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Reply

In the first place, we would like to thank Drs Andersen, Kryger, Lassen and Mikkelsen for reading our article carefully and raising some important questions. The words 'symptom', 'pain', 'disorder' and 'disease' are indeed used indeterminately in epidemiological articles. The terminology varies according to the clinical setting. The questionnaire used in our paper [1] is based on the modified Nordic questionnaire [2], which uses trouble as an outcome variable meaning 'ache', 'pain' or 'discomfort'.

Our outcome variable in the results and discussion sections of our paper is 'pain', although we used 'disorder' in the title and introduction as a result of reference to other articles. As far as the definition is concerned, a dictionary [3] defines disorder as 'a disturbance of the physical or mental condition'.

The EU Agency report [4] encourages health monitoring and surveillance systems to consider musculoskeletal disorders also without specific diagnosis or pathology.

We agree that cross-sectional studies are liable to biases, as we discussed [1] in the latter part of our paper.

Follow-up studies on this topic are few. The study by Kryger *et al.* [5] indicates that work with a computer mouse for >30 h per week involves an increased risk of forearm pain. Their 1 year follow-up produced an insufficient number of 'pure' incidence cases on which to draw any conclusions. In addition, they found that 5–9 h of work with a mouse per week was a risk factor for forearm pain, but they did not consider it as a true physical effect because of the lacking threshold. Many other problems also exist in follow-up studies: for example, variation in working time according to season, personal factors, and assessment of active mouse and keyboard time by using questionnaire instead of real-time measurements (CPU interruptions).

In our opinion, the effects of work with a keyboard and mouse should not be measured separately; instead, the time with the keyboard and mouse (hand using mouse) should be combined in some way. Consider a VDU worker who uses a keyboard for, say, 6 h and a mouse for a few seconds every minute. The daily mouse time is very short, but use of the mouse may cause stress due to the unstable use of the upper extremity and the load from activated muscles moving the arm. Furthermore, this instability may also increase muscle load during keyboard use.

The preceding solution for keyboard and mouse time measurements, real-time measurements, may, however, underestimate the actual situation; the measurement is based on clicks, double clicks and mouse movements, but does not take into consideration the muscle loading during moments of waiting.

In our questionnaire, we inquired about the placement of the mouse (beside the keyboard, on a side table or elsewhere) and used the responses to rate the placement as good or poor.

In their questionnaire, Kryger *et al.* [5] used a ruler to measure the position of the mouse at 20 cm intervals. They rated the mouse distance as abnormal if the mouse was >40 cm from the edge of the desk or >40 cm to the right of the shoulder, and did not find the abnormal result to be a risk for forearm pain. Why was the distance not used as a continuous variable? Was the point at which distance was rated as 'abnormal' too far away? One can ask the question of whether the position of the upper extremity was stressful when the mouse was much nearer (a raised risk could not be measured).

At the end of Andersen *et al.*'s comments, it is emphasized that participants with pain also report more dissatisfaction with placement. We agree that in a cross-sectional study such bias may affect the results, and we discussed the possibility in our discussion.

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Re: Tom Garland

Dear Sir

I was fascinated to read the tribute to Tom Garland by Bill Glass in the Christmas Issue of *Occupational Medicine*, which describes an area of work which was unfamiliar to me.

I was appointed by him as a locum Tuberculosis Officer for Tottenham and Edgeware in 1946 for 9 months. What impressed me was his skill as an administrator, and that he took the trouble to visit the clinic at Edgeware Hospital in which I worked to ask me how I was faring. One of his questions addressed a crisis about which I had not thought: namely, how would I deal with the sudden death of a patient in my consulting room without upsetting those who were waiting outside by wheeling the body past them. Between us, we worked out a reasonable compromise using another door into a back corridor.

His personality was so positive that I can remember him well.

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Dear Sir

I read with interest the article by Omokhodion *et al.* [1]. It is based on a descriptive study where prevalence of back pain has been assessed with a description of the associated factors. However, I would like to suggest that it is only by following an analytical study design that the putative causative factors could be confirmed as risk factors [2]. Therefore, the factors described in this article can only be referred to as 'associated factors', not as 'risk factors'.

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