endogeneity effects because personality (Judge & Cable, 1997) or ability (Kristof-Brown, 2000) may influence direct or indirect measures of fit.

As is often the case, it is easier to identify the problem than delineate simple solutions. However, drawing from our friends in labor economics, we can address endogeneity biases by, as much as possible, including the distal characteristics that may predict variation in measures of fit. For example, if we are linking the degree to which organizational members fit to an outcome variable, then to eliminate the endogeneity bias, we can include in our regression those variables (like personality, abilities, and values) that may be antecedents to the fit measure. A variation of this technique is to use a two-stage regression approach (the same thing can be accomplished in SEM) that explicitly models the effect of the distal variables on fit the measure. Results from properly specific models will then inform us about the causes of fit, and also provide more accurate estimates of the effect of fit on outcomes.

Issue #4: Is Fit More Illusory Than Real?

In one of his more interesting (and certainly controversial) points, Harrison (2007) argues that direct measures of fit (meaning overall perceptions of fit) are so meaningless as to deserve abolition from the literature. Although most, myself included, disagree with such a strong recommendation, he has a point in this sense. If direct measures of fit are relatively poorly related to “indirect” measures, and there is reason to believe that this is the case (Edwards et al., 2006), then what does that tell us about the ontological meaning of direct measures of fit? It could tell us that indirect measures are so fraught with measurement problems as to be meaningless.

However, it also could mean that perceived or direct measures of fit are more illusory than “real.” By illusory, I mean: Is person-organization fit mostly a general impression that may say as much about a person’s general attitude toward her organization? Recently, in another paper, Harrison, Newman, and Roth (2006) found that specific job attitudes (job satisfaction, organizational commitment) are indicated by a common factor and that the common factor was better suited to predict broad organizational criteria. It may be that one could use other attitudes, such as direct perceptions of fit, as additional indicators of this broad job attitude factor and that, when considered in this light, there is little unique variance attributable to direct perceptions of fit beyond this general factor. In short, although direct measures of fit may be correlated with organizational criteria (Kristof-Brown, Zimmerman, & Johnson, 2005), this does not mean that direct perceptions of fit have any unique meaning beyond their indication of a broad job attitude factor. As Ostroff (2007) notes, direct

measures of fit are “...in some ways analogous to other measures of affective responses to the organizational situation (e.g., job satisfaction) in that it captures an overall subjective or affective reaction to the contextual environment.” If we show that direct or molar perceptions of fit are “important” in terms of their relationship to certain outcomes, are these molar perceptions merely indicators of some more general orientation toward the job or organization, as suggested by Harrison’s study?

Issue #5: Statistical Issues

Jeff Edwards has made a persuasive case for the problems with indirect (D-scores, profile similarity indices). By and large, I agree with his criticism. I worry, however, that the remedy may kill the patient as quickly as the disease. The previous comment may be a bit of hyperbole, but the correct way of measuring and analyzing fit – using polynomial regression / surface modeling – has limitations of its own. Allow me to elaborate.

Moderated regression, a simpler form of polynomial regression, relies on rarely-tested assumptions, such as homogeneity of error variances and freedom from range restriction (Aguinis & Stone-Romero, 1997; Alexander & DeShon, 1994). Moreover, A×B interaction terms have high power requirements (i.e., require very large sample sizes to have a reasonable probability of detecting a “true” interaction). As Alexander and DeShon (1994) note: “A major problem in reliably detecting interaction effects is that even in the best of circumstances, such tests have very low power” (p. 312). There is a common refrain one hears in response to this issue, which is: “Yes, but since our interaction term (terms) was (were) significant, this is a non-issue.” That may be true enough if a “pure” Null Hypothesis Significant Testing (NHST) approach were followed (I hesitate in using “pure” and “NHST” in the same sentence, but bear with me), but I believe this is rarely the case. In most situations, I think the authors “peek” at their results before writing their introduction and it would be naïve to assume that such “peeks” have no implications for what is developed and tested in the paper. Thus, in practice, surface plots, perhaps to an unusual degree, are as much a product of induction as deduction. If I am right, because such results may capitalize on chance, then the acid test is whether the interaction is replicated independently. With “simple” A×B interactions, there is reason enough to be dubious about replicability. With polynomial regression, both the statistical power and the ability to replicate the specific response surfaces seem to me to be perversely low. The power problems with moderated regression (McClelland & Judd, 1993) are significant enough – before we consider not only A×B, but *also* quadratics, and interactions *with* quadratics.

Issue #6: Time, Changes in Fit, and Temporal Dynamics

Fit is, by implication, a state that may vary over time. It is true that, depending on how one defines fit (e.g., values, traits), both the person and situation component may be relatively stable. However, even in these cases, neither the person nor the situation variable are wholly stable, which means that fit can be expected to vary over time. When dealing with direct perceptions of fit, one might even expect to see day-to-day fluctuations, as has been shown with respect to other attitudes (e.g., Fisher, 2000; Fuller, Stanton, Fisher, Spitzmüller, Russell, & Smith, 2003). There are good longitudinal studies in the fit literature. However, there are not many studies that look at *changes* in fit over time, nor those that attempt to decompose variance into between-individual (individual differences in fit) and within-individual (intra-individual variation in fit over time) sources. Studying fit in such a multilevel context would seem to be a particularly promising area for future research.

Most investigations of fit have been relatively static in nature. Yet, some people who don't fit don't leave. What do organizations do with employees who do not fit their culture or intended goals? What do individuals do when they are embedded in a job or organization that misfits them? The ASA model discusses selecting in and selecting out strategies, but does not consider the question of what happens when these forces are blocked (for whatever reason). Although neither I nor the authors can answer these questions, I do think a particularly promising place to look for inspiration is the attitude literature. In reading Eagley and Chaiken's (1992) seminal book on attitudes, I have always been surprised by how much of it focuses on attitude change and related topics like persuasion. If person-organization fit (at least direct judgments of it) can be likened to an attitude, then perhaps some of the concepts and approaches can be adapted from the attitude literature to study changes in fit perceptions over time.

There has been somewhat of a divide between those who study newcomer socialization/mentoring, which tends to take a dynamic or change-oriented perspective, and those who study person-organization fit, who generally take a more static view of individuals (by focusing on personality and values – which are generally seen as stable). Thus, one might argue most person-organization fit researchers take the perspective that the person "reads" the organization (makes an assessment of his or her values or personality and determines how well this matches the organization, the work group, or the job) and makes a choice of whether to join or remain in the organization (or job or group) based on this reading, and the organization does the same ("reads" the values and personality of the applicant or employee, and makes personnel decisions about the person accordingly). This perspective assumes that people "are who they are" and that fit is dynamic only so far as misfits exit and those who fit enter (and thrive). To be sure, there are some who have brought these perspectives together (e.g., Cable & Parsons, 2001; Caldwell, Herold, & Fedor, 2004; Cooper-Thomas, van Vianen, & Anderson, 2004), but such undertakings are the exception rather than the rule.

Having learned a fair degree from the "reading" approach, an exciting avenue to pursue is whether values and personality might actually change. When this change represents a movement toward the organization (or job or work group), then fit increases, even if the people are the same (no one exits or enters). I have long felt that we in organizational psychology have assumed, to an inordinate degree, that personality is fixed – that because it has genetic origins (true), it is immutable (false – only the genetic part is fixed, and even that can interact with the environment). There is clear evidence that personalities do change and as Caspi, Roberts, and colleagues (Caspi & Roberts, 2001; Caspi, Roberts, & Shiner, 2005; Roberts, Walton, & Viechtbauer, 2006) have noted, one can find evidence to support the stability and the change perspectives. Given the centrality of work to people's identities (Hulin, 2002), if the situation can lead to a change in personality, the work environment (or construals thereof) certainly might be a place where that might happen.

Issue #7: How People Make the Place

Since Schneider's ASA model (Schneider, 1987, 2001; Schneider et al., 2000), fit researchers have implicitly accepted the notion that the environment may be "made" by the people. There is evidence to support the model at the individual and the organizational level. At the individual level, people whose values match the dominant organizational culture appear to be more likely to be selected, and to self-select, into the organization compared to people whose values do not match (Chapman, Uggerslev, Carroll, Piasentin, & Jones, 2005). Similarly, individuals who mismatch appear to be more likely to exit the organization (Arthur, Bell, Villado, & Doverspike, 2006). At the organizational level, there is significantly greater homogeneity in personality within organizations than between organizations (Schneider et al.,

1998). However, we have relatively little data on the subject of what makes the environment. To be sure, at a somewhat obvious level, an organization's culture might be substantially defined by the personalities and values of the people in it (particularly in the upper echelons). However, if this does happen, how does it happen? And, as Schneider (2001) notes, we have relatively little understanding about the environment in fit research.

I wonder if greater utilization of Mead's (1934) and Blumer's (1969) concept of symbolic interactionism might be of use here. Symbolic interactionism assumes that "reality" is a subjective concept that is construed or even created by the actor, and these construals are based on pragmatism – people base their knowledge on what has proven useful and adaptive for them. Under this view, there is no "actual" or "objective" fit. Fit is a perceptual process, based on individuals interactions with others, and based on the utility of their perceptions. Thus, in contrast to Harrison's view, symbolic interactionism might suggest that perceptions of fit do have an important meaning. However, to be meaningful, symbolic interactionism would require that such perceptions be studied in a radically different way. It would require giving up the idea of veridicality of perceptions and focus on the causes – and consequences – of such perceptions in their own right. One of the key tenets of symbolic interactionism is that we see ourselves as we believe others see us. As noted by Jussim, Soffin, Brown, Ley, and Kohlhepp (1992), "Others' evaluations influence targets' self-concept indirectly, as mediated by targets' perceptions of those evaluations" (p. 403). If people do internalize such reflected appraisals, then it becomes interesting to try to understand how people arrive at judgments that they fit into an organization (or occupation or group). To use Myers-Briggs (MBTI) acronyms, if someone learns that one is an INFP and one's supervisor is an ESTJ, how does such feedback influence our appraisals of fit? Feedback from others is a powerful source of beliefs about our selves, and fit should be no exception. Symbolic interactionism has garnered more than its share of criticism – some of it justified – but it continues to be of interest to social psychologists. It may have a greater role in fit research as well.

Issue #8: Fitting and Surviving

Many argue that organizations and the individuals in them strive to achieve homogeneity on salient characteristics, including demography. Ellis and Tsui (2007) argue that that homogeneity produces positive affective outcomes at Stage 1 (positive feelings and psychological states such as satisfaction, cohesion, and efficacy), but negative outcomes at Stage 2 (negative cognitive outcomes such as inertia, groupthink, and low conflict), Stage 3 (negative behavioral outcomes such as low learning and creativity, impaired decision-making, and poor performance), and Stage 4 (negative survival outcomes such as individual exit and firm failure). I think this multi-stage process model is very thought-provoking, and below are three counter-thoughts I had in reading their work.

First, on the link between demographic homogeneity and affective outcomes, does demographic similarity always lead to positive feelings? Would that not depend on the person? Do men always prefer to work with other men (or women with women)? Might not those who are dispositionally open value diversity (believing that encountering people from different backgrounds makes life more interesting)? Or, would organizational climate (whether leaders support diversity) make a difference?

Second, the model assumes that positive affective outcomes generally lead to negative cognitive outcomes. Though clearly one can think of situations in which this might be the case, one can think of many (arguably more) examples where that would not be the case. For example, would satisfaction and efficacy generally lead to low conflict or groupthink? Maybe. Or maybe not. My point is not to "hammer" the model for its failure to include moderating influences. After all, any model at some level omits moderating (and mediating)

mechanisms. Rather, these are questions that are relevant for future research testing this part of the model.

Third, I can think of many situations in which the “negative” cognitive outcomes (such as inertia and low conflict) are adaptive. For example, the idea that low task conflict is associated with low group performance has been recently questioned (De Dreu & Weingart, 2003). To reiterate, I raise these questions not to criticize Ellis and Tsui. A model that raises so many questions on an important topic is exactly what one would want in a “thought piece.” Imagine if researchers committed themselves to testing the questions above? To quote that great philosopher Martha Stewart: “That’s a good thing.”

I should note that the authors do not argue that diversity is unequivocally negative for their Stage 2-4 outcomes. They do a good job of reviewing evidence suggesting, in some cases and with some studies, negative effects of demographic diversity. However, their general orientation toward demographic homogeneity is, in their words, “the deeper and long-term impact of homogeneity on groups or organizations is diminished survival potential.” There is a difference between valuing demographic diversity in an intrinsic sense, and valuing it in a social scientific sense. It behooves us to separate these two – not favoring one over the other, but recognizing that the former is an individual or societal value choice and thus is inherently subjective, whereas the latter depends on a cold, hard look at the evidence (at least to the degree researchers are capable of doing that).

Issue #9: Fit and Personnel Selection

If this concept of fit is so pervasive, why has it really not proven itself in personnel selection research? Although there is recent support for trait \times trait interactions in the selection literature (Witt, Burke, Barrick, & Mount, 2002), as well as moderators based on job or organizational characteristics (Rothstein & Goffin, 2006), by and large, the validities of the best predictors of job performance appear to be quite robust for general mental ability (Schmidt & Hunter, 2004), conscientiousness (Barrick & Mount, 2005), integrity (Ones & Viswesvaran, 2001), work samples (Roth, Bobko, & McFarland, 2005), and interviews (Huffcutt, Roth, & McDaniel, 1996). Generalizing from these results, one has to wonder whether fit is all that relevant to job performance, at least as far as the research literature has been able to demonstrate.

To be sure, cognitive ability is more predictive of performance in complex than in relatively simple jobs, but it is still has significant validity for relatively simple jobs (Bertua, Anderson, & Salgado, 2005). I think fit probably is a relevant concept for many jobs and explains why the interview continues to be used (Judge & Ferris, 1992). However, if one believes this, one cannot help but be disappointed by the relative failure of the selection literature to support the view. Perhaps we have not conceptualized or investigated the relationship in the right way. By and large, the personnel selection literature has focused itself on abilities but, again, at least as far as individual differences are concerned, it has been assumed that the more ability, the better. And the evidence does not appear to contradict this view. So, is demands-abilities fit irrelevant for personnel selection? Why is it that selection researchers have been relatively uninterested in this type of fit? Are other types of fit (e.g., needs-supplies), or other fit concepts (e.g., molar fit) properly outside the realm of personnel selection? In I-O psychology, we have selection researchers, and fit researchers, but there seems to be very little integration of the two fields. Such collaboration might contribute to each area.

Issue#10: Is Homogenizing Around Fit Ethical?

An employer has no business with a man's (sic) personality. Employment is a specific construct calling for specific performance, and for nothing else. Any attempt of an employer to go beyond this is usurpation. It is immoral as well illegal intrusion of privacy. It is abuse of power. An employee owes no "loyalty," he owes no "love," and no "attitudes" – he owes performance and nothing else.

Drucker (1973, pp. 424-425)

I find this quote fascinating, in no small part because it raises interesting ethical questions for those of us interested in personalities, emotions, and attitudes at work. It seems clear that research has shown that these concepts are relevant to workplace attitudes and behaviors. Most fit researchers assume that fit is socially desirable, and fit is an implicit outcome in socialization processes. Clearly, though, if one tries to reconcile Drucker's comment with the belief that (a) fit is desirable from the vantage point of an organization, (b) organizations attempt to increase fit through selection and socialization processes, further questions arise. If fit is desirable, is it viewed only in terms of work outcomes (those that might be derived from work demands)? Do socialization efforts concern themselves only with specific performance outcomes? If one answers these questions in the negative, then Drucker's comment suggests ethical concerns. I do not suggest answers to these questions, but in the fit literature one sees little effort to grapple with these concerns. One can certainly envision an organization ensuring attitudinal homogeneity along the lines of Oceania's Ministry of Truth or The World State's "Community, Identity, Stability." When do efforts to achieve fit cross a moral boundary?

Conclusion

It is an exciting time to be doing fit research. As I have noted in my comments, I do believe there are some problems that are seemingly intractable. However, it is at times like this that the greatest innovations are made. Very recently, a reclusive Russian mathematician, Grigory Perelman, won (and refused to accept) the Fields Medal, the highest honor in mathematics, for introducing the solution to Poincaré's conjecture, a hypothesis involving the (very complex) structure of three-dimensional objects (Johnson, 2006). Many mathematicians had considered the conjecture insoluble. In the admittedly more prosaic fit area, in the future someone is going to figure out some of these formidable problems. Will it not be exciting to see what that future holds?

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