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Health effects of reduced workhours?

In a recent paper in this journal, Wergeland et al (1) concluded that shortening regular workdays from ≥7 to 6 hours "may considerably reduce" the prevalence of neck-shoulder pain among persons with physically demanding care work. We do not believe that the material presented by the authors supports this conclusion.

The reduced prevalence of neck-shoulder pain reported in the study appears to be impressive. The prevalence was reduced from 40.9% to 25.6% in Oslo, from 57.1% to 39.1% in Helsingborg, and from 81.6% to 68.3% in Stockholm. The authors reported an average reduction of 58.7% to 40.4% for the intervention groups. For their "reference" groups the average prevalence was 41.9% before the intervention and 41.3% after the intervention.

Our main concern is the initial difference between the intervention groups and the reference groups. The data show that the initial high prevalence of neck-shoulder pain was reduced to "normal" after the intervention. This finding may, of course, have been an effect of the intervention. However, since no randomized control group was included in the study, we will never know whether such an effect occurred or not. Are we faced with yet another regression towards the mean or the normal ups and downs of "chronic" pain conditions? Regression towards the mean is the main pitfall in uncontrolled studies. As the saying goes, what goes up must come down. If treatment is started when the values are extreme, the values will become more "normal" no matter what is done.

There are also many other problems with the paper. While references are made to several unpublished reports that may contain more information about the methodology, there is no proper reference to international peer-reviewed publications. How were the intervention groups selected? Were they selected because of their high prevalence of pain, or was this initial difference merely a simple lack of luck? What did the participants expect from the short workhour program and what was their motivation for joining the program? What instructions did the participants receive, and what did the researchers expect? Even if the study had been done properly with a randomized control group with the same level of pain, the experiment and the publicity around it may have biased the results and produced an artefactually high level of effect (Hawthorne effect, subject bias, experimenter bias, natural history of neck pain, sociopolitical bias, in addition to the regression towards the mean effect).

We also question the use of a one-tailed test for these data. One-tailed tests are rarely appropriate (2). They should be used only when it is reasonable to consider that a real difference can occur in only one direction. We do not believe that a one-tailed test was justified in this case. Even if the authors found it unlikely, reduced hours may have been the wrong form of treatment, since remaining in usual activity is recommended for patients with neck and back pain (3). The slight increase the authors found for back pain in Oslo and Helsingborg suggests that the results could have been in either direction.

Wergeland et al (1) report a one-tailed P-value of 0.034; this value would be unlikely to reach an acceptable or conventional significance level in a two-tailed test. The "effect" is also weak for the 1-year follow up, with a risk ratio of 1.08 (95% confidence interval 0.84–1.37) for neck-shoulder pain in the reference group when compared with the intervention group. In other words, there was no difference at all due to the reduction in the number of workhours.

We accept that 6-hour workdays may have several beneficial effects on many employees. We also accept that 6-hour workdays may be immensely popular, at least for some employees for some time. However, we do not feel that this publication is of any help in deciding what is really good for neck-shoulder pain or what the health consequences really are for shorter or, for that matter, longer workhours.

References

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Authors' reply

We thank Hege Eriksen and her co-workers for their important remarks (1) in relation to the nonexperimental design of our study (2). They argue in essence that a true experiment with randomized control groups is required to justify our conclusion. We agree that the possible impact of statistical regression should have been dealt with explicitly in our paper. However, we consider our conclusion to be well justified, that a reduction in workhours "may considerably reduce the prevalence of neck-shoulder pain among persons with physically demanding care work [p 27]". Research, whether experimental or not, can only draw tentative conclusions. Future research may or may not support our conclusion.

Regression to the mean would reduce the prevalence of neck-shoulder pain in intervention groups if their baseline measurements were higher than their true mean. No data were available to establish whether this was the case. Additional preintervention measurements would have been helpful, together with an extended intervention period and postintervention measurements, as recommended in our paper.

Within each project, intervention and reference groups (institutions) were selected according to the same criteria, which were mainly organizational. Sickness absence above average was indeed suggested by local politicians as a particular criterion for intervention in the Oslo project, but it was not applied. With regard to possible misclassification of pain according to group membership (including a Hawthorne effect), we refer to the discussion section of the original paper. Bias due to publicity or expectancy, as mentioned by Eriksen et al (3), is unlikely. The municipalities initiated their projects to explore a wide range of effects, from economic consequences to quality of service, including general measures of employee health. Neck-shoulder pain was not an outcome of specific interest in the original projects, but was recorded among a multitude of other symptoms from various organ systems.

We welcome randomized controlled trials in this field, but not as the only legitimate source of knowledge. Random assignment may be feasible and appropriate for interventions consisting of the treatment or training of individuals (3). Random assignment of individuals or clusters (institutions) to 6-hour workdays (with full-time salary) over 2 years is less feasible, for economic and practical reasons. And methodological problems will prevail. The setting would probably differ from a natural work setting to an extent that threatens external validity. Unwanted social and organizational effects, due, for example, to the desirability of the intervention, may confound the results.

Knowledge about the health effects of reduced workhours is limited. The few studies that have been carried out have typically taken the form of "natural experiments". Our paper is an attempt to document the results of three such "experiments" and make them available to the scientific community. Workplace interventions to improve productivity, quality, and work conditions are continuously carried out by management in real worklife. Such interventions have a scale and a sustainability rarely achieved in projects initiated by scientists (4). To make a randomized assignment of intervention an absolute requirement would mean to discard these sources of knowledge. The focus of workplace intervention research would have to shift from primary prevention through the modification of work conditions to the treatment of individual workers.

References

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