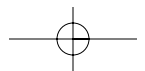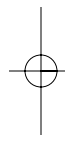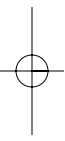# PART III

Concluding Remarks

The final chapter in this volume provides a commentary on each chapter. In addition, new insights to guide fit research in the upcoming years are offered.

CHAPTER 13

The Future of Person–Organization Fit Research: Comments, Observations, 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 body of 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. Both of these elements, the notes of optimism and of pessimism, I believe, are suggested by the chapters in this volume. In this concluding chapter, I offer a few thoughts on each chapter and end by noting some issues, quandaries, questions, and future directions that I believe deserve attention by fit researchers.

Before digging in, however, I want to express two notes of thanks. First, I want to thank Cheri Ostroff for being a wonderful collaborator. Cheri was the genesis of this book, and throughout she has displayed a remarkable mix of initiative, diligence, patience, and enthusiasm. In my experience, this constellation of qualities is unusual, and I am glad to have had the chance to work with her. Second, as Cheri and I noted in the preface, we are grateful to the contributors to this volume. As even a causal reader of the fit literature knows, this set of authors is a real *tour de force* of contributors. It has been a joy to read their work.

COMMENTARY ON CHAPTERS

Ostroff and Schulte: Levels of Analysis and Fit

The first chapter, by Ostroff and Schulte, takes a levels of analysis approach to fit. Ostroff was ahead of her time in thinking about levels of analysis issues in the fit literature (Ostroff, 1992; Ostroff & Schmitt, 1993), so there is no better person to tackle this complex area. As Ostroff and Schulte note, most research on fit has bifurcated into micro and macro areas, with the former being more voluminous than the latter. In one sense, this is surprising because perhaps the dominant perspective on person–organization fit is Schneider's (1987) attraction–selection–attrition (ASA) model, which incorporates both micro and macro perspectives (see Schneider, Smith, Taylor, & Fleenor, 1998). On the other hand, the micro–macro divide is a deep one in social science (Bar-Tal, 2006; Goldspink & Kay, 2004), and in this sense it is no surprise that fit research has experienced a similar chasm. One has to be excited by the authors' effort to bridge this gap.

Ostroff and Schulte do an excellent job of providing a brief review of the history of person–environment and person–organization fit research. Then they move to their real mission—to provide some much needed integration of the micro and macro perspectives to bridge this aforementioned gp. To do so, they introduce some terms, some old (person–job fit) and some new (person–person compilation and person–individual fit). These terms vary by target or level (depending on whether the person is fitting to a job, to a work group, etc.), by mode (person–person, person–situation, and situation–situation), and by type (supplementary and complimentary). This is challenging stuff, and I give Ostroff and Schulte credit for their effort to integrate the extant terms and concepts of fit. I must confess some amount of confusion about the interrelationships among the categories and terms, and one might arrange or conceptualize the concepts differently. However, as in the case of a recent integrative work (Edwards, Cable, Williamson, Lambert, & Shipp, 2006), one sees great value in attempting to draw some boundaries around fit concepts and then trying to establish links among them. There are many important insights to be gained from a careful reading of the chapter. Although (given their efforts at integration) the authors might disagree, given the complexity of the model and the alternative conceptualizations possible, I think research that tests their concepts by tearing off bites at a time might prove most useful. I hope that researchers will draw inspiration from their integrative efforts and test these concepts: a few efforts would advance the fit literature further.

Higgins and Freitas: Regulatory Fit

Although the general premise of person–environment interactionism—that individuals and their environment exist in mutual interaction—is hardly a

new concept in psychology, there have been surprisingly few theoretical statements that specify exactly how this interaction takes place. Higgins' [AQ3] (2000, 2005) regulatory focus theory is thus an important addition to psychology, as evidenced by its application to many areas of psychological science in just the past few years, such as romantic relationships (Roese et al., 2006), test-taking performance (Keller & Bless, 2006), interracial interactions (Trawalter & Richeson, 2006), reactions to antismoking advertisements (Kim, 2006), consumer preferences (Yeo & Park, 2006), affect and decision-making (Leone, Perugini, & Bagozzi, 2005), negotiation (Galinsky, Leonardelli, & Okhuysen, 2005), and cross-cultural differences in motivation (Lockwood, Marshall, & Sadler, 2005). This is a theory that is being applied to myriad psychological and social–psychological processes, and the link to the person–organization fit literature is an obvious but relatively unexplored one.

Indeed, although one might see regulatory focus theory fitting under the umbrella of approach–avoidance motivation, which is attracting renewed interest (e,g., Centerbar & Clore, 2006; Elliot, Gable, & Mapes, 2006; Updegraff, Gable, & Taylor, 2004), what is unique about Higgins' work is that it places motivation in a fit context. Specifically, according to regulatory focus theory, promotion-focus striving (working toward a goal with a sense of hope and eagerness—from a sense of "feeling right") is oriented toward bringing the actual self in line with the *ideal* self, whereas prevention focus striving (working toward a goal from a sense of duty and vigilance—from a sense of "feeling wrong") seeks to bring the actual self in line with the *ought* self. What is intriguing about regulatory focus theory is that it is not merely whether a goal is attained; how it [AQ1] is attained matters (in this way it is similar to the self-concordance model [Sheldon & Elliot, 1999]). In a real sense, this is a radical shift in thinking. As Higgins and Freitas noted, since the "cognitive revolution" in the 1960s, there has been considerable attention paid toward goal strivings. The implicit assumption of these various literatures is that the objective is goal attainment. Although regulatory focus theory does not deny the potential relevance of goal attainment, it indeed shifts focus from outcome (goal attainment) to process (reasons underlying goal striving).

Another exciting aspect of regulatory focus theory is the diverse way in which its effects have been tested, beyond the array of topical areas noted earlier: There have been both within-individual and between-individual designs, both direct and indirect measures, laboratory studies and field studies, experimental and correlational designs, and regulatory focus measured as experienced (i.e., self-reported) and regulatory focus induced (e.g., manipulated similar to framing effects in prospect theory). Although the results are not always fully consistent, in general, the results support the view that promotion focus is associated with more positive thoughts and feelings and prevention focus with more negative thoughts and feelings.

What is somewhat surprising, though, is the limited degree to which any of this work is associated with the person–environment or person–organization fit literature. There is a reason for this. As we saw in the Ostroff and Schulte's chapter, the vast majority of fit research is oriented toward fit with organizations, vocations, jobs, and workgroups. Much less is known about how well people fit with the goals they pursue (Judge & Kristof-Brown, 2004). Because both the ideal and ought selves are probably prominent identities for most individuals at work, one can see all sorts of questions relevant to fit research. For example:

- What sort of workplace moods and emotions are regulatory fit discrepancies associated with? Do people who experience discrepancies between their actual and ought identities experience guilt?
- Because work–family conflict, by definition, means a conflict in roles, how is regulatory focus relevant in the work and family domains? Do promotion and prevention strivings differ in the work and the family domains?
- Are promotion-focused people more likely to experience actual/objective/indirect fit and perceived/subjective/direct fit? Are they better at perceiving actual fit (have a closer correspondence between actual and perceived fit)?

Kammeyer-Mueller: Newcomer Fitting In

As Kammeyer-Mueller notes, 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 that 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 workgroup, 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, as noted by Kammeyer-Mueller, an exciting avenue to pursue is whether values and personality might actually change. When this change represents a move-

ment toward the organization (or job or workgroup), 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. It is exciting to see Kammeyer-Mueller take a dynamic perspective on *fixed* individual differences (personality and values). I await eagerly some data to test this concept and approach.

Kristof-Brown and Jansen: Person–Organization Fit

Relative to the other chapters in this volume, Kristof-Brown and Jansen offer a narrower and more discrete perspective. Specifically, the authors review past evidence on whether person–organization fit is relevant to the most commonly studied criterion in organizational behavior (OB), [AQ2] and discuss how fit should be assessed. In the second half of their chapter, they turn their attention to future issues in fit research. With its focus on person–organization fit and the literature directly in that area, the scope may be somewhat more bounded than that of other chapters, but it is no less important because most fit research in OB has focused on person–organization fit. Their review of past research on person–organization fit provides an excellent summary of where we are.

Perhaps the highlight of the chapter, however, is when the authors turn their attention to future research and focus specifically on temporal dynamics. As I have noted in reviewing Kammeyer-Mueller's chapter, most investigations of fit have been relatively static in nature. As Kristof-Brown and Jansen note, "Some individuals who don't fit don't always 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 it 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 such as 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.

Cable and Yu: Recruitment, Selection and Fit Perceptions

A social psychologist once wrote: Of all the things we have learned about social psychology, perhaps none is more pervasive than the similarity–attraction paradigm. By the same token, we have learned from person–organization fit research that nothing attracts an individual to an organization or an organizational member to an individual as the belief that they are similar. Consistent with this viewpoint, in their chapter, Ellis and Tsui comment: "Fit is a desired state of being strived for by all social entitites." The power of this statement is tempered, however, upon deeper consideration of its meaning—it may be important for people to perceive fit, but what does that really mean? What does it really mean when a job seeker thinks that "a job is a good fit for him," or when an interviewer chooses an applicant because "she is a good fit?" If fit perceptions were easily broken down into discrete elements, then such statements would be simple enough in their meaning and implications. However, as Edwards et al. (2006) recently noted in their excellent article, it appears that different approaches and measurements of fit are not interchangeable and that "the approaches apparently tap into different subjective experiences" (p. 818). So I return to the question: What does it really mean when someone perceives fit?

Cable and Yu address this question by focusing on the role of recruitment and selection practices in shaping perceptions of fit. The authors draw on a blend of theory and research from "outside" disciplines (communication, attitudes, and persuasion) in arguing that individuals' beliefs regarding fit are shaped by the credibility of the source and the richness of the information: the more credible the source, and the richer the information (such as face-to-face interactions), the more likely that perceptions of fit will be shaped by messages given by the organization. By applying these concepts to recruitment and selection methods, the authors are able to analyze the possible effects of different recruitment and selection methods (web pages vs. interviews, for example) as potential sources of fit perceptions. In research that would test their model, or pieces of it, further elaboration would be required (e.g., are we talking only about molar fit perceptions here?), but such tests would have the unusual potential to blend basic theory in social psychology with practical issues germane to the recruitment literature (which recruitment methods work best?).

Atwater and Dionne: Leader–Follower Fit

As Atwater and Dionne note, although fit research has focused on how individuals fit in different contexts (to the vocation, to the organization, and

to the job), there is a paucity of research linking fit between leaders and followers. To be sure, there are leadership concepts (most notable, as the authors state, is leader–member exchange theory) that consider fit in a broad sense. However, such leadership theories have not been integrated with research in person–organization fit, despite some compelling parallels. Toward this end, Atwater and Dionne develop a process model that attempts to build on past leader–follower research.

Indeed, I think their model has the potential to contribute to both leadership and person–organization fit research. In terms of fit research, most research is based on the premise that fit perceptions flow from inherent congruities (or incongruities) between values (or some other characteristic). Atwater and Dionne turn this hypothesis around and posit that early leader–follower similarity will induce attempts to align values, attitudes, and personality. This is a great insight. As robust as the similarity-attraction model is—Bryne and Nelson (1965) went so far as to label it the "*law* [italics added] of attraction"—one can easily conjure up situations in which an individual who likes someone else strives to find points of similarity with that person and to find areas of divergence from people they do not like. I do not think Atwater and Dionne mean to deny the merits of the similarity–attraction paradigm, nor do I, but I do think their point is that the causality can run the other way, and sometimes constructions of similarity follow from early attraction. As I will note later, I think alternative causal sequences in the person–organization fit literature are needed, and Atwater and Dionne provide one here.

In terms of the leadership literature, I think the Atwater and Dionne's chapter (and their model) is a real step forward in that it focuses on (a) dynamic (time-variant) processes and (b) the role of congruence as a linking mechanism, either in bringing the leader and follower closer together or in driving them farther apart. Leadership research continues to lack process explanations (although that situation has improved in the past decade), and I think leader–follower similarity is a particularly interesting and promising area for future research. As is no surprise given my praise of the dynamic models in Kammeyer-Mueller's and Kristof-Brown and Jansen's chapters, I think the change- or state-like nature of their model is a particularly exciting avenue for future person–organization fit research and for leadership research too.

Edwards and Shipp: How Fit Matters

Edwards and Shipp's chapter, like the Ostroff and Schulte's, illustrates the complexity of fit. Edwards and Shipp distinguish fit by three dimensions, two of which have three levels and the other has five levels. This is a lot to ponder, especially when one considers that 45 distinct fit configurations are suggested by the model. It is somewhat difficult to distinguish

proximal categories, although the authors do note that their model is mainly for heuristic purposes.

I think the real heart of the chapter is when Edwards and Shipp begin to apply their conceptualization of fit to outcomes. Few in the fit literature (I most certainly include myself in this criticism) do as careful a job linking their conceptualization of fit to the theoretical nature of the outcome/criterion variable. Because their approach is extraordinarily well grounded, it is unusually easy to find points of convergence and divergence.

[AQ3]

One area of divergence lies in how Edwards and Shipp often label broad concepts as confounded. For example, they argue that some definitions of job satisfaction confound affect and cognition. Although to some extent this is true, I think it is true of any broad concept. As the British empiricist David Hume noted, an idea is an abstraction from specific impressions, but that does not mean that ideas are inherently problematic as might be implied by the term *confounded*. The point of clear thinking, in Hume's philosophy, is to be able to separate ideas from impressions, but this is not meant to deny the importance of ideas.

Similarly, we can investigate affects and cognitions as distinct influences on job satisfaction, but (a) that is difficult to do (few important cognitions are affect-free) and (b) that does not mean there is anything contaminated or otherwise wrong about general measures of broad concepts (like a measure of overall or general job satisfaction). Broad and specific concepts have their mutual purposes. But we make a mistake in arguing that a broad concept is confounded. To be fair, Edwards and Shipp do not directly argue that job satisfaction or other broad measures are flawed. However, I am afraid that their arguments about confounding could be misconstrued and thus misused.

In terms of convergence, the argument that needs–supplies fit is more directly related to job satisfaction than demands–abilities fit strikes me as right. I like the way that Edwards and Shipp translate Locke's value–percept theory into needs–supplies (although I have always felt that *need* is a concept that is practically impossible to meaningfully define). What I particularly like about the chapter is that Edwards and Shipp move from a discussion of conceptual linkages between their forms of fit and attitudes toward specifying the functional forms of those linkages. For example, they note that a typical discrepancy model (their Fig. 7.2a) is insufficient (an oversimplification of fit) because it ignores the levels of the variables. As they note, "wanting and having a simple job is very different from

[AQ4]

wanting and having a complex job," even though the degree of absolute misfit in these two cases could be exactly the same. Of course, Edwards has made this point before, but here the point is useful in specifying the expected relations between different forms of fit and various, important outcomes. I do not doubt that empirical data would show that some of their hypothesized forms are supported and that some are not, but the

key is that this is the right way to look at the relationships, both concep-
tually and statistically.

DeRue and Hollenbeck: Fit in Teams

I remember when I first learned about John Hollenbeck's foray into the
teams literature. It was 1994, I was an assistant professor at Cornell, and
we were trying to convince John to join us at Cornell (our efforts failed).
John presented his ideas about the increasing importance of work teams
in organizations and discussed his ideas about personality and team
composition. I was convinced that John was the right person for the job; I
was less convinced that team research was going to pan out. Scores of
publications later, John is perhaps even better known for his teams
research than for the research I admired him for then (and still do: his
research on motivation and applied research methods). How could a
sage prognosticator be wrong? Certainly, I underestimated the processes
that could be manipulated and measured in a laboratory setting. Also, I
underestimated the complexity of team composition, the interesting
processes, interactions, and outcomes that could be studied from a team
vantage point.

It is on this issue—the complexity of teams—that DeRue and Hollen-
beck focus their attention. Their premise is that past teams research has
not fully captured the complexity of how teams work and what interac-
tions might make them work better. The model that they derive from that
premise, with its focus on internal and external fit, is a masterstroke.
What leads me to such a strong conclusion? First, their model effectively
summarizes the Hollenbeck–Ilgen team laboratory (HITL) research, which
has predominantly focused on team composition issues, under the rubric
of internal fit. As DeRue and Hollenbeck note, team composition is about
fit; for example, if it is valuable to have a highly conscientious team
member, what about two highly conscientious team members, or having
all team members being highly conscientious? Or, how might personality
differences, or trait constellations, fit with one another? These are all what
DeRue and Hollenbeck term *internal fit* issues, and the HITL research
provides answers to these team composition questions.

Increasingly, the HITL folks have turned their attention to process,
systems, and outcomes and that is reflected in the guts of the model
DeRue and Hollenbeck present, as well as in what they label *external fit*.
Having provided a wealth of interesting findings concerning team com-
position and aspects of internal fit, I had thought that HITL researchers
were probably winding down. As their section on external fit shows,
however, there are myriad interesting and important questions yet to be
answered. And this is where, as the authors recognize, the fit literature
can be especially helpful. Because team composition research is, in Ostroff

and Schulte's lexicon, person–person oriented, the environment has a somewhat limited role. However, because by definition external fit concerns the match between teams or team members and their environment, this is where person–environment fit concepts and research are especially apropos.

In sum, if this chapter is any illustration of things to come, it appears that I have underestimated Hollenbeck and the HITL yet again. They show no signs of exhausting the array of interesting ideas and tests of those ideas anytime soon. I look forward to reading their research and hope they do more, as is evidenced here, to link their work to the fit literature.

Ellis and Tsui: Fitting and Surviving

Ellis and Tsui make the compelling case, supported by mounds of research from various literatures (including the similarity–attraction paradigm, whose centrality to social psychology I discussed earlier), that organizations and the individuals in them strive to achieve homogeneity on salient characteristics, including demography. The authors take the position 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 multistage process model is unusually clear and provocative. For one who likes academic disagreement, here I make a few points. But do not infer that I find the work of Ellis and Tsui troubling—quite the contrary, I think it is very thought-provoking and here are three counterthoughts 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. Although clearly one can think of situations in which this might be the case, one can think of many (arguably more) examples for which that would not be the case. For example, would satisfaction and efficacy generally lead to low conflict or groupthink? It may or may 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 book chapter. Imagine if researchers committed themselves to testing the aforementioned questions? 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 that researchers are capable of doing that).

Overall, I think the authors have done a laudable job of leading the reader to think about the long-term outcomes of homogeneity. Of course, scholars in organizational psychology (Schneider, Smith, & Goldstein, 2000), social psychology (Tajfel & Turner, 1986), organizational behavior (Ibarra, 1992), and organization theory (Hannan & Freeman, 1984; Powell & DiMaggio, 1991) have written about the dangers of homogeneity. However, the authors' perspective here, even in some of the linkages that I question, represents a good review of what we have learned about demographic diversity and where future research might profitably head.

Gerhart: Human Resource Management and Fit

As Gerhart notes and as I commented previously, the vast majority of fit research is at the individual level of analysis. In this chapter, Gerhart applies fit to a topic—human resource (HR) systems—at the organizational level of analysis. In so doing, he makes use of the concepts of internal and external fit, but in a way different from that of DeRue and Hollenbeck. In Gerhart's model, internal fit is horizontal fit, or how well HR practices fit with one another (e.g., it hardly makes sense to hire on the basis of factors presumed to indicate motivation and then fail to reward motivation once these highly motivated employees are hired), and external

fit is vertical fit, or the match between HR systems and contextual factors such as business conditions (e.g., investing a great deal in a sophisticated selection system would not appear to make much sense if business or industry conditions required hiring of nearly every available applicant).

As someone who still has the cobwebs of memories from my economics, strategy, and organization theory training from Illinois, it was enlightening to see Gerhart review these concepts, with some new thinking from the industrial relations literature with which I was unfamiliar. Although I hardly qualify as an expert, it seems to me that Gerhart does an excellent job of reviewing this new way of thinking of fit in terms of HR systems, while also grounding this work in the literature with which industrial/organizational psychologists are more familiar.

In terms of internal fit, judging from Gerhart's review, there seems to be a prevailing view that "bundling" complementary HR systems or practices leads to higher organizational effectiveness. As Gerhart notes, testing whether such bundles or horizontal fit is indeed associated with higher performance is a complicated issue, and the results appear, at least to Gerhart's keen eye, to be inconsistent. What to do about this inconsistency is not an easy question to answer. However, as I will note later, this is a question that fit researchers at the individual level should be asking themselves as well. In terms of vertical (external) fit, judging from Gerhart's review, the situation appears much the same as with horizontal (internal) fit: ample claims of its importance, but few strong tests with consistent results.

I think the most intriguing part of Gerhart's paper and the one that should be of interest to any fit researcher (micro, macro, or otherwise), is his section "Challenges in Studying Fit." He, naturally, focuses his attention here on the HR systems literature, yet any of the difficulties that he notes can be generalized to the person–organization fit literature. For example, Gerhart discusses statistical power issues and selectivity problems (variation in poor fit may be restricted because firms with poorly aligned systems go out of business), and indeed I will have more to say on both of these issues later. In sum, I urge you to read Gerhart's chapter. Although it surely has value for researchers who study macro/HR systems–oriented fit, it is highly accessible and a source of many relevant insights into the micro/person–organization fit literature.

The Methods Chapter: Methods/Statistical Issues in Fit Research

One evening a few years ago, one of my best friends and I were enjoying dinner at a local restaurant. Each of us ordered fish: I ordered grouper and my friend ordered tilapia. When the plates arrived, we did what diners do—we first glanced at our own plate, and then looked at the other's plate. Rarely had two presentations been so different; my plate had a large,

firm, fresh-looking piece of fish, surrounded by an attractive set of accompaniments. My friend's plate had a somewhat limp, tired-looking piece of fish (it was dead after all), on top of an unimaginative clump of rice, surrounded by limp, overcooked vegetables. As we looked again at our plates, my friend pointed at my plate and said, "Uhm, I think they mixed up our orders; you have my fish." We promptly exchanged plates, and I am sorry to tell you that "my" plate of fish tasted as stale as it looked. After our waiter noticed that we switched plates, he informed us that we, in fact, had been served the right plates. My friend (unwittingly) thought that my fish was his because my plate turned out so well. (We have enjoyed many a laugh about this episode since.)

Following this story line, the idea for this chapter was mine. No, in all seriousness, the idea for this chapter was Cheri's. When Cheri asked me about it, I immediately thought: "What a great idea; rather than having one researcher's perspective on methodological issues, readers can enjoy multiple perspectives." Assuming that you have read chapter 11, I think you will agree that it works. It is gratifying to know my idea turned out so well (just kidding, Cheri).

Ostroff's section is the first one in the chapter, and in it she discusses several important methodological issues in fit research, including measurement of fit (direct or indirect measures), levels of analysis, and possible sources of bias. This is good material. I particularly liked her treatment of commensurability (questioning the "virtual dismissal" of it in the fit literature) and restriction of range (under the heading "Sampling Issues").

Next, Caldwell, Chatman, and O'Reilly discuss profile comparison methods for assessing fit. When Dan Cable first introduced me to the Organizational Culture Profile, I remember feeling excited. As the authors note, one advantage of a profile comparison approach is that the person and the environment are matched on the same basis. Another advantage is that a correlation can be computed for each person, reflecting his or her degree of correspondence with the environment. The problem with profile comparison approaches, as Edwards aptly notes in his section, is in analyzing the data. There are better and worse ways to analyze fit data, including ipsative data. However, no matter how data are analyzed, various statistical problems are introduced when we analyze ipsative data using standard statistical techniques. Thus, in a technical sense, I do not disagree with a single word that Edwards writes. I will stop here, though, as I have more to write on this issue later.

In the fourth section of the chapter, Harrison and Sin provide a review of measures of misfit—measures that reflect dispersion or diversity. As the authors note, fit researchers generally have used measures of fit (higher scores indicate a higher degree of fit), so a review of measures with the opposite objective and interpretation (higher scores indicate a lesser degree of fit) is worthwhile. They do an unusually good job of clearly

and concisely reviewing the various measures of misfit, such as variability (standard deviation or variance), coefficient of variation (which controls for the fact that variability is correlated with the mean), and Euclidean distance (such as D or D^2). In reflecting on their recommendations, I could not help wondering how Edwards would react to some of their recommendations. For example, Harrison and Sin recommend D or D^2 for measuring misfit at the individual level. Edwards has criticized such measures, which makes me wonder how Harrison and Sin react to the criticisms (for example, although D loses less information than D^2, used by itself it still ignores differences underlying two equal D scores (e.g., $D = 3$ [5 – 2] and $D = 3$ [9 – 6]). Again, this is an issue to which I will return shortly.

Finally, Mumford and Espejo discuss cluster analysis. Although I have used cluster analysis in the past, I must admit to be chastened somewhat by criticisms of the method. The choice of the clustering algorithm is subjective, yet important; using the nearest neighbor method will often produce very different results from using Ward's method. Interpretation of the results is also subjective (it is often a very subjective decision about how many clusters are derived. A three-cluster solution often will look very different from a four- or two-cluster solution. In fairness, one might argue that unless we allow ourselves to be yoked to the ox of statistical significance or null hypothesis significance testing, interpretation of effect sizes is inherently subjective and in that sense cluster analysis is no better or no worse than other statistical methods. However, because the metrics underlying cluster analysis are not meaningful (similar in that way to sum of squares in analysis of variance), we do not have meaningful effect sizes to interpret nor confidence intervals around estimates. Now, it is true that the clusters derived from the analysis can be tested (perhaps even validated) against other criteria, but often the tests involve statistical procedures with problems of their own (analysis of variance, discriminant analysis, and others). Despite limitations, as Mumford and Espejo note, there are questions for which cluster analysis is uniquely situated to answer. Whether the gain is worth the price is something for the reader to decide.

Harrison: Testing the Construct of Fit

In opening this chapter, I argued that one could find reasons for hope and despair in pondering the fit literature. Harrison's thought-provoking chapter, to this reader at least, exposes some of the reasons for despair. Although he does not use this terminology, I think Harrison makes a persuasive case that the literature on fit is an exemplar of weak paradigm development. 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 uni-

versalism (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, and so on. It is true that, to some degree, these arguments could be made about most any OB topic. However, as Harrison notes in specific cases, questions of "what is fit" loom large over the area. As Schneider (2001) concluded, "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. Harrison's chapter reminds us that the fit literature has problems. To some intrepid researcher, problems become opportunities, but, today, problems are problems.

To his credit, Harrison proposes the beginnings of one path to stronger paradigm development. He seeks to delineate what is, and as importantly, what is not, fit. It is possible that his effort does not clarify but rather shifts ambiguity in a different direction. For example, he argues that commensurability is critical to a useful definition of fit. From this assumption, he then rejects alternative conceptions of fit that do not, in his view, 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 Harrison's test of commensurability. Would this, then, not be a study of person–organization fit? I wonder whether researchers will see fit (excuse the pun) to follow Harrison's lead. I do believe, though, that to engage his argument is critical if the field is to develop a stronger paradigm and realize the consequent advantages as suggested by Kuhn (1996).

FUTURE FIT RESEARCH

Having reviewed the chapters and made note of their many contributions, I conclude my review by noting some areas for future fit research. Of course, there are many such areas suggested in the chapters themselves,

which I do not repeat here. Some of the topics below pick up on some of the points made in the chapters, and some are ideas that arose from or were reinforced by reading the chapters.

Terminology Confusion

As the astute 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 such as 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 that the situation has improved. We are swimming in terms and concepts and, in some ways, this book has only added water to an already overflowing pond. I think, rather obviously, that the key is not stop to conceptualizing but to emphasize works that will integrate the terms/concepts and to encourage "ideational consolidation" (Felps, 2006). Although some of chapters make progress toward integration, I think the field awaits more integrative work. The recent Edwards et al. (2006) article, I believe, is one step in the right direction. We need more, lest we act out the Chinese proverb, "One step forward, two steps back."

The Problems of Endogeneity and Selectivity Biases

In my doctoral education at Illinois, labor economists were abundant. At times I felt that 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 noted, "I think the point cannot be overemphasized: When either the person variable or the environmental variable (or both) is 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 indi-

vidual'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, because 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 possible 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 to 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 (such as 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 structural equation modeling) 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.

Changes in Fit

Fit is, by implication, a state that may vary over time. It is true that, depending on how one defines fit (e.g., values or traits), both the person and situation component may be relatively stable. However, even in these cases, neither the person nor the situation variable is 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 et al., 2003). There are good longitudinal studies in the fit literature. However, there are not many researchers who have looked at

changes in fit over time nor who have attempted to decompose variance into between-individual (individual differences in fit) and within-individual (intraindividual variation in fit over time) sources. Studying fit in such a multilevel context would seem to be a particularly promising area for future research.

Is Fit More Illusory Than Real?

In one of his more interesting (and certainly controversial) points, Harrison argues that direct measures of fit (meaning overall perceptions of fit) are so meaningless as to deserve abolishment from the literature. Although most, me 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 his or her organization? Recently, in another article, Harrison, Newman, and Roth (2006) found that specific job attitudes (job satisfaction or 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 notes in her section in chapter 11, 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?

Statistical Issues

Edwards and Harrison each make persuasive cases for the problems with indirect (*D*-scores, profile similarity indices) and direct (global fit perceptions) assessments of person–organization fit. By and large, I agree

with their criticisms. 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, $A \times B$ 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) noted: "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 nonissue." That may be true enough if a pure null hypothesis significant testing approach were followed (I hesitate in using *pure* and *null hypothesis significant testing* 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 their articles. Thus, in practice, surface plots, perhaps to an unusual degree, are as much a product of induction as of 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 $A \times B$ 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. What I am asking is this: Given the power problems with moderated regression (McClelland & Judd, 1993), much less with polynomial regression (which includes interactions, quadratics, and interactions with quadratics), and the fact that many of the surface plots may have been inductively derived (at least in part), what is the probability that each significant coefficient (which is critical to the nuanced interpretation of polynomial regression) would be replicated in a future study? Unfortunately, few such efforts are visible in the literature, which makes the question all the more salient.

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 with people whose values do not match (Chapman, Uggerslev, Carroll, Piasentin, & Jones, 2005). Similarly, individuals who are mismatched 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) noted, 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 to be 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 Type Indicator acronyms, if someone learns that he or she is an INFP and his or her supervisor is an ESTJ, how does such feedback influence appraisals of fit? Feedback from others is a powerful source of beliefs about our selves, and fit should be no exception. As Stryker (1987) noted, symbolic interactionism has garnered more than its share of criticism, some of which is justified, but it continues to be of interest to social psychologists. It may have a role in fit research as well.

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 ×

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 is 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, as Edwards and Shipp's chapter implies, we have not conceptualized or investigated the relationship in the right way. By and large, the personnel selection literature has been focused on abilities but, again, at least as far as individual differences are concerned, the assumption has been the more ability, the better. And the evidence does not seem 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 industrial–organizational 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.

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. Ellis and Tsui argue that fit is socially desirable, and Kammeyer-Mueller notes that

fit is an implicit outcome in socialization processes. Clearly, though, if one tries to reconcile Drucker's comment with the beliefs that (a) fit is desirable from the vantage point of an organization (Ellis & Tsui) and (b) organizations attempt to increase fit through selection and socialization processes (Kammeyer-Mueller), 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 them. 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?

Fit in Terms of What?

With few exceptions (Kristof-Brown, 2000), little research has been done to compare different operationalizations of fit. Is fit around values more important in driving fit perceptions and outcomes than fit around personality or goals? And, if personality and values are important, which aspects are most important? Even if one is concerned only with values, there are different types of values. Rokeach (1973) defined values as "desired end states" or "ways of being." Locke (1976) defined values more narrowly, as "that which one acts to gain and/or keep" (p. 1304).

As noted by Edwards and Schipp and Harrison, omnibus measures of fit collapse across all values of traits, obscuring important practical and conceptual information on whether fit on some values or traits is more important than fit on others. In the relationship literature, research has suggested that fit in terms of the Big Five traits operates quite differently by trait. For extraversion, there is evidence to support complementarity, such that dominant interaction partners prefer submissive partners and submissive partners prefer dominant ones (Tiedens & Fragale, 2003). For other traits, similarity appears to rule the day for which, of the Big Five traits, similarity in conscientiousness may be most important to marital (Nemechek & Olson, 1999) and roommate (Kurtz & Sherker, 2003) satisfaction. And, finally, there are traits for which fit seems unimportant. For example, the main effect of the agreeableness of one's spouse seems to be more important to marital satisfaction than how similar one is to one's spouse in agreeableness (Kurtz & Sherker, 2003). The point is that if we are going to learn more about what really drives fit, we need more comparative studies of personality, values, and goals and finer-grained analyses of fit by each specific trait, value, or goal.

CONCLUSION

[AQ11]

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 solutions for some of these formidable problems. Will it not be exciting to see what that future holds?

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[AQ1] Per publisher's style, italics are not used for emphasis.
[AQ2]"organizational behavior" as meant by OB?
[AQ3] Edit OK to avoid repetition of "unusually"?
[AQ4] changes per actual wording in chapter 7.