Author's response to reviews

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

Authors:

Martin P Eccles (martin.eccles@ncl.ac.uk)
Jeremy M Grimshaw (jgrimshaw@ohri.ca)
Marie Johnston (m.johnston@abdn.ac.uk)
Nick Steen (nick.steen@ncl.ac.uk)
Nigel B Pitts (n.b.pitts@chs.dundee.ac.uk)
Ruth Thomas (r.e.thomas@abdn.ac.uk)
Elizabeth Glidewell (e.glidewell@abdn.ac.uk)
Graeme MacLennan (g.maclennan@abdn.ac.uk)
Debbie Bonetti (d.bonetti@chs.dundee.ac.uk)
Anne Walker (Anne_walker@yahoo.co.uk)

Version: 2 Date: 20 April 2007

Author's response to reviews:

Dear Dr Mittman,

MS: 6147742111268223 Applying psychological theories to evidence-based clinical practice: Identifying factors predictive of managing upper respiratory tract infections without antibiotics.

Martin P Eccles, Jeremy M Grimshaw, Marie Johnston, Nick Steen, Nigel B Pitts, Ruth Thomas, Elizabeth Glidewell, Graeme MacLennan, Debbie Bonetti and Anne Walker.

Thank you for asking us to respond to yours and the reviewers comments on our paper. The reviewers detailed comments were particularly useful and have, we hope, helped us to write a clearer paper. In addressing their specific comments we have hopefully addressed your two suggestions to (a) better tie the work to past research and (b) strengthen documentation of details of our operationalization of the theories.

Reviewer 1 (Dr Steinman)

Reviewer comment 1. 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.

Author response. We have re-framed the start of the methods as an overview paragraph and have also written an overview paragraph for the results. The start of the Methods now reads: "This was a predictive study of the theory based cognitions and clinical behaviours of general practitioners (GPs) from Scotland. Theory based cognitions were collected by postal questionnaire survey. Behavioural data was collected from routinely available prescribing data and planned analyses explored the predictive value of theory based cognitions in explaining variance in the behavioural data." We have re-worded the Design and Participants section to accommodate this new text.

At the start of the analysis section we have added the following text: "The overall analytic approach was firstly to check the internal consistency of the measures. Next, for each of the three outcome variables, we examined the relationship between predictor and outcome variables within the structure of each of the theories individually. Finally, for predictors that were statistically significant irrespective of whether or not they came from the same theory, we similarly examined the relationship between predictive and outcome variables."

In different ways both reviewers made comments on the discussion. The discussion is relatively long and complex as it discusses the issues that emerge from the study. We have added two small sections of text to try to highlight the implications of the study but beyond this we have not made substantial changes.
Reviewer comment 2. 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.

Author response. In order to provide more information about strategies to reduce antibiotic prescribing we have added the following text: "Ranji et al's review included 34 studies (reporting 41 trials) addressing treatment decisions (as opposed to drug choice decisions) most of which were looking at prescribing for acute respiratory infections. All the interventions examined (clinician education, patient education, provision of delayed prescriptions, audit and feedback, clinician reminders and decision support systems and financing and regulatory incentives) were effective at reducing prescribing (median absolute effect -8.9% [interquartile range -12.4% to -6.7%] but no individual strategy (or combination of strategies) was more effective at reducing prescribing."

In order to provide more information about behaviour change strategies in general we have extended the short section of the Background to read: "This research demonstrates that a wide range of empirically defined interventions can be effective. These span the range of strategies aimed at individuals (e.g. audit and feedback, reminders, outreach visiting), those aimed at organisation of care (case management, revision of roles, Continuous Quality Improvement) through to financial and regulatory interventions. For example, Grimshaw et al reviewed studies of interventions to promote the uptake of clinical guidelines and showed that all interventions were effective some of the time, with a median absolute effect size of around 9%. However, all interventions had a range of effect sizes across the studies examining them and the basis for choosing a particular intervention was usually not described. A consequence of this is that when such studies are reviewed the lack of any common underlying framework means that they provides little detailed information to guide the choice, or optimise the components, of such complex interventions when they are introduced into routine care settings."

Reviewer comment 3. 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.
Author response. We have added the text "All theories were tested and only significant results are presented." to the start of the results section of the Abstract. We have amended the sentence containing higher intention score to read "GPs who reported that they had already decided to change their management to try to avoid the use of antibiotics had a significantly higher intention to manage URTIs without prescribing antibiotics."

Reviewer comment 4. 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.
Author response. We have re-ordered the background so that all the text relating to prescribing antibiotics is now together and the remaining text on the rationale for the use of theory is now hopefully more coherent.

Reviewer comment 5. 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.
Author response. We have not changed anything in response to this point. We appreciate the point the reviewer is making; such text forms part of our published study protocol which is identified and referenced in the text. If the paper is accepted (given this is an electronic journal) then reference 23 will be hot linked to the study protocol (published in Biomed HSR) to which a reader will have immediate access. This will be two mouse clicks more than the alternative of cutting out the relevant (3 pages of) text from the protocol and including them as an Additional File which would then be hot linked at this point.

Reviewer comment 6. Page 9, penultimate para: Unclear what the phrase "For the five 'cause' items in the Common Sense Self-Regulation Model..." means.
Author response. We have re-worded this to read "For the five "perceived cause of illness" questions in the
Reviewer comment 7. 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)?

Author response. If by interaction effects the reviewer means that, say, TPB and SRT together explain more than the additive effects of combining them then we didn’t check for interaction effects between models as this was not the primary aim of the study. However, we were interested in the potential overlap between the constructs within the models and addressed this by the “cross theory” analysis.

Reviewer comment 8. Page 11, second para: Please clarify what the numbers presented (i.e. 0.17, 0.19) mean - are these Pearson r statistics?

Author response. Yes they are and we have amended the text to clarify this.

Reviewer comment 9. 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.

Author response. We mean retained and have amended the text for each of the analyses accordingly.

Reviewer comment 10. 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?

Author response. Where available we followed standard methodologies to operationalise the theories; when we did this it became clear that there was overlap in the constructs. This suggests either that the constructs are not independent or the methods of operationalising are not independent - this is an advantage of a multi-theory approach. There is no methodology to differentiate constructs though we are currently engaged in efforts to develop one. We have left the text unaltered.

Reviewer comment 11. 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.

Author response. We agree with the reviewer. We feel we have already acknowledged this when we say "Thus, while we should be cautious about generalising from our respondents to the population of Scottish GPs, this is less of an issue at this exploratory stage of using these methods."

Reviewer comment 12. 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).

Author response. We have changed the labels F1 and F2 for the terms used in the previous table of "self" and "behaviour". The presentation in Table 3 was an exploration of whether or not our "indirect" (belief-based) measures added to the explanatory power of the model. According to the structure of the theory they shouldn't but, as you can see, they do.

Reviewer comment 13. 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?

Author response. As this is an exploratory analysis to see if these theories work on health professionals we don't need to worry about multiple testing in the sense that we are going to base decisions on spuriously low p-values. We are interested in anything that may have potential for future research. Results that come out of this will need to be repeated and validated and built on elsewhere.

Reviewer comment 14. 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?

Author response. We think this is unlikely in that the scenarios were constructed to vary elements of the underlying evidence rather than theoretical constructs.

Reviewer 2 (Dr Nick Sevdalis)
Reviewer comment 1. Essentially, the study is attempting to extend previous findings regarding GPs prescription behaviour (as reviewed on p. 5). This is not evident at all from the way the study is "positioned". I understand that the authors may want to focus on methodological issues, but the manuscript is not written for a social psych journal. With this in mind, I feel that an attempt should be made to show some continuity...
in the available evidence on prescribing behaviour.

Author response. We hope that our response to Reviewer 1 comments 2 and 4 we have done this.

Reviewer comment 2. Moreover, in the Discussion the authors could explore more *systematically* their findings in terms of what parts of each theory appeared to work well in their models. (As it stands, the Discussion looks like a list of possibilities, not very well integrated with each other.) This would tie in nicely with the specific behaviour that they are trying to model. In other words, I'm not asking for a full-blown discussion of the components of the theories. I am asking for a discussion of how well specific components of the theories worked in the modelling of prescription behaviour. (This discussion may also inform the authors' reasoning regarding the low explained variance in actual prescribing behaviour.)

In different ways both reviewers made comments on the discussion. The discussion is relatively long and complex as it discusses the issues that emerge from the study. We have added two small sections of text to try to highlight the implications of the study and to say something more about how the models worked.

Reviewer comment 3. Furthermore, I do have concerns regarding the presentation of the Methods and of the Results sections of the manuscript - as should be evident by my suggested revisions in the following section. The authors' description of what they did does not always match what they report in the Results. Moreover, there are bits of the text that are unclear. These require revision, if the manuscript is to be read and understood easily (as the manuscript stands, some parts are not at the moment).

Author response. Hopefully we have dealt with this in addressing the reviewers points as they are detailed below.

Reviewer comment 4. One last point: I wonder whether the fact that some of the GPs reported having changed their management of URTIs affected the modelling. The authors report models based on the whole sample, but are the models for the 2 sub-groups of doctors (those who did and those who didn't change their management) the same? I understand there's a statistical issue here (not enough subjects), but, nevertheless, I wonder whether this analysis would reveal something interesting.

Author response. We agree with the reviewer that this would be an interesting question and have plans within the research team to pursue this. However, the size and nature of such an exercise would be too large to incorporate in this paper.

Reviewer comment 5. P. 8: secondly, individual prescribing...to conduct a sensitivity analysis": very unclear section, needs rewriting.

Author response. We have rewritten this section and (hopefully) clarified the situation. The text now reads "Secondly, individual prescribing data was standardised by the number of patients the GP saw (our proxy measure of this was the number of half day sessions worked by each respondent).

Each prescription carries an identification code that is unique to the prescribing GP. However, it is possible that another clinician (e.g. a training doctor) might use a respondent's prescriptions thus producing an over estimate of the total number of prescriptions issued by that respondent. In order to allow us to make some estimate of this all respondents were asked to estimate "Over the last six months how often have acute antibiotic prescriptions been written/printed by someone else (e.g. locum/trainee) using your cipher number?" with response options of Never, Sometimes, Frequently, Don't Know. The response to this question was used to conduct a sensitivity analysis."

Reviewer comment 6. P. 8, last sentence: "low score...high intention...not to prescribe". I suggest: "responses were summed and scaled so that higher scores indicate higher intention to prescribe" - be simple when defining scale anchors! The full range of the measure should also be given here (I assume it is 3-21) and it should then be repeated on p. 12. Alternatively, do not add the measures, average them (as it's easier for the reader to keep in mind the 1-7 scale throughout the manuscript).

Author response. We take the reviewers point. We have re-written the text as "responses were summed (range 3 - 21) and scaled so that lower scores indicate lower intention to prescribe." We have also added the scale range to the section on predicting behavioural intention in the results.

Reviewer comment 7. Pp. 9-10: "Having done this...regression analysis": unclear what the authors mean.

Author response. Drawing on the introductory text we have now written for the analysis section in response to Reviewer 1 comment 1 we have re-worded this section to read "Finally, for predictors that were statistically significant, irrespective of whether or not they came from the same theory, we similarly examined the relationship between predictive and outcome variables. All constructs which predicted the outcome (p <0.25 for a univariate relationship) were entered into a stepwise regression analysis to investigate the combined predictive value of significant constructs across all theories."

Reviewer comment 8. P. 11 and 17: the N of participants who said that others write prescriptions in their
Reviewer comment 9. Pp. 11-12: independent group comparisons are reported but the test statistic is not mentioned (is it a t-test, for instance?)
Author response. We have added a line to the introduction to the analysis saying "When comparing groups independent t-tests were used as appropriate."

Reviewer comment 10. Pp. 11-13: the authors report the number of participants who endorsed the statement "I have already decided to change my management of URTIs...etc". The number is 167 on p. 11, 182 on p. 12 and then becomes 188 on p. 13 - unclear, requires clarification.
Author response. The numbers change because we had more respondents provided usable data on intention (188) than provided usable data on behavioural simulation (182). Both these figures were larger than the number of respondents who agreed to allow us to receive their behaviour data (167). We have added a sentence explaining this to the start of the results.

Reviewer comment 11. P. 13: similar point: adding up the 188 participants who did and the 66 participants who did not endorse the statement gives us a sum of 254, but the total N = 228 (p. 11). As above, requires clarification.
Author response. The explanation is the same as for comment

Reviewer comment 12. P. 14: the knowledge item(s) are mentioned here for the first time - they do, however, feature in Table 4. The authors need to explain what the items were, give examples in Table 1, and include the items in their presentation of the predictive factors in the Methods section (p. 7).
Author response. We have added the text "Five knowledge questions were developed by the study team based on issues for which there was good evidence." to the Methods section. An example was already included at the bottom of Table 1 under "Other Measures".

Reviewer comment 13. P. 9: a reference is needed to Green's work
Author response. This has been added.

Reviewer comment 14. P. 11: the inter-correlations between the measures would be easier to read if they were presented in a correlation matrix instead of in the text
Author response. Reviewer 1 didn't comment on this, and as it is a discretionary revision then, rather than add a fifth table, we have chosen to leave this as text.

Reviewer comment 15. P. 15, 2nd para: perception of the condition
Author response. We have inserted the missing word.

Reviewer comment 16. Throughout the manuscript: word limits permitting, I suggest the authors avoid the acronyms for the theories.
Author response. We accept the sentiment behind the reviewer's comment but for now we wish to leave this element of the text as it is. We think that referring to the theories by their full names would make the paper (particularly the results section) more difficult to read. However, we appreciate that it is easy for us (who are familiar with the abbreviations) to say. If the editor feels strongly about this we are prepared to then do this.

We hope our manuscript is now acceptable for publication.
Martin Eccles