Reviewer's report

Title: Applying psychological theory to evidence-based clinical practice: Identifying factors predictive of managing upper respiratory tract infections without antibiotics.

Version: 1 Date: 12 September 2006
Reviewer: Michael Steinman

Reviewer's report:

General

I approach this review as a physician with expertise in health services research but little formal training in research on psychological theories. My perspective is thus as a general reader of this journal, and my comments should be taken in this context.

This is an interesting and valuable article that evaluates the predictive validity of several psychological frameworks for antibiotic prescribing behavior among physicians managing upper respiratory tract infections. Strengths of the article include the novelty and importance of the research question, its broad scope, and its use of several measures of antibiotic prescribing against which each predictor framework is tested. However, there are some areas that could be modified to improve the paper, as outlined below.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

There are no "compulsory" revisions as defined in the reviewer's instructions. The "major comments" detailed below reflect issues of presentation and background, and not the quality of the research itself.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

Major Comments:

My first major point is that I found the paper a bit difficult to follow at a number of points. In particular, I found the methods difficult to follow insofar as I didn’t understand the general flow of the analysis after first reading this section (although it later became clear after reading the results). In addition, I found the discussion slightly overwhelming in that it tried to make sense of many different analyses. In general, it is a daunting task to present a paper with so many different predictor and outcome variables. Although the authors have already organized this logically, providing a clear roadmap at each step along the way (particularly in the methods and results) could help guide the reader through the multiplicity of analyses and approaches. Also, in the discussion it might be helpful to place a greater emphasis on one or two main points as the cornerstone of the findings, in the context of which other findings could be discussed.

The other major point is that some additional background in the introduction and discussion about influences on behavior would be helpful in rounding out the reader’s understanding of this issue. For example, at the end of the first paragraph of the introduction the authors state that “understanding of how best to achieve [reductions in antibiotic use] is limited.” Brief elucidation of what studies have taught us about changing antibiotic prescribing (for example, in reference 12: Ranji et al) would help the reader better understand how the present work fits into a larger context. Similarly, in the intro and/or discussion it would be useful to briefly discuss what other research has taught us more generally about physician behavior and behavior change, for example as summarized by Bero (BMJ 1998; 317(7156): 465), Oxman (CMAJ 1995; 153(10): 1423), or others.

The work is already lengthy, and I certainly do not ask for extensive discussions of the above. However, a brief summary of each would be helpful in orienting the reader and placing the present work in a larger context.
Abstract: I was a bit confused by the results section insofar as it seemed that only certain theories were being tested for each outcome (i.e. OLT and actual behavior). Further clarifying this (i.e. all theories were tested, and only significant results are presented) would be helpful. Also, at the end of the results the phrase “higher intention score” is confusing (i.e. what does this mean?), and could be reworded more clearly.

Pages 4-5: I found the discussion of the relationship between this research and implementation science to be confusing. It would help to more clearly delineate the intersection of research interventions and implementation science, and explain where the theories examined in this paper fit into that framework.

Page 6: It would be helpful to supplement the existing explanations with a brief background / explanation of each theory that is accessible to a general reader.

Page 9, penultimate para: Unclear what the phrase “For the five ‘cause’ items in the Common Sense Self-Regulation Model…” means.

Page 9, final paragraph: Did you check for interaction effects between models, and is there any reason why you might hypothesize these to be important (or unimportant)?

Page 11, second para: Please clarify what the numbers presented (i.e. 0.17, 0.19) mean – are these Pearson r statistics?

Page 11, penultimate para: Do you mean that only the only theory that *entered* the regression model was OLT, or that OLT was the only theory that was "retained" in the regression model? According to Table 2, SCT (and maybe TPB) also appear to meet criteria for entering the model, since on bivariate analysis they are significant at P<.025.

Page 15, 2nd para: This paragraph is nicely stated. Do the authors have any data (or speculation) as to the extent that the questions asked on the survey accurately reflect the constructs that they attempted to measure? Stated otherwise, are the measures valid?

Page 17, 2nd and 3rd paras: The authors thoughtfully discuss the issue of response rate and make the interesting point that achieving representativeness was not the main goal of the study. This makes sense, but it needs to be acknowledged that their results may nonetheless be biased insofar as the relationships between psychological theories and outcomes among respondents may be different than the relationships that exist among non-respondents, and among the general population of Scottish GPs.

Table 3: Please clarify what F1 and F2 mean, as suffixes to a number of “predictive constructs” as listed in the table. Also, as probably reflects my ignorance of psychological research, I do not understand why the analyses of Intention and PBC Direct were duplicated within the TBP analyses (i.e. separate sets of analyses with and without PBC Power).

Discretionary Revisions (which the author can choose to ignore)

Finally, in the spirit of this critique being posted on the internet, I offer up two final questions for the authors. These are not necessarily for inclusion in the manuscript but are areas that may merit attention.

First, is there a concern for multiple hypothesis testing, insofar that the authors evaluate a number of putative predictors, each tested against 3 distinct outcomes?

Second, is it possible that the behavioral scenarios are biased towards producing high variance for certain theories because they were constructed with an idea of testing those theories?

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.