

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Utilisation of Preventative Health Check-Ups in UK: Findings from individual-level repeated cross-sectional data from 1992 to 2008
AUTHORS	Labeit, Alexander; Peinemann, Frank; Baker, Richard

VERSION 1 - REVIEW

REVIEWER	Carrieri, Vincenzo University of Salerno
REVIEW RETURNED	19-Jun-2013

THE STUDY	<p>1. The paper aims at investigating the determinants of health check-ups in the UK. The definition of the research question is too vague and the theoretical motivations of the paper are not sufficiently clear.</p> <p>5. While I think that the method is appropriate, the paper fails to justify carefully the adoption of such a method in this particular context. This is perhaps related to the fact that the research interest of the paper is too broad and there is no a special focus on one or few determinants.</p> <p>8- The paper fails to explain the potential consequences of the model assumptions in the specific context of health check-up and it does not explain why the method adopted give strength to the results compared with the methods adopted in previous papers.</p>
RESULTS & CONCLUSIONS	<p>3- The discussion of results is confused in many parts and the economic or medical explanation of results are often not discussed.</p> <p>6. The message of the paper is not sufficiently clear because the research interest is perhaps too broad. A sharper focus on some specific aspects of the determinants of check-ups may help to clarify the motivations of the paper. For instance, it might be interesting to deeply investigate the role of the GP or the effect of the policy changes on check-up instead of analyzing superficially many aspects. This leads to a confuse message for the reader.</p>
GENERAL COMMENTS	<p>1. Introduction is not clearly written. I think it would be better to explain from the very beginning the motivations of the paper and the contributions of the paper to the literature. The passage from the description of the institutional setting to the economic modelling is too sharp. I further suggest to shorten the introduction and include a paragraph of literature review on the determinants of</p>

preventive care use with a detail on the economic motivations for the inclusion of each variable in the model.

2. The method seems to be appropriate for the research interest of the paper. Still, I think it is necessary to motivate economically and not just statistically the assumptions behind the model used in the paper. For instance, it is not clear why the author includes the average value of few time-varying variables (income and health status) in the auxiliary regression of the specific random effects. There are other time-varying variables which might be correlated with unobservables, such as smoking behaviour (with risk aversion) or education (with time preferences) and so on. I suggest to include all variables in the auxiliary regression, or at least to explain the rationale behind the inclusion of only that variables.

2. I am not completely sure that the introduction of the average of health status as it is measured is really meaningful and innocuous for the estimates. Self-assessed health is an ordinal variable and not a cardinal one. Which is the interpretation of an average self-assessed health of 2.21 (as indicated in table 1 of the paper)? Is this inclusion innocuous? The authors should explain why they think it is not problematic in their context. Eventually, they may think to consider other health status variables.

3. One limitation of the paper (recognized by the authors) is that the results of previous check-ups are not known. At the same time, the authors argue at page 13 line 52 that "dynamic specification chosen takes into consideration this aspect". The reader would like to know how the model takes into account this problem.

4. It seems that the authors consider also individuals had check up in a private setting in their analysis. Check-up undertaken in a private setting is coded as 0 in the definition of their dependent variable. In this way the reference category is confused because the model estimates the probability of take preventive care in a NHS setting out of the probability of not taking preventive care at all or in a private setting. I am wondering whether it is better to do not include individuals had check up in a private setting in the analysis to make easier the interpretation of the effects. Or at least it should be explained whether the inclusion or the exclusion of such observations is innocuous or not for the estimates (ie. Does it generate sample selection issues?). More generally, I think it is necessary a deeper discussion around the inclusion of private check-ups in the analysis and also some descriptive statistics on the recourse to private check-ups in the UK.

5. All the discussion paragraph is based on the estimated marginal effects. However, such results are not shown in the paper. I strongly suggest to include marginal effects and their standard errors in table 2.

6. The discussion paragraph does not explain why the paper is relevant for the literature and which are the main messages of the paper. Perhaps a sharper focus on some specific aspects of the determinants of check-up would allow a more interesting discussion of the results.

7. One of the determinant of the model is a policy change variable.

	<p>Still, it is not clear how it is measured and what it aims to investigate. From what I may understand such variable refers to a policy changes extending the time interval but it should be explained a bit better to the reader that may ignore such policy. Still, If it is the case, is it measured as a dummy variable for the year of the policy (ie. 2003)? If yes, the authors should explain how the effect of the policy is identified their model. It seems to me that in this way the effect is not identified as it is confused with the effect of any possible thing happened in that year in the UK. My personal suggestion is to investigate carefully this point which is extremely important. Alternatively, I would not include it in the model.</p> <p>8. The authors argue at page 25 (conclusion) that the main innovation of their analysis is the method adopted. I think that this affirmation is not very useful as the innovation is not necessarily a good feature of the paper if the authors do not explain why the method give strength to their results compared with the methods adopted in other related papers . More generally, I think that the main problem of the paper in its current version is that it is difficult for the reader to appreciate its message. Thus, I strongly suggest to focus first on the economic or medical motivations of the analysis and only after on statistical aspects.</p>
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REVIEWER	Patnick, Julietta NHS Cancer Screening Programmes
REVIEW RETURNED	23-Aug-2013

THE STUDY	The authors refer to other similar studies and compare their results to these other studies. But they do not reference Moser K, Patnick J, Beral V. Inequalities in reported use of breast and cervical screening in Great Britain: analysis of cross sectional survey data. BMJ. 2009 Jun 16;338:
RESULTS & CONCLUSIONS	<p>The authors refer (p23 line 39) to the reduction in cervical screening interval from 5 to three years in younger women was intended to increase uptake in that age group. This is their assumption. Where is the evidence for this? All public statements at the time referred to increasing the protection afforded by cervical screening in that age group</p> <p>The reason extending the breast screening age group had a noticeable effect is that the breast screening programme sends out timed appointments and has a strictly policed screening interval. This is not true of any of the other interventions in this paper.</p>
GENERAL COMMENTS	<p>This is a somewhat technical paper on an interesting question. It is unfortunately badly informed in a few key areas.</p> <p>It considers women in England, Wales and Scotland, but does not describe the different health policies in those three countries accurately.</p> <p>Cervical screening starts at 25 in England since 2003 but still at 20 in</p>

	<p>Scotland and Wales. Screening in that age group is now universally 3 yearly, but in England previous to 2003 was AT LEAST 5 yearly and many women were in fact screened three yearly which is evident from the statistics bulletins of the programme. In addition, in Scotland the cervical screening programme stops inviting women at 60 and both Scotland and Wales call women 3 yearly over 50. Indeed again prior to 2003 many women over 50 in England were screened 3 yearly. Finally, in England, screening is not offered to those 65 and over who have never been screened, although it is available.</p> <p>There is a fundamental difference between the breast and cervical programmes which send out routine periodic invitations to women and dental and eye checks where invitations are left to individual practices to decide upon. Healthchecks for cholesterol etc are only being rolled out as an invitational programme now. No comment is made on these different designs and the effects they might have</p> <p>Eyesight checks in Scotland are free for the over 60s as in dental care in Wales.</p> <p>The sentences lines 15-30 on page 9 have odd grammar and need attention to the wording</p> <p>It is not clear (lines 8-12 on page 10) whether employment predicts attendance or non-attendance for cervical cancer screening. This whole section is difficult to read and does not flow. There is a jerky notes style employed. It is a list rather than a description</p> <p>The reference age group for cervical screening is 16-24. But many women aged 20-24 will have been invited for screening because they lived in Wales or Scotland or because the change in policy had not yet been implemented in their part of England. It took some time for the policy to be in place across the country. Does this affect the findings? Is this an appropriate reference group?</p>
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VERSION 1 – AUTHOR RESPONSE

1. The paper aims at investigating the determinants of health check-ups in the UK. The definition of the research question is too vague and the theoretical motivations of the paper are not sufficiently clear.

The research question is given more precisely: The determinants for different health check-ups are compared in UK, because only very few papers have done this and there is a focus on health related variables. Main motivation is to compare the determinants for the different health check-ups: which are common determinants and which are differing determinants?

2. While I think that the method is appropriate, the paper fails to justify carefully the adoption of

such a method in this particular context. This is perhaps related to the fact that the research interest of the paper is too broad and there is no a special focus on one or few determinants.

It is now described at the beginning of the methods section (page 13) why the adoption of dynamic panel data models is sensible for analysing uptake behaviour.

3. The paper fails to explain the potential consequences of the model assumptions in the specific context of health check-up and it does not explain why the method adopted give strength to the results compared with the methods adopted in previous papers.

The breast cancer screening programme (NHSBSP) and cervical cancer screening programme (NHSCSP) were introduced in 1988 before the beginning of the BHPS and also the four other health check-ups had been available to individuals before the BHPS had started. It is now mentioned on page 16 that for our estimation technique the assumption exists that the health check-ups which had been done before the first wave of the BHPS are uncorrelated with the health check-ups for which information is in the BHPS available. If this assumption is violated the inclusion of initial conditions of health check-ups for the years 1992 to 1994 could result in biased estimates for our regressions. The possible disadvantages of statistical techniques used by previous papers are now mentioned in the methods section. Moser et al. (2009) have used cross-sectional data and Sabates et al. (2006) and Sabates et al. (2008) have used unbalanced panel data. Using balanced panels is preferable, because they satisfy the assumptions of the Mundlak-Wooldridge estimator.

4. The discussion of results is confused in many parts and the economic or medical explanations of results are often not discussed.

The discussion is now more clearly structured. The covariates are discussed in the following sequence: first are mentioned aspects of dynamic modelling (state dependence, initial conditions), then age, role of health-related variables (GP, SAH, specific health problems, smoking) and the 3 policy changes. The influence of socioeconomic determinants is discussed shortly and at the end the interpretation of an averaged variable (i.e. averaged (permanent) income). Also results are discussed in a medical and economic framework, with a pronunciation of medical reasons for the regression results.

5. The message of the paper is not sufficiently clear because the research interest is perhaps too broad. A sharper focus on some specific aspects of the determinants of check-ups may help to clarify the motivations of the paper. For instance, it might be interesting to deeply investigate the role of the GP or the effect of the policy changes on check-up instead of analyzing superficially many aspects. This leads to a confuse message for the reader.

Main focus of the paper discussion is the comparison between the different health check-ups. The focus of the analysis and interpretation is now more on health related variables (e.g. GP visit, self-assessed health). We have thought about to analyse the role of GP or the effect the policy change, however this creates new problems. The institutional setting for the different health check-ups is different. For cervical cancer screening, blood pressure check and cholesterol test: these tests can be done in a GP practice or another medical institution. For breast cancer, dental screening and

eyesight test: these tests cannot be done in GP practices, because they are offered in other specialised medical institutions and a previous visit of a GP is not necessary. For the first 3 health check-ups a recursive probit would be a sensible technique, for the last 3 health check-ups such a technique would be not sensible. Policy changes (changing screening guidelines) are only for three of the health checks-ups available. However, for the future it is planned to analyse certain health check-ups more deeply such as the role of the GP for cervical cancer screening with a recursive probit.

1. Introduction is not clearly written. I think it would be better to explain from the very beginning the motivations of the paper and the contributions of the paper to the literature. The passage from the description of the institutional setting to the economic modelling is too sharp. I further suggest to shorten the introduction and include a paragraph of literature review on the determinants of preventive care use with a detail on the economic motivations for the inclusion of each variable in the model.

First, there is the motivation of our paper given, that there is only one publication (Moser et al. 2009) which compares the determinants for the different health check-ups in UK. Also the analysis of health related determinants of uptake such as the role of GP and health status for the comparison of uptakes for the different health check-ups is done in our paper which has not be done in other papers. At the end of the first paragraph the structure of the outline is given (The passage from the description of the institutional setting to the economic modelling is too sharp): In the next section the institutional regulations of the six different health check-ups are introduced, then followed by the theoretical framework for our analysis and then there is the (economic) motivation given why a specific variable is included in the regressions with a discussion of previous empirical results which is related to our own work. Considering all the reviewer comments leads to longer introduction.

2. The method seems to be appropriate for the research interest of the paper. Still, I think it is necessary to motivate economically and not just statistically the assumptions behind the model used in the paper. For instance, it is not clear why the author includes the average value of few time-varying variables (income and health status) in the auxiliary regression of the specific random effects. There are other time-varying variables which might be correlated with unobservables, such as smoking behaviour (with risk aversion) or education (with time preferences) and so on. I suggest to include all variables in the auxiliary regression, or at least to explain the rationale behind the inclusion of only that variables. I am not completely sure that the introduction of the average of health status as it is measured is really meaningful and innocuous for the estimates. Self-assessed health is an ordinal variable and not a cardinal one. Which is the interpretation of an average self-assessed health of 2.21 (as indicated in table 1 of the paper)? Is this inclusion innocuous? The authors should explain why they think it is not problematic in their context. Eventually, they may think to consider other health status variables.

I have taken 4 different variables into consideration which are available in the BHPS for measuring the health status:

1. Self-assessed health (SAH)
2. Caseness score from the Subjective wellbeing General Health Questionnaire (GHQ) with a 12 point

scale from least distressed to most distressed

3. Information about existing health problems (yes (1)/no(0))

4. GP visits (yes/no)

The first 2 variables are measured on an ordinal scale. The first one (SAH) has been used by Sabates (2006) and Labeit et al. (2013) to analyse the uptake of cervical cancer screening with BHPS. The self-assessed health (SAH) scale has properties of a continuum scale from poor to good health (Manderbacka et al. (1998): Examining the continuity of self-rated health. *Int J Epidemiol.* 1998 Apr;27(2):208-13) and this variable could be potentially used as a continuous measured variable. However, because I want to analyse the effect of health status on uptake in more detail the five possible categories of SAH have been used for the regression analysis. The descriptive statistics table (table 1) gives the frequencies for SAH (excellent, good, fair, poor, very poor). The regressions use the category excellent SAH as reference category and the four categories (good, fair, poor, very poor) as predictor variables. For measuring the health status of an individual there are further variables in the BHPS available: information about existing health problems and information about a visit of a GP. Information about existing health problems (yes (1)/no(0)) and also the averaged variables were not significant in any of the regressions. So I have used in the regressions (and auxiliary regressions) for every wave of the balanced panel SAH as categorical variable, GP visit and their averaged variables as predictor variables.

3. One limitation of the paper (recognized by the authors) is that the results of previous check-ups are not known. At the same time, the authors argue at page 13 line 52 that "dynamic specification chosen takes into consideration this aspect". The reader would like to know how the model takes into account this problem.

This section (p. 13) why a dynamic specification with lags for the previous 3 years has been clarified: considering routine check-ups according to the screening guidelines (third order lag) or as a response to an inconclusive result from a health check-up in the previous year (first order lag) or belonging to a high risk group (first order lag).

4. It seems that the authors consider also individuals had check up in a private setting in their analysis. Check-up undertaken in a private setting is coded as 0 in the definition of their dependent variable. In this way the reference category is confused because the model estimates the probability of take preventive care in a NHS setting out of the probability of not taking preventive care at all or in a private setting. I am wondering whether it is better to do not include individuals had check up in a private setting in the analysis to make easier the interpretation of the effects . Or at least it should be explained whether the inclusion or the exclusion of such observations is innocuous or not for the estimates (ie. Does it generate sample selection issues?). More generally, I think it is necessary a deeper discussion around the inclusion of private check-ups in the analysis and also some descriptive statistics on the recourse to private check-ups in the UK.

Also this section has been clarified. Analysis is restricted only to health check-ups which are provided by the NHS. This restriction makes the analysed sample more homogenous and also it is possible to compare our results to the results of Sabates et al. (2006) and Carney et al. (2013). Also it is sensible to restrict the sample in this way, because all individuals in UK are in the NHS. The possible influence of having a private insurance on prevention uptake has been analysed for UK by Courbage et al.

(2004; Prevention and Private Health Insurance in the U.K., Geneva Papers on Risk & Insurance) and also the possible endogeneity of private health insurance status.

5. All the discussion paragraph is based on the estimated marginal effects. However, such results are not shown in the paper. I strongly suggest to include marginal effects and their standard errors in table 2.

Marginal effects with standard errors for the main variables are given in table 3.

6. The discussion paragraph does not explain why the paper is relevant for the literature and which are the main messages of the paper. Perhaps a sharper focus on some specific aspects of the determinants of check-up would allow a more interesting discussion of the results.

It is explained in the first 2 sentences of the discussion section (p. 22), why the paper is relevant for the literature and what is innovative. There is now a sharper focus on the health related variables (role of GP and health status) on the uptake.

7. One of the determinant of the model is a policy change variable. Still, it is not clear how it is measured and what it aims to investigate. From what I may understand such variable refers to a policy changes extending the time interval but it should be explained a bit better to the reader that may ignore such policy. Still, if it is the case, is it measured as a dummy variable for the year of the policy (ie. 2003)? If yes, the authors should explain how the effect of the policy is identified their model. It seems to me that in this way the effect is not identified as it is confused with the effect of any possible thing happened in that year in the UK. My personal suggestion is to investigate carefully this point which is extremely important. Alternatively, I would not include it in the model.

For analysing the policy changes for the 3 different health check-ups (breast cancer screening, cervical cancer screening, dental screening) a dummy coding is chosen and the coding is explained: for years before and announcing the year of policy change a 0 and for all the following years after a policy change 1 was coded. It is tried to make the distinction for each of these health check-ups in a period before and after policy change. A difference-in-difference (DiD) approach for these 3 different health check-ups (breast, cervical and dental screening) is not feasible, because control groups are missing.

8. The authors argue at page 25 (conclusion) that the main innovation of their analysis is the method adopted. I think that this affirmation is not very useful as the innovation is not necessarily a good feature of the paper if the authors do not explain why the method give strength to their results compared with the methods adopted in other related papers. More generally, I think that the main problem of the paper in its current version is that it is difficult for the reader to appreciate its message. Thus, I strongly suggest to focus first on the economic or medical motivations of the analysis and only after on statistical aspects.

The motivation for our paper is described: First, there are no analyses which describe with the same data set (sample) the medical and socioeconomic determinants of uptake with a focus on health-related variables. Second, the advantage of estimating a balanced panel in comparison to an

unbalanced panel is given (attrition bias).

Reviewer: Prof Julietta Patnick, NHS Cancer Screening Programmes, Public Health England UK

1. The authors refer to other similar studies and compare their results to these other studies. But they do not reference Moser K, Patnick J, Beral V. Inequalities in reported use of breast and cervical screening in Great Britain: analysis of cross sectional survey data.

BMJ. 2009 Jun 16;338:

The study of Moser et al. (2009) is now referenced. First it is mentioned in the introduction (p. 6) and then compared to own results in the discussion section (p. 29).

2. The authors refer (p23 line 39) to the reduction in cervical screening interval from 5 to three years in younger women was intended to increase uptake in that age group. This is their assumption. Where is the evidence for this? All public statements at the time referred to increasing the protection afforded by cervical screening in that age group.

The reason why a uniform 3 yearly recall period for women between age and 25-49 was implemented is explained (recommendation by Cancer Research UK, because a 3-year recall policy seemed most effective after analysis of UK data). Results for the policy change variable (year after 2003xage group 25-49) are interpreted in accordance with data from the Cervical Bulletin Statistics Data (falling coverage rates and especially younger women in the target groups).

3. The reason extending the breast screening age group had a noticeable effect is that the breast screening programme sends out timed appointments and has a strictly policed screening interval. This is not true of any of the other interventions in this paper.

The reason why breast screening programme sends was effectful (timed appointments and a strictly policed screening interval) is mentioned in the discussion section.

4. This is a somewhat technical paper on an interesting question. It is unfortunately badly informed in a few key areas.

It considers women in England, Wales and Scotland, but does not describe the different health policies in those three countries accurately. Cervical screening starts at 25 in England since 2003 but still at 20 in Scotland and Wales. Screening in that age group is now universally 3 yearly, but in England previous to 2003 was AT LEAST 5 yearly and many women were in fact screened three yearly which is evident from the statistics bulletins of the programme. In addition, in Scotland the cervical screening programme stops inviting women at 60 and both Scotland and Wales call women 3 yearly over 50. Indeed again prior to 2003 many women over 50 in England were screened 3 yearly. Finally, in England, screening is not offered to those 65 and over who have never been screened, although it is available.

The different health policies for cervical cancer screening for England, Scotland and Wales are now described in the introduction (NHS Cervical Screening Programme (NHSCSP) programme

description).

5. There is a fundamental difference between the breast and cervical programmes which send out routine periodic invitations to women and dental and eye checks where invitations are left to individual practices to decide upon. Health checks for cholesterol etc. are only being rolled out as an invitational programme now. No comment is made on these different designs and the effects they might have.

The implementation for every of these programmes is now described shortly in the introduction and also the possible effect of the different implementations on uptake rates is discussed shortly in the discussion section (p. 27).

6. Eyesight checks in Scotland are free for the over 60s as in dental care in Wales.

The recommendations for dental screening and eyesight test for Scotland Wales are described in the introduction. Also references are given.

7. The sentences lines 15-30 on page 9 have odd grammar and need attention to the wording

Odd grammar for this section has been corrected (p. 10).

8. It is not clear (lines 8-12 on page 10) whether employment predicts attendance or non-attendance for cervical cancer screening. This whole section is difficult to read and does not flow. There is a jerky notes style employed. It is a list rather than a description

The whole section which is about the influence of variables has been rewritten including the section about the variable employment (p. 11). It is clarified that employment could involve opportunity costs for visiting the screening examinations and empirical results are mixed.

9. The reference age group for cervical screening is 16-24. But many women aged 20-24 will have been invited for screening because they lived in Wales or Scotland or because the change in policy had not yet been implemented in their part of England. It took some time for the policy to be in place across the country. Does this affect the findings? Is this an appropriate reference group?

There are different possibilities for selecting the reference group for cervical (and breast) cancer screening possible.

Carney et al. (2013) have selected for breast cancer screening (similar problem as cervical cancer) screening: 16-49, 50-64, 65-70, 71+ (women of all ages and not only in the target groups). Sabates et al. (2006) have used for their estimation women of age 22-65 (women only within the NHSBSP and women could be not invited directly after they reach 20). In our opinion there are the 2 following age categorisation possibilities sensible:

Age categorisation in specification 1: age 16-19 (ref.), 20-24, 25-49, 50-64, 65+ (sample age ≥ 16).

Age categorisation in specification 2: age 20-24 (ref.), 25-49, 50-64, 65+ (sample age ≥ 20).

We have selected to choose age categorisation as in specification 1 (20-24 with reference group 16-

19) in the comparison table 2 for the different health check-ups. The estimation results in specification 1 and 2 with the different age categorisations are very similar for all other variables. This regression table is added as additional file (Technical appendix).

Our balanced panel estimation sample includes 867 women for each year for England, Wales and Scotland. It is no information available for us with how much delay the policy change was implemented in England in the different PCT areas- also mentioned in the NHSCSP programme description. Considering the small number of women in this age group (275 for the whole balanced panel) the number of women in Wales and Scotland is even smaller in comparison to England, so that analyses for this age group with differentiating the area in which they live in UK would be difficult.

VERSION 2 – REVIEW

REVIEWER	Carrieri, Vincenzo University of Salerno
REVIEW RETURNED	04-Oct-2013

GENERAL COMMENTS	<p>The authors responded adequately to some of my comments and suggestions. However, I still have some concerns.</p> <p>Here below my suggestions:</p> <p>1. Introduction is not concise and clearly written. It includes both institutional arrangements for health check-ups in the UK and an economic explanation of the inclusion of variables in the model. I think that the authors should shorten this paragraph or alternatively they should include economic motivations in a new paragraph dealing with economic and health services literature on the determinants of preventive care use. I also suggest to dense all institutional aspects related to health check-ups in one table explaining for each check-up, screening recommendations (age limits and comorbidities), time span of invitations, exceptions (ie. differences between countries in age limits) and user charges.</p> <p>2. Auxiliary regression: The authors responded partially to my previous comment related to auxiliary regression (comment number 2 of previous referee report). They include the mean of other health variables in the auxiliary regression but they fail to justify why other time-varying variables are not included in the auxiliary regression. For instance, smoking behaviour, education, family status (married or not, have a child or not) are all time-varying variables potentially correlated with important unobservables such as risk aversion, time preferences, etc. I think that this inclusion might be relevant in this case. I thus suggest to re-run estimates as additional check or alternatively, the authors should explain why they think that only health status variables are needed in their auxiliary regression.</p> <p>3. I understand that a diff-in-diff approach is not feasible to test the effects of policy changes. Thus, if I have understood well, the effect</p>
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	<p>of the policy is tested only through a before-after comparison which is not the best possible alternative to assess the causal effect of the policy, ie. it fails to take into account unobservables varying in that period such as macroeconomic conditions. The authors should at least recognize this limitation in the discussion paragraph and give less emphasis to this result.</p> <p>4. Discussion paragraph is now much better with respect to the motivation of the paper and the contribution to the literature. Still I think it is not written in a concise way. I suggest to comment results for each determinant with a quick reference to results found in other studies.</p>
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VERSION 2 – AUTHOR RESPONSE

1. We have followed the second proposal and the introduction has been separated in two parts: The first part (introduction) describes the institutional arrangements for health check-ups and the second part (theoretical approach) describes the relevant economic models for preventative care and health services literature on determinants which are relevant for the use of preventative care. For all health check-ups the institutional aspects are now described in an extra table (table 1).

2. Auxiliary regressions (table 3): Averaged values for all time varying variables are used in all the estimations for the 6 different health check-ups regressions (they have also been in the previous version of this table included). Please see table 3 (row 3 (averaged household income) to row 19(averaged health problem sight)). There are not only health related variables used in the auxiliary regressions: also smoking status, education level, partnership status, etc. are used. Discussion of results for the auxiliary regressions (averaged GP visit (p. 26), averaged household income (p. 29) is short.

3. This limitation is mentioned and discussed. Also less emphasis is given to this result, because the change of medical guidelines for cervical and dental screening is not any more mentioned in the discussion part. Only the policy change in breast cancer screening is shortly discussed, because there are sensible reasons why this extension to the age group 65-70 was successful: strictly policed screening interval with timed appointments.

4. In the first part of the discussion every used determinant in the health check-up regressions is discussed with one or several references which are related to other studies. In the second short part of the discussion there are studies discussed and compared with the own study which have analysed the socioeconomic determinants of the uptake of the different health checkups with the BHPS as dataset (Carney (2013), Sabates (2006), Batchelor (2004)).