advance, provide a rationale for the statistical tests that are used, and consider alternative models, especially when the research is somewhat exploratory in nature. Such processes are typically not required for publication and are often not part of the standard practice when submitting manuscripts for review; however, they reduce the likelihood of errors and provide both editors and readers with a more thorough understanding of the findings.

The power of SEM is that it allows one to develop detailed and flexible models that permit judgment of overall model fit in both absolute and relative (i.e. compared to alternative models) terms. In our view, this makes SEM a valuable tool for researchers in many pain-related fields, and we believe that it renders possible a deeper understanding of pain-related psychosocial processes when identified models include all relevant information. For a more detailed treatment of this subject we refer readers to other well-written sources (McDonald and Ho, 2002; Tomarken and Waller, 2003).

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Response to Dr Kudel's Letter to the Editor

Dear editor, we thank Dr Kudel and his colleagues for their interest in our study, published in *Pain* (Goubert et al., 2004a) and for the opportunity to reply to their comments. We are living in an era with powerful computers and user-friendly statistical programs. These allow useful, but

complex and multiple analyses of data that were hitherto impossible. The time that one had to perform factor analyses by hand with a calculator is far away. In that context the comments and suggested guidelines for communicating the results of confirmatory factor analysis (CFA) and structural equation modeling (SEM) are timely and much needed.

We agree with many suggestions of Dr Kudel and his colleagues regarding the communication of statistics. Before performing parametric analyses, the data should be screened and the distributional assumptions tested. In our study these assumptions were met. We agree that these preliminary analyses should be reported, or at least mentioned. We also agree that for SEM, but also other analytic techniques such as multiple regression analyses, a table containing the correlations or the covariance matrix is necessary. We are, however, reluctant about the idea that the complete matrix should be reported. This is neither practical nor attractive, as many papers would contain pages of matrices, which are only helpful for a few. There are other ways to obtain data for further analyses or re-analysis. We also agree that SEM should be used for confirmatory and not exploratory reasons. The comment of Dr Kudel and his colleagues that we used SEM for exploratory purposes and did not test alternative models is therefore unusual. Our analysis was based upon two influential theoretical models of attention and pain-related fear (Eccleston and Crombez, 1999; Vlaeyen and Linton, 2000). We also investigated alternative models, amongst which the model of Watson and Pennebaker (1989), which states that neuroticism explains vigilance to pain. In a previous version of the paper we had reported the results of mediation analyses as described by Holmbeck (2002). As these analyses produced the same results, we are confident that our results are robust.

As with many domains in science, there is often agreement that there is disagreement. This also applies to the use and interpretation of statistics with CFA and SEM. For example, the chi-square statistic has a long tradition of opponents and proponents. At present, there is a lively debate between the exact-fit supporters and the close-fit adherents. Based upon the fact that our chi-square statistic is significant, Dr Kudel and colleagues argue that our model is an indication of poor fit. However, others have warned of the overinterpretation of the chi-square test statistic, and for its unrealistic criterion. With large samples even the slightest deviation of a model from the actual structure, could lead to the rejection of the model. Furthermore, most scientists have abandoned the idea that models are true representations of the real world, and have accepted the idea that models are useful and approximate constructions of that world (Byrne, 2001). Therefore, alternative measures of overall fit have been proposed. For example, the chi-square test statistic divided by its degrees of freedom, provides a more stable index of overall fit. The relative chi-square of our model (CMIN/DF = 1.66) indicated a good overall fit of our model. Using goodness-of-fit indices (GFI's) in addition to the chi-square statistic is another alternative. These were

also acceptable for our model. A downside of these measures is the often wide variety in the criteria for good fit. The comment of Dr Kudel and his colleagues about the RMSEA illustrates this point. An RMSEA of 0.07 (as we found) is considered acceptable by many. Their suggestion to rely on the confidence interval is also contested because it depends on sample size and the complexity of the tested model (MacCallum et al., 1996). This again illustrates that all fit measures have advantages and disadvantages. It is not the place here to start a debate. We refer to the edited work of Bollen and Long (1993) for a thorough and accessible discussion. We, however, agree with Dr Kudel and his colleagues that it is sensible to report several fit indices and to provide a rationale. In fact, we have followed this strategy, and explicitly referred to the seminal work of Bollen and Long (1993). We have reported several fit indices, including the significant chi-square and the confidence interval. It is always our policy to provide the reader with sufficiently detailed statistics in order to check our interpretation, or even to look at the data from a different perspective. Some of the comments of Dr Kudel and his colleagues regarding the poor fit of our model only attest to the fact that we have been thorough.

We would like to take this opportunity to extend the discussion. We have learned that the results of only one study are insufficient to build or change theories. There is a huge need for replication in science, although not necessarily exact replication. The same hypotheses should be tested in different samples and in different settings. A multi-method approach including self-reports, psychophysiological measures and behavioural paradigms is warranted. We follow this multi-sample and multi-method approach in our research about attention, fear and pain. Using self-report measures, we have been able to replicate the finding that the threat value of pain plays a critical role in vigilance to pain in fibromyalgia and low back pain (Crombez et al., 2004). Using a reaction time paradigm, we have been able to show that low back pain patients who had catastrophic thoughts about a lifting task, engaged less in a distraction task because they were vigilant for pain (Goubert et al., 2004b). In a series of experimental studies with healthy volunteers, Van Damme et al. (2004a; 2004b) repeatedly found that students who catastrophize about pain, have unique and pronounced difficulties to disengage attention from pain (signals) once pain was detected.

We conclude that a discussion about how to efficiently and consistently report CFA and SEM is to be encouraged. We do not believe that a consensus is within reach, or even necessary. A debate should, however, result in more consistent reporting of CFA and SEM results.

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Food insensitive editorial on outcome measures

I could not get myself to read the editorial by McQuay (2005) on 'Consensus on outcome measures for chronic pain trials' in the January 2005 issue of PAIN, even though the title was attractive. The reason is the bold subheadings 'the meat' and 'the beef'. Being a pure vegetarian, I could not fathom what these subheadings could imply. Moreover,