Industrial and Organizational Psychology, **4** (2011), 426–432.

RESPONSE

Innovative Ideas on How Work–Family Research Can Have More Impact

ELLEN ERNST KOSSEK
Michigan State University

BORIS B. BALTES
Wayne State University

RUSSELL A. MATTHEWS
Louisiana State University

Abstract

The commentaries on our focal article agreed with its main premise that work–family research should follow new strategies to improve its practical impact, and made suggestions clustering into three main themes. The first theme built on our suggestion to improve the research focus, terminology, and framing of work-family research. These essays offered additional ideas such as decoupling work-family from work-life research, and examining contextual factors more deeply. The second theme focused on how to better apply the findings from work family research. These commentaries provided social change approaches for making work-family issues more central to key stakeholders and to organizations. The third theme focused on broadening our scope to the societal level. These editorials advocated tactics supporting the development of basic rights of work–life balance within and across nations.

We were excited to learn that a record-breaking number of commentaries were written in response to our focal article. Work–family research seems to be not only timely but also controversial. The 13 commentaries selected by reviewers clustered into three main themes. Authors argued for improvements in research focus, terminology, and framing; in the application of work–family findings; and in broadening researchers' lens to the societal level.

Correspondence concerning this article should be addressed to Ellen Ernst Kossek.
E-mail: kossek@msu.edu
Address: School of Human Resources and Labor Relations, Michigan State University, East Lansing, MI 48824

Strategies for Focusing Research to Have Greater Impact

The first cluster of commentaries suggested broad changes to the topics and methods currently used by mainstream researchers. Grawitch, Maloney, Barber, and Yost (2011) argue that rather than surveying employees with measures developed a priori by researchers to assess specific work–life demands that may have little psychological meaning to the respondent, personal projects analysis (Emmons & King, 1988) methodology could be used. A personal projects analysis approach asks individuals to identify what demands are most salient to them. We think this approach has promise for the development of individual self-improvement strategies and should be piloted. However, we worry that its highly

ideographic and microfocus may not have broad actionable impact for a swath of employees across organizations. We would like to see more analysis on how to link improved policy to such personal projects analysis research.

Grawitch and colleagues also took issue with our suggestion to keep using the term ''work–family.'' They suggested ''work–nonwork'' as the preferred terminology. We were perhaps misunderstood and needed to be clearer in our explanation. We argue that researchers should measure the family caregiving role separately from other nonwork roles. We are not suggesting one term is better than another, rather that researchers be thoughtful and specific in the labeling and measurement of their constructs. We do not want to see the family caregiving role subsumed in nonwork measures as it is a unique demand that is qualitatively and quantitatively different in its relation to the work role. A recent study by Huffman, Youngcourt, Payne, and Castro (2008) generally supports this point. The authors determined that, although related, measures of work–family conflict and work–nonwork conflict are systematically differentially related to a variety of outcome measures as a function of the presence of dependents or not. As such, Huffman et al. provide preliminary evidence that measures with different foci (i.e., work–family vs. work–nonwork) are related but function differently.

For example, one might be able to reschedule when to exercise, go to continuing education, meditate, or volunteer. However, it is more challenging to reschedule attending to a serious problem faced by a parent in a nursing home or taking a sick child to the doctor. Thus, we were simply taking issue with the historical evolution in corporations to morph work–family into ''work–life'' as a public relations move to make corporate support of working caregivers less visible and did not want researchers to unconsciously follow this jargon path.

Our view to still focus research on work–family relationships is consistent with Rothausen's (2011) contention in her commentary that we need, as researchers, to ''unpack dependent care'' from work–family research. We couldn't agree with Rothausen more. Rothausen also makes the similarly astute comment that we need to unpack ''work–life'' as well to focus on specific roles in order to countervail similar obscurement. We agree with this as well; however, we also worry about participant survey fatigue. Will individuals now have to answer work versus nonwork scales for every nonwork role they engage in? That sounds a bit tedious.

Rothausen also suggests that work–family research itself is still stigmatized in industrial–organizational (I–O) psychology and organizational behavior (OB) research, despite the fact the dependent care, gendered, and other nonwork roles deeply impact work attitudes and constructs. She argues that work–family, work–life, and dependent care issues should not be looked at *in addition* to other work constructs. Rather, these issues should actually be incorporated into the measurement of core work concepts such as job satisfaction, stigma, and other core work constructs, which is a novel idea. Given that recent surveys (e.g., Gerdes, 2009) seem to suggest that Generation Y employees list work–family balance as one of the most important factors in deciding on where to work, the idea of including work–family conflict, for example, alongside job satisfaction as a major attitudinal variable makes perfect sense.

Cunningham (2011) makes the observation that interrole conflict is not inherently bad but part of life. In his commentary, he observes that simply focusing research on the provision of role flexibility does not necessarily reduce work–family conflict and actually is misguided. He argues for studies to focus on how to increase employee self-awareness, which will enable them to better understand and prepare for role conflicts over the life course. Such research could teach employees how to effectively

manage role demands and expectations. We strongly encourage more research and interventions focusing on these issues to enhance improved management of self and communication of needs and seeking of social support. We agree that focusing research on the individual is a good place to start. Yet given that individuals and groups socially construct their lives and choices as a function of their organizational and social contexts, as discussed in several of the other commentaries, a focus on individuals is insufficient for social change and improved research insight. We hope to see more contextually based research to help individuals develop positive strategies in situ.

Such a view is consistent with the observation from Agars and French (2011) that contextual factors, especially those relevant to specific populations (e.g., immigrants, military, lower SES workers), have been severely underexamined in the mainstream literature. We agree wholeheartedly with their observation that these special populations nested in contexts do not merely add variance but actually change the meaning of the work–family phenomenon. For example, low income workers and immigrant workers face high strain in both work and nonwork contexts. Therefore interventions designed to only reduce work strain may be ineffectual without also attending to the context of family strain. Similarly, the meaning of work–family relationships shifts for special populations (e.g., Matthews, Booth, Taylor, & Martin, 2011; Repetti & Saxbe, 2009). Low income workers with young children, for instance, may see working as a way to be better parents by being role models and providing basic economic needs. Individuals in military families may see constantly adjusting the family to adapt to the military demands as a positive development, given nearly total systems overlap between personal and professional life.

The last article with the theme of changing researchers' focus was by Johnson, Kiburz, Dumani, Cho, and Allen (2011). They argue that scholars need to worry less about applied impact as it is a secondary goal. They maintain that researchers' primary responsibilities are to conduct programmatic scientific work based on sound theory and methodology. They suggested researchers might refocus work–family research questions to follow evolving developments in the practice, such as the increased interest in workplace flexibility. Although we agree researchers should follow new trends, we also think it is important that work–family scholars examine employer motives and continual reframing of the work–life arena. For example, based on recent conversations of the first author with corporate leaders, we see the interest in flexibility as being motivated more as a business process improvement tool and a way to cut costs, such as saving on real estate, than on actually helping employees have improved work–life relationships. Additional evidence of employers increasing tendency to view work–family issues primarily through the cost lens comes from Johnson and colleagues sanguine observation that employer support of paid maternity may have only plateaued since 2006 and employers have reduced the amount they contribute to total disability benefits.

How to Better Apply Work–Family Research Findings

The second cluster of commentaries gave advice on how to better apply work–family research findings. Aumann and Galinsky (2011) of The Families and Work Institute argues that a main reason work–family policies have failed to live up to potential is because work–family researchers have too often focused too narrowly on work–family issues as an end in themselves and overlooked broader business needs and human resource systems. Consequently, work–family practices often fail to be incorporated into overall job design and work cultures and are marginalized as a benefit. They call for work–family issues to be framed as part of business effectiveness more generally. Benchmarking current flexibility initiatives, norming practices against other employers, giving

out educational technical assistance, and making awards for best practices are strategies suggested by Aumann and Galinsky. Although we agree with these strategies, we caution again on having action research mainly driven by business interests as the lead stakeholder. A multiple constituency approach to work–life initiatives as human resource innovation needs to be followed (cf., Kossek, 1989).

We also recommend that action research on the efficacy of these strategies be scientifically assessed by impartial evaluators. Such on going research partnerships will better marry business initiatives and informal state and national policy with scientific evaluation. These studies could augment the research Galinsky has done by adding much needed topics understudied in the work–family field. More scholarship is needed on, for example, positive strategies on how organizations and managers learn and move work–family issues to a mainstream agenda or on the design of innovative naturally occurring quasi-experimental studies with pre- and post implementation evaluation and control groups. Studies are also needed that include employers who are not only leaders of best practices in implementing work–family but on the cutting edge of many progressive people practices. That is, more work is needed on traditional and even nonprogressive firms. This later group needs to be studied to better understand the process of organizational change to help bring along laggards in implementation up to basic requisite work–life support.

Senior government psychologist Wells (2011) also suggests a change management approach to improve the application of work–life research. Rather than viewing managers as merely barriers to change, in her commentary she argues for the adoption of a sensemaking perspective. Work–family best practice implementation would be enhanced by attending to how key stakeholders struggle to assign, integrate, and take action on evolving meaning. Yet few studies take an organizational change and multiple stakeholder approach and understand member responses to work–family issues in their own terms. Wells uses telework implementation as an example of a practice that disrupts managers' abilities to manage basic acts such as coordinating a simple meeting. We concur with Well's that more attention needs to be given to work–life programs as organizational change with uneven implementation. Such research should examine how policies are adopted as a privilege for some workers and not others and have unintended consequences and even negative outcomes from use. Finally, we concur with Well's that an organizational change perspective would allow researchers to more carefully operationalize the extent of change and the dimensions of the policies as human resource innovations (Kossek, 1987). For example, work–life policies can be vastly different in implementation and design. As an example, telework can range in application from being ad hoc in terms of one day a week to a 100% virtual office.

Deuling and Mallard's (2011) argument for a standardized typology of work–nonwork policies along dimensions might also help address the issue that policies similar in name may vary greatly in application. They suggested that typologies might be developed identifying the audience served, resource needs, number of workers eligible, and fit to the organizational culture. A typology approach might enable a needs assessment approach for implementation as policies would then be implemented that have the best fit with employees needs. Return on investment (ROI) could also be calculated to estimate the ''biggest bang for the buck'' program. Although we think the idea of a typology has promise, and of course who could argue with organizations assessing employees needs and ROI, it may be that such an approach will lead organizations to only attend to the needs of the majority of the workforce. As noted by Repetti and Saxbe (2009), work–family scholars should be careful to assume a ''one-size-fits-all'' mentality. For example, single parents, those with handicapped children or disabled adults, employees with mental health

conditions, or those with debilitating illnesses, who add value to their jobs, should be able to receive some support. Perhaps here is where partnering with other firms or the government to provide special needs support may be needed to ensure applications are addressing needs unmet by mainstream programs. Although obviously ROI and needs assessment focused on the majority makes business sense, some additional strategies are needed for special populations, and employers may not be willing to foot the bill alone.

In a similar vein to the needs assessment argument, a fundamental I–O practice emanating from training research, Major and Morganson (2011) argue in their commentary for a return to other basic I–O applications such as job analysis, leadership training, and performance appraisal. With their colleague, Bauer, they have developed a Work Life Job Analysis tool to summarize the work–life demands of a position, arguing that work characteristics can explain half the variance in work–family conflict between occupations. Job analysis could then inform a realistic job preview and could be added to O-Net. They also argue that performance appraisals should hold leaders accountable for support of work–life demands as rating criteria. Leadership effectiveness training in work–life support with pre- and post test assessments can also be conducted. Although all these ideas are laudable, implementation seems difficult. It is likely organizations are going to balk at the idea of holding leaders accountable for support of work–life demands of their employees. Furthermore, for what demands would they be held accountable? As we and several commentaries have argued, a one-size-fits-all mentality is inappropriate. Thus, incorporating work–family issues into a general performance appraisal paradigm for managers may be highly problematic. A managers' ability to effectively support an employee may be functionally outside their control. That is, the demands of an individual's family may not be amenable to support from a supervisor, or the nature of the job may preclude a manager from being supportive in the first place.

Broadening Our Lens to the Societal Level

Although the focus of our article was on organizational impact, we generally applauded and agreed with all of the papers suggesting broadening the work–life agenda to the societal level and enhancing publicity on its general social importance in a third and final cluster of responses.

Huffman, Sanders, and Culbertson (2011) aptly noted in their commentary that work–family research does indeed have a public relations problem. They contend that framing work–life initiatives as needing to mainly align with and meet business interests as the primary stakeholder is problematic. Such an approach leads work–life issues to be viewed as a ''nicety and not a necessity.'' Because research shows work–life policies do not improve the bottom line or performance in the short run, work–life policies face a stigma similar to what is faced by social responsibility or corporate sustainability domains. Furthermore, work–life issues are value laden so that even if corporate financial benefits could be made clear, issues of fairness and ''perceived lifestyle choices'' rise to the surface, in effect paralyzing meaningful corporate action. We agree that work–life research must take a broader societal humanist perspective in its objectives.

Ollier-Malaterre (2011) also argues that the business case argument is simply not enough to force deep change in organizations and society. In her commentary, she maintains that it is not organizations we necessarily need to convince on the value of work–life balance but society and citizens directly. She advocates for the development of a floor of basic rights of work–life balance, such as being provided in many European countries. Her vision is one of a balanced society where individuals can have more choice in how they articulate life roles. Such a vision would better meet the needs of the often silent and invisible

stakeholders—children—although we also point out the often neglected elderly. She gives a wonderful example of how a French government think tank recommends that active grandparenthood with family over the life course benefits society as a whole in developing communities and future citizens. Similar recognition could be given to part-time workers and father involvement with caregiving to increase the value of caregiving and breadwinning. We agree that cross-national cultural research and practice teams may be helpful to broaden how we study and frame the benefits and impact of work–life initiatives.

Over 95% of work–life research has been published based on U.S. and UK samples, and we need to move away from seeing the U.S. value of work–life relationships as the standard for work–life research. To add new knowledge on this issue, Ollier-Malaterre is editing a special issue of the *European Management Journal* on national context in work-life research (with Valcour, Den Dulk, and Kossek). Such work may lead us to new theories and thinking about values, norms, and framing of work–life research.

Leslie and Manchester (2011) argue that we need to move societal framing away from seeing work–life as a women's issue, which they say confines work–life to low status in organizations and society. In their commentary, they suggest ''degendering'' work–family conflict. Publicity is needed to increase awareness that both genders experience work–family role demands and that work–life needs to be moved out of diversity departments into organizational effectiveness areas. Mainstreaming work–life will improve use of policies by all kinds of workers within organizations and across society. As an example of the later, they point to men's increased use of work–family policies in countries such as the Netherlands. We agree that work–life effectiveness now has to be moved from a workforce inclusion issue to being viewed as an aspect of effective job design and working conditions.

Las Heras and Grau (2011) also argue that we need to broaden work–life research to the societal–cultural level. Such an approach will move us away from taking as a given prevailing corporate and societal cultural norms regarding work–life issues but would shift scholars to interpret and question prevailing norms. However, in their commentary they advise academics to be positive in their critical views to target actionable change. They suggest that researchers partner with business, associations, unions, and government or other groups to contribute positive ideas on how to develop solutions to improve work–life relationships. Scholarly research can also be used to improve societal publicity on the importance of preventing the hazards of lack of work–life balance such as absenteeism, increased health costs and suicides, and children's poor academic and social well-being.

In conclusion, we were excited to see the amount and quality of the commentaries on our focal article. We believe this speaks to the importance of work–family research both to academics and practitioners. It appears that most of the commentaries agreed with our focal article's main thrust that more nuanced approaches (both theoretically and methodologically) are needed for work–family research to have a greater impact. The commentaries also raised several additional avenues for improved research focus, research translation, and societal impact. Overall, we hope the combination of the ideas presented in our focal article and the commentaries will lead to new and important future work–family research. The effectiveness of families, work organizations, and society as a whole are likely to be positively influenced by ''better and different work–family research.''

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