Reviewer's report

Title: Caution required when relying on a colleagues advice; a comparison between professional advice and evidence from the literature

Version: 1 Date: 7 March 2005

Reviewer: Peter Westerholm

Reviewer's report:

This is a very interesting paper and I shall start in responding to questions in BMC Health Services Research template for reviewers

1. Q: Is the question posed by authors new and well defined?
A: Yes, it concerns a topical problematics in Occupational Health and indeed also in primary health care. The research question is well defined

2. Q: Are ther methods appropriate and well described etc?
A: Yes, it is a methodologically robust approach with underlying creative thinking.

3. Q: Are the data sound and well controlled ?
A: Yes

4. Q: Does the manuscript adhere to relevant standards for reporting and data deposition.
A: Yes

5. Q: Are discussion and conclusions well balanced supported by the data
A: Well, yes, as conclusions are framed they are quite all right. Some clarification and support in data presentation and discussion would improve clarity

6. Q: Do title and abstract convey what has been found?
A: Yes

7. Q: Is the writing acceptable ?
A: Yes.

I have some comments to add to these overriding appreciative answers to BMC HSR questions

1. It is here of fundamental importance to recognize the characteristics of the study populations. The sampling of the OP:s which are the index population in bull's eye of study is, for obvious reasons, a yield of a convenience sampling. It was essential, clearly to have colleagues volunteer to participate in the survey. This factor of free volition is important to keeping mind since it may imply selection of a study population which is not guaranteed to represent study base.

This is in itself no criticism of the study, only a reminder.

As to the selection of the professional advisors the situation becomes a bit more tricky. First, the reader is given to understand that the professional advisors were chosen or assigned by the OP:s of the index group. I assume that this is correct.

Having this for point of departure it is important to keep in mind that the concept and generic term professional advisor in this study has a rather loose content. Which were actually the criteria used in selection of professional advisors or peers for consultations on patient cases?

Clearly, the advice or expert opinion provided by advisors or peers will depend on how they are assigned and on which criteria. In this study this was also carried out as a convenience sampling even if this is not actually stated. That is anyway given to the readers understanding. OP:s have, as
have primary care physicians, their habits implying that advisors may be consulted based, beside trust in competence, on social relations, agreed fee levels, tit-for-tat deals such as consultations in return for a week's stay in consulting doctors chalet in Schwarzwald and other types of agreements outside the professional sphere.

The composition and recruitment of peers and professional advisors may have a bearing on the external validity and inference to be drawn from findings. The advice given by, say, university hospital departments specialized in diagnostics and management of stress-related disorders may take different views to the idea of being “Evidence Based” in giving advice as compared to advisors consulted by colleagues.

Here again, this is not to criticize what has been done in this study. Only to remind that with study populations assigned on basis of convenience sampling the inferences to be drawn are restricted to the study population rather more than to the broader category of “professional experts”. Still, I feel that the observations are important and deserve to be published.

2. As to the statistics applied in comparisons of OPs and their advisors we are obviously in a game of relatively small numbers. I find the statistical analyses to be flawless and convincing but I would recommend to have a statistician to look it all through to see that assumptions for use of statistical techniques in calculating differences and confidence intervals are satisfied.

3. As to the cases I have observed some details

Case 1. Ankle ligament sprain and time when work can safely be resumed. The severity of sprain may be subject to differing judgement based on information provided. The accuracy of the clinical assessment made at the outset, i.e. time of injury is of critical importance. That severe injuries tend to stay immobilised for longer periods of time is a common experience.

The bottom line is here that the answer here rests on consensus documents, not evidence base in a more strict sense. A published consensus document edited by a qualified panel of experts clearly carries high authority. But is it to be regarded as “evidence”?  

CAT no III

This is a case of work-related stress and its management. My question is if the study by Kivimäki et al in BMJ 2002 addresses the type of stress invoked as causal factor in this case. Personally I am convinced that workplace stressors do contribute to increased levels of risk for cardiovascular health outcomes. There are in the literature dozens of models to study this. Karasek-Theorell (1990) has the demand.control model, Johannes Siegrist (1996)the effort-reward model. Michael Marmot (1997) has studied models with interaction between occupational strains and socio-economic gradients. Lazarus (1984) has focussed coping with stressors.

I have not checked Kivimäki reference source. My comment is that when placing reliance on one study it is of critical importance to see to it that the source selected is pertinent. It is in experience risky to have a line of reasoning depending on one study only. This is a research group from the Netherlands. It appears to be easy to have a more broad-based assessment of available evidence using f.i. experiences and published results of research groups in Holland

CAT X

Effectiveness of St John’s Wort.

Well, yes. An impressive line-up of RCT:s in favour of hypothesis. My only two questions are:
- Is there a biomedically credible explanation for this effect?
- Is there reasonable assurance that the effect under study is attributable to St John's Wort? Other explanations, such as other components in treatment regimens appropriately controlled?

CAT XII

RtOW after inguinal hernia operation. Yes, fine. Excellent review of evidence base. It seems, however, important also in this case to distinguish between what is to be regarded as peer opinion carrying authoritative weight on one hand and evidence base on determinants of risk of recurrent herniation on the other hand. The evidence explored in this case gives readers an impression of being primarily of the category first mentioned. Here again, there is nothing inherently wrong in this. This is only to remind of the necessary distinction between normative statements given by experienced specialists in clinical medicine and evaluations following a rigorous research protocol.

To sum this up then, this is an excellent paper in its originality of design and in its addressing an important area of clinical case management replete with issues of what constitutes relevant evidence for guidance of practice.

Since I am not a regular reviewer of the BMC, nor a member of its Editorial Board I refrain from giving a recommendation. I hold this to be a matter to be decided on by the Editor(s)

I limit myself to observing that in a few other research journals in Occupational health field where I am on the EditBoard I would recommend the authors to revisit their text for a few clarifications. This is envisaged to result in a manuscript deserving to be published.

What next?: Accept after minor essential revisions