

# LETTERS

## JOB STRAIN AND HEALTH-RELATED LIFESTYLE

Because of several significant methodological and interpretational errors in Heikkilä et al.<sup>1</sup> pertaining to job strain and health-related lifestyle, we disagree with the authors that

reducing work-related psychosocial stress . . . is unlikely to be an important target for any policy or intervention aiming to influence health-related lifestyle factors or overall lifestyle.<sup>1(p2095)</sup>

First, this contradicts the literature review in the introduction, in which the authors state that “psychosocial stress at work has been shown to be associated with individual unhealthy lifestyle factors,”<sup>1(p2090)</sup> as well as the authors’ conclusion in another article, in which the authors write that “interventions to increase physical activity in the population may benefit from taking workplace factors into account.”<sup>2(p1087)</sup> Second, although they found a significant association between job strain and unhealthy lifestyle in their cross-sectional analysis with 11 studies, there was no significant association in their longitudinal analysis with four studies. There were different definitions of “unhealthy lifestyle” in their longitudinal analysis (“having 1-4 unhealthy lifestyle factors” in the online supplemental table) and in their cross-sectional

analysis (“having all 4 unhealthy lifestyle factors” in Table 2).<sup>1</sup> Because of a large variation among those who have “1–4 unhealthy lifestyle factors” (e.g., 32% had one unhealthy lifestyle factor and 3% had all four unhealthy lifestyle factors in cross-sectional studies), we believe that the authors should have reported the longitudinal association between job strain and unhealthy lifestyle defined as “having all 4 unhealthy lifestyle factors” as in the cross-sectional analysis. Third, the authors should have discussed two probable biases in their longitudinal analysis, a differential exposure misclassification,<sup>3,4</sup> and a differential attrition rate by job strain status.<sup>5</sup> The authors used only a one-time (baseline) measure of job strain in their longitudinal analysis. Furthermore, their other article<sup>4</sup> that uses the same four cohort studies indicates significant exposure misclassification—58% of the people in the job-strain group at baseline changed to the nonjob-strain group at follow-up, and 11% of the people in the nonjob-strain group at baseline changed to the job-strain group at follow-up.

In addition, Clays et al.<sup>5</sup> reported that considerably more people in the job-strain group (at baseline) than in the nonjob-strain group dropped out during the follow-up period in the Belstress study, one of the four cohort studies. These errors indicate an underestimation of the association between job strain and unhealthy lifestyle in Heikkilä et al.<sup>1</sup> Lastly, this article contained other errors such as obesity being described as a “lifestyle” factor<sup>6,7</sup> and an underrepresentation of workers with job strain in their individual studies.<sup>8–10</sup> ■

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### Contributors

All authors contributed substantially to the conceptualization and design of the letter and the interpretation of data. B. Choi drafted the letter, and S. Ko, P. Landsbergis, M. Dobson, and P. Schnall revised it.

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## POTENTIALLY MISLEADING CONCLUSIONS: JOB STRAIN AND HEALTH BEHAVIORS

I read with interest the recent article examining the relationship between job strain and health-related lifestyle factors.<sup>1</sup> Given the findings presented in the study I was perplexed by one of the authors' conclusions that

reducing work-related psychosocial stress . . . is unlikely to be an important target for any policy or intervention aiming to influence health-related lifestyle factors.<sup>1(p2095)</sup>

This conclusion is misleading for multiple reasons. Most importantly, it does not reflect the findings presented in the study. Job strain and passive work were both associated with a reduced probability of adopting a healthy lifestyle (Figure 1).<sup>1</sup> A policy approach that encourages people to adopt a healthier lifestyle could target low job control.

There are also methodological inadequacies in the longitudinal analyses. These include the crude methods used to measure change in lifestyle, whereby a respondent who stops smoking, but continues to be inactive, would be considered to not have engaged in a healthier lifestyle. More rigorous methods to measure change are available<sup>2,3</sup> and should be implemented. Furthermore, job strain was only measured on one occasion, likely resulting in weaker relationships that if two assessments had been used.<sup>4</sup> And, if the impact of job strain on unhealthy behavior occurs relatively quickly, it would be missed by all the studies included (of which two years was the shortest follow-up). Note that these limitations are in addition to the general limitations associated with the harmonization of job strain and health behavior responses across the various cohorts.<sup>5</sup> In light of these methodological deficiencies, it seems premature to dismiss the impact of job strain on health-related lifestyle factors until a more rigorous analysis has been undertaken.

Finally, job strain does not equate to all psychosocial stress.<sup>6</sup> It is entirely possible that other dimensions of the psychosocial work environment such as effort–reward imbalance, lack of social support, or organizational injustice may have an even larger impact on health behavior change than job strain does in these analyses.

Individual-level data across multiple cohort studies bring with it great power—both

statistically and publication-wise (because of the opportunity to publish relatively simple analyses in top-tier journals). But with power also comes responsibility. The authors involved in the Individual-Participant Data Meta-analysis of Working Populations analyses have the responsibility to present conclusions that are not misleading in relation to the potential importance of job strain, or to implicate other dimensions of the psychosocial-work environment on health behaviors and other health outcomes. ■

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### HEIKKILÄ ET AL. RESPOND

We appreciate the interest in our research by both Choi et al. and Smith and welcome their critical reading of our study.

Choi et al. point out that we used different definitions of an unhealthy lifestyle in our cross-sectional and longitudinal analyses

(4 unhealthy lifestyle factors in the cross-sectional analysis; 1–4 unhealthy lifestyle factors in the longitudinal one). Extreme shifts in lifestyle, from healthy to unhealthy or vice versa, are rare. Among the 43 971 participants in our longitudinal analysis, one person moved from a healthy (4 healthy lifestyle factors) to an unhealthy lifestyle (4 unhealthy lifestyle factors) and no one moved from an unhealthy to a healthy one. For these reasons, we investigated changes from "not healthy" (1–4 unhealthy lifestyle factors) to "healthy" (as in the cross-sectional analyses, 4 healthy lifestyle factors) and from "healthy" to "not healthy."

Choi et al. write that we could have discussed two possible biases in our longitudinal analyses—differential exposure misclassification and differential attrition by job strain status. This is a valid point. Such biases may have diluted some associations in our analyses. The authors also state that regarding obesity as a "lifestyle factor" is an error. We realize that personal choice is not the only factor influencing obesity and that using the term "lifestyle-related factor" may have been more accurate. However, for convenience, obesity is often labeled as a lifestyle factor in research studies. We doubt that following this convention has caused significant confusion in the interpretation of our findings.

We are unsure why Choi et al. suspect that workers experiencing job strain would be underrepresented in the IPD-Work (Individual-Participant Data Meta-Analysis of Working Populations) Consortium. They have previously suggested that the Consortium studies<sup>1–4</sup> include mainly white collar workers who may be less stressed. However, this is not the case. Of the eleven studies in our analyses, six were population-based samples from the general workforce (KORA 1–3 studies, HeSSup, and the 2 WOLF studies). Four studies were workplace-based but included participants from across the socioeconomic range of occupations (Belstress, FPS, Gazel, and HNR). Whitehall II was the only study of white collar workers. It is unlikely that the inclusion of this study would have substantially diluted the associations in our analyses.

Choi et al. and Smith were concerned with the following sentence from the Discussion section:

[A]lthough reducing work-related psychosocial stress would undoubtedly increase the psychological well-being of the working population, it is

unlikely to be an important target for any policy or intervention aiming to influence health-related lifestyle factors or overall lifestyle.<sup>(p2095)</sup>

To clarify, we do not regard it as a statement that interventions to reduce job strain or work-related psychosocial stress would have no impact at all on health-related lifestyle. Our findings suggest that the association between job strain and health-related lifestyle is relatively weak and the longitudinal analyses did not consistently support a temporal association, as noted in “Conclusions.” Given this and the fact that observational studies such as ours tend to indicate stronger associations than can subsequently be confirmed in randomized controlled trials,<sup>5</sup> it seems likely that policies or interventions to reduce job strain would not have a marked impact on health-related lifestyle.

Smith was also concerned about our having measured lifestyle changes inadequately; a respondent who stops one unhealthy behavior but continues another was considered not to have engaged in a healthy lifestyle. We aimed to investigate the co-occurrence of multiple lifestyle factors, which we referred to as healthy and unhealthy lifestyles. Thus, though stopping one unhealthy behavior is undoubtedly good for any individual, we would not consider stopping smoking and continuing to be physically inactive, for example, to constitute a change to a healthy lifestyle. Smith writes that job strain measured at one time point only may have diluted our estimates. However, we had two measurements of job strain available and we investigated the associations between job strain at baseline and at follow-up with healthy and unhealthy lifestyles at both time points in Table 3 of our study.<sup>(p2094)</sup>

Smith suggests that the follow-up in our analyses may not have captured possible short-term associations between job strain and health-related lifestyle. We understand this concern; further research into the possible short-term lifestyle consequences of any measure of work-related stress would elucidate this. However, long-term rather than short-term lifestyle changes are likely to influence the risk of chronic diseases. Finally, Smith states:

Individual level data across multiple cohort studies brings with it great power. . . . But with power comes responsibility. The authors involved in the Individual-Participant Data Meta-analysis of Working Populations analyses have

the responsibility to present conclusions that are not misleading in relation to the potential importance of job strain.

We agree. We think it is important to publish findings and to present conclusions that are backed up by data and analyses, even if they do not support those of some of the previous studies or commonly held beliefs. ■

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### Human Participant Protection

Each IPD-Work Consortium study was approved by the relevant local or national ethics committees. All participants provided informed consent.

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