Author’s response to reviews

Title: Predictors of participation in preventive health examinations

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Author’s response to reviews:

To

Editor-in-Chief of “BMC Public Health”

Vienna, September, 30th, 2013

Dear Editor in Chief!

Thank you very much for your letter of August, 18th, 2013 regarding the manuscript entitled “Predictors of participation in preventive health examinations” by Brunner-Ziegler et al, MS: 8165860551015045.

We appreciate the helpful comments of the Reviewer and have revised the manuscript accordingly.

We would like to respond to the reviewer’s comments on a point to point basis as follows:

Reviewer 1

Comment:
The paper would benefit from a fuller discussion of the tests included in the comprehensive screen, where these are offered by whom and in what context. For example, are the screens provided to all individuals (stratified by gender) by the same type of practitioner in the same type of clinical facility? Is there a hurdle mechanism by which they are offered for example by a family doctor but performed elsewhere (which seems likely)? If there are differences in where screens are offered that correlate with socio-demographic grouping this might explain variations in uptake. Equally if screens are offered (say) only by family doctors might observed variations reflect differences related to how doctors are
reimbursed for other services and efforts they expend in securing this income stream? These issues need to be explored.

Response:
Background information on the context and the characteristics of providers and participants of screens has been expanded in the revised version of the manuscript. In particular, differences in where screens are offered to which sex and age group of individuals and differences related to how doctors are reimbursed have been explored in more detail (second paragraph of the background section).

Comment:
In respect of the methods, the categorization of any chronic condition in a single variable strikes me as destroying potentially useful information. Did the authors consider using a count of conditions or including a series of dummy variables for each condition? The stratification by gender I imagine reflects a recognition of differences by gender in the type and number of tests offered to males and females – though it would be useful to have a rationale provided. If this is the case might stratification by age group not also provide a useful line of enquiry? PSA testing, mammography and cervical screens will all vary by age.

Response:
We have analyzed the data in logistic regression analyses, in which we controlled for each of the diseases separately as dummy variables, without significant alterations of the final results. For higher comprehensibility we decided to merged the individual chronic diseases into one independent variable. The rationale for stratification results by sex (i.e. to account for differences of the type and number of tests offered to males and females) is explained in the revised version of the methodology section (last sentence of the statistics section).

Comment:
A key finding of the paper is that (line 374) the check-ups are used by a high proportion of people who were not the primary target group. Given the range of tests that appear to be covered, the possibly of tests for primary and secondary prevention, that those not tested (e.g. by virtue of an existing condition) may be under routine surveillance by other healthcare professionals, this conclusion seems overstated.

Response:
We agree with the reviewer and have revised the abstract and discussion and conclusion section accordingly. From the present results it is not valid to get into the discussion on whether check-ups are predominantly used by the “target population” or not. (“….Check-up utilisation rates are not equally distributed among subgroups…”; abstract and conclusion section).

Comment:
Line 53 – the authors I think confuse “more often” with “more likely” – they don’t have count data to work with.

Response:
We corrected the wording accordingly.

Comment:
Line 58 – the conclusion in respect of respondents with chronic conditions is not I think supported. Checks may relate to secondary prevention and as noted what if any other health checks, tests etc. respondents are in receipt of is unclear.

Response:
see also response to comment 3:
We agree with the reviewer and have revised the conclusions of the abstract and discussion section accordingly. From the present results it is not valid to get into the discussion on whether check-ups are predominantly used by the “target population” or not. (“….Check-up utilisation rates are not equally distributed among subgroups; abstract and conclusion section…”).

Comment:
Line 163 – explain why Cronbach’s alpha is reported and what it tells us.

Response:
Cronbach’s alpha is reported as a measure of internal consistency of the built parameter “psychosocial discomfort”. The Cronbach’s alpha of 0.807 shows an appropriate reliability of the parameter, which consists of five questions. We added a short explanation to the methods section of the revised version.

Comment:
Line 216 ff. My understanding is that the comparisons reported must be stratified and remain separate for the two genders. For example, at lines 223-225 the base categories in two regressions differ making the comparison of females and males by age meaningless.

Response.
The results section has been modified as a whole (as also suggested in the comments of reviewer 2). In the modified version of the manuscript results have been broken up into several paragraphes and only key results have been focused.

Comment:
Line 315-317 – the results could be interpreted as showing differences in health consciousness but I don’t think they definitively show it.

Response:
We have modified the paragraph by describing the results and just proposing a potential interpretation.

Reviewer 2
Comment.
The sub-title of the manuscript is “Is there a “Healthee Screenee effect?” In general I hear this term used in the context of explaining why people who receive prevention have better outcomes. The idea is that people who receive preventive care might be more likely to have better outcomes not due to the preventive service, but rather because of the same factors that led them to get the preventive care.

While they discuss this a bit in introduction, I never understood why we get a better understanding of the healthy screenee phenomenon by looking at socio-demographic factors that predict likelihood of receiving a PHE in Austria. The results are mostly supportive of the healthy screenee effect - higher education/higher income/who feel better about themselves are more likely to get a PHE. Now that they know this, how does that help us understand the healthy screenee effect?

Response:

We have modified the title of the manuscript. We agree with the reviewer that the main focus of the manuscript was not to provide new insight in the explanation of the healthy screenee effect, but to understand the characteristics of subjects, participating at health check-ups. We deleted the term healthy screenee effect from the title.

Comments:
I was surprised they did not cite the many many studies on predictors of other preventive services (e.g. mammograms, colon cancer screening). Their results are mostly consistent with this prior work and that should be noted.

Another issue with goal of the study is that they never go into more depth on the controversy about PHEs. In fact, whether PHEs are helpful or not helpful is an issue of great controversy. This is an issue only tangentially addressed. The recent Cochrane review published this year on PHEs seemed to indicate little or no benefit from PHEs. Other studies and thought pieces they could consider citing from the US are included below.

They should obviously note this controversy. Also, it seems they should address the question of whether it is good or bad that 40% of Austrians get a PHE. Is that
a waste of time and money? It would be useful to raise this issue in introduction and discussion.

Response:
We gratefully included the suggested additional citations into the discussion and reference list.

Comment:
Given the audience of this journal, it would be very helpful to give more context on the use of PHEs in other OECD countries. I’m most familiar with the US experience where PHEs are not recommended by any national guidelines, but many patients and physicians think they are helpful and payers will pay for them.

Response:
In the revised version of the manuscript, more context on the use of PHEs in other OECD countries besides Austria is given (first part of the background section).

Comment:
It would also be helpful to provide a bit more context on the financial issues for physicians related to a PHE. In the US, a PHE is reimbursed slightly higher than a regular office visit. So if a physician is seeing a patient every 2 months for a chronic illness, often the physician will call one of those regular visits a PHE to earn higher revenue. That is why patients with chronic illness are more likely to get a PHE. I’m not sure if that experience is relevant to the Austrian setting.

Response:
As in the United States of America the Austrian PHE is slightly higher reimbursed than a regular office visit.

We agree with the reviewer, that this fact might contribute to the explanation why people with chronic diseases show a higher attendance rate at PHE. We have included this aspect in the revised version of the background and discussion section of the manuscript (line 89 and line 273).

Comment:
Does the survey include any questions about other contact with the health care system? If so, I would be interested in whether patients who see physicians for other reasons are also more likely to get a PHE. Chronic illness might be a rough proxy for utilization, but I wonder if there are others.3) bitte Frage an Thomas

Response:
Unfortunately, the survey did not include data that can be used to elaborate this very interesting question.

Comment:
they state that PHEs should be targeted to younger adults. However, later they say the guidelines recommended older adults get more frequent PHEs.

Response:
The guidelines recommend the use of PHEs basically in the same way for both, younger and older adults, but a more frequent use of PHEs for older adults, to account for the positive association between age and the likelihood to develop health related complaints. Thereby the recommendations distinguish between different age groups only with respect to the suggested interval.

Regardless of these recommendations, however, actual results show, that younger people are generally less likely to perform a PHE. We therefore concluded that it would be important to develop strategies to increase general attendance rates in younger adults.

We have clarified this in more detail in the modified version of the background section (line 97-100) of the manuscript.

Comment:
They could compare their estimates to other countries. For example, the study above by Mehrotra and colleagues estimated use of PHE in the US.

Response:
In the revised version of the manuscript estimates have been compared to other countries (second paragraph of the discussion section).

Comment:
In abstract, they should make it clear that ~40% of Austrians receive this “in the last three years”

Response:
We have modified the abstract section accordingly.

Comment:
While country of birth was an important predictor, it would be useful to explain why they included this variable in the model. It seemed an important predictor, but why.

Response:
Country of birth has been included in the regression model, as in Austria already more than one million people have migration background, meaning that they were born abroad and/or do not have Austrian nationally. It has been shown
previously, that Austrians with migration background have different health
behaviours (Dorner et al., 2013), which could also influence the attendance rates
of health check-ups. As it is very likely, that numbers will rise further within the
next future, it is important to explore the causative mechanisms for their
integration deficits with respect to education, career opportunity and also health
outcome.

The discussion has been extended: Sixth paragraph of the revised version of the
manuscript.

Comment:

Only 2/3 of patients responded to the survey. For many questions, this might
not be an issue, but it seems to me that those who respond to the survey are
more likely to get a PHE. They should note in their limitations that they could be
overestimating the prevalence of PHEs due to survey response bias.

Response:
The limitation section at the end of the discussion has been expanded
accordingly.

Comment:

Their interpretation is that the rate of PHE are “high”. I question that
interpretation. The Austrian government is recommending that people < 65
receive a PHE every 3 years, but only 40% of adults do so. That seems quite
low. For adults over >65, one can guess that ~25% have a PHE every 2 years
contrary to recommendations. They might want to comment on the low rate of
PHE use.

Response:

We relativized our conclusion on a “high” rate of PHE performance. 40% adherence rate to recommendations is higher compared to other countries, but still not as high as intended, underlying the need for investigating potential predictors for adherence/non-adherence.

The abstract, discussion (second paragraph) and conclusion and policy
implication section have been modified.

Comment:

The results section is quite hard to read. They might break up the single
paragraph that goes on for 1.5 pages into several paragraphs. Also, instead of
repeating the results in each table, they might consider just focusing on the key
results and actually providing the numbers.

Response:
The entire results section has been modified and streamlined substantially.

Comment:
Very minor issue. In their Table 2, it was a bit awkward to make the reference group middle aged adults. They might consider making the reference group the oldest or youngest.

Response:
Table 2 has been modified accordingly.

Comment:
I didn’t find Table 3 results that helpful in understanding why PHEs are important. I would drop it or explain in more depth why looking at individual chronic illness diagnoses are important.

Response:
Table 3 has been removed.

We hope that we addressed the comments of the Reviewer adequately so that the revised version of the manuscript will be suitable for publication in “BMC PH”.

Sincerely,
Sophie Brunner-Ziegler