Reviewer's report

Title: Do hospitalist physicians improve the quality of inpatient care delivery? A systematic review of process, efficiency and outcome measures

Version: 1 Date: 23 February 2011

Reviewer: Michael Peterson

Reviewer's report:

The paper is very well written and understandable – it is a pleasure to read. The framework for consideration of current and future research related to performance of hospitalists was insightful and I think better treatment of the subject than I have read before. My comments below are meant as friendly criticisms:

Major Compulsory Revisions

1. A great deal of effort has gone into preparation of consensus guidelines for completing and reporting of systematic reviews – the most recent incarnation being the PRISMA Statement (Liberati A, et al. The PRISMA statement for reporting systematic reviews and meta-analyses of studies that evaluate health care interventions: explanation and elaboration. PLOS Medicine 2009;6:e1000010). The PRISMA statement assures that reviews are completed in a standardized and reliable manner. I believe the authors need to state the framework they used for completing and reporting their review (if a formal guideline was used) and at least mention the PRISMA Statement. If the PRISMA Statement hasn’t been used as the guideline, there should be some justification of this as far as is possible and I believe mention of it in the limitations section of the paper.

2. Could the authors state whether a systematic review protocol was used as recommended by the PRISMA Statement? If this was done, it should be mentioned and if not, it should be stated and justified under limitations.

3. Pages 10, 11 state that “at least one author” selected articles and a single author extracted data. While this was an accepted practice in the past it may no longer be adequate -- most current reviews use duplicate reviewers for data extraction as a means to reduce errors. If the data extraction is by a single person, the authors should justify this practice or at least mention it as a limitation.

Minor Essential Revisions

1. In the 1st new paragraph on page 13, the description of the “two randomized controlled designs” given in the paper doesn’t meet a definition of randomization wherein the assignment of the next subject can’t be predicted. I believe this description might be fairly called “quasi-random.” In support of this, see The Cochrane Handbook of Systematic Reviews of Interventions, 2008. P. 213 under...
the heading of “Inadequate methods of sequence generation.” I believe the paper by Huddleston is randomized but to my knowledge the Kearns paper is not a truly randomized study.

2. The text in parentheses “See 6, 109 for overview” was not immediately clear to me on page 24.

Best wishes,
Michael Peterson, MD, MSc

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**

I have no competing interests except for having published on this topic and having had a paper referenced in this review.