Reviewer's report

**Title:** Clinicians’ caseload management behaviours as explanatory factors in patients’ length of time on caseloads: a predictive multilevel study in paediatric community occupational therapy

**Version:** 1  **Date:** 5 May 2010

**Reviewer:** Anton Miller

**Reviewer’s report:**

The authors are to be commended for undertaking research into understanding the variation in the length of time that children spend on community occupational therapists’ caseloads. Length of time on caseload (LOT) may be associated with lack of access to services, as the authors point out, and may also be a marker of inappropriate utilization of services. Besides the fact that it tackles new ground, the study also has a number of other strengths: it is community based, and care was taken to ensure a representative sample of therapists. However, I have concerns about a number of aspects of the study and the paper. The serious concerns are grouped under the header, Major Compulsory Revisions.

**Major Compulsory Revisions**

1. There is very little justification for the decision to examine patient characteristics and therapist clinical behaviors to predict LOT. Perhaps such justification does not exist in the literature, but the authors do reference some material which may be relevant. This needs to be brought out more fully, and perhaps further searching done for material that is even indirectly relevant (such as factors that affect the way that various kinds of clinicians do things, make decisions etc). In turn, there should be less detail given in the Background to access to services, because, while indirectly relevant to the study area, it is not the focus of the present article.

The reason why I am emphasizing the lack of review of what is known, is that the study has produced some findings which are not only questionable in terms of validity (discussed below), but which become almost impossible to interpret in the absence of any prior evidence or theory.

2. Linked to this is a further concern about why these particular predictors were selected for study. In the absence of existing suggestive evidence or theoretical justification, it appears that these predictors may have been used simply because they were available to the researchers, so for reasons of convenience. If this is the case, then the whole study can only be taken as a very early exploratory study, and the results not used to explain patient care phenomena.

3. Another major concern is the fact that it appears that there were large portions of missing data, of a kind that were very relevant to addressing the main research question. Table 2 shows that there was substantial missing data for at
least 5 of the 14 ‘caseload management behaviors’ that are the main predictive factor under study. (Ironically, some of the behaviors for which substantial data are missing, appear at least to be the very ones that might be most capable of explaining variations in LOT, such as establishing treatment goals and monitoring progress against treatment goals). The impact of this is twofold: first, it is not clear that the data analyzed and presented in Table 3, are equally representative of caseload behaviors across therapists. It is not clear that all of these LOTs can be validly compared in one model, when LOT related to certain behaviors was ascertained from just 5% (or 12%, or 26%) of the total sample. A statistician could advise on this definitively, but I don’t see how LOTs can be compared when one is from 12% of the sample and another is from 93%.

4. A further problem related to the above does not need statistician input. The authors have analyzed the significance of various behaviors by their p-values, and have gone on to conclude that only these behaviors are linked to LOT. However, the p-values in univariate regressions will be importantly influenced by the number of observations or sample size, so it is quite possible that some behaviors emerged as non-significant only because of differences in the number of observations available for analyzing the role of each behavior. This problem materially affects the authors’ main conclusions.

5. Another serious limitation of the paper, over and above these considerations, is to conclude that these (or really, any) therapist behaviors explain variations in LOT. The authors mention that causality cannot be inferred from a cross-sectional study such as this, but then go on to say that therapists’s caseload management behaviors “contribute significantly to the variation”, and that interventions to change therapists behaviors are warranted, and that these might help to reduce variability in LOT, and hence in access to services. Any of this is possible, but such statements go well beyond what this study is capable of showing. This is particularly acute as there is little or no theoretical justification of why the particular behaviors found to be statistically significant in this study, are in fact, meaningful. Indeed, it seems likely that at least some of the behaviors examined in this study might more accurately be considered proxies or markers of some other aspects of therapists approach to their work and caseload, that were not measured, but that would be more useful at explaining variations in LOT. I realize that this other kind of data were not readily available, but that is a different problem.

6. I also have concerns about how the data were analyzed and the results presented. Although I am familiar with linear regressions models, I have not seen results presented with a number of months under what is presumably the regression coefficient column as in Table 3. Also, I was not able to follow the coefficients in the Table. If a coefficient is -28 months, what is this relative to? Similarly in the Results section, “older children remained on caseloads for shorter time [-1months]” – but relative to what or to whom? Also in Table 3, I could not understand why only 10 behaviors are listed, and not all 14. Finally, the authors talk about an effect size sample of 15 in the Sample Size paragraph. I am not familiar with the phrase ‘an effect size sample of 15’ and wonder if the authors
could explain what they mean here. Again, statistician advice or consultation might be helpful.

Minor Essential Revisions
I appreciate the way the data are presented as box plots in Figure 2, but could the authors provide some notation of what the asterisks and open circles are in the Figure?

I also found it difficult to make sense of Table 4. Is there some other, clearer way to present this material?

There were instances where terminology was unclear and should be explained or expanded:

**Background:** “throughput”

**Background:** “length of patient episode”

A minor point, maybe due to different usages in different settings and disciplines, but even the term ‘caseload management behaviors’ is somewhat ambiguous. In some ways it suggests the ways that therapists specifically manage their ‘caseload’ in the aggregate sense, but what I think is being referred to is the management activities they bring to individual cases or clients.

**Level of interest:** An article whose findings are important to those with closely related research interests

**Quality of written English:** Acceptable

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests