

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.¹ 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.^{1(p2095)}

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).¹ 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^{2,3} 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.⁴ 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.⁵ 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.⁶ 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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*This letter was accepted October 20, 2013.
doi:10.2105/AJPH.2013.301758*

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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^{1–4} 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