PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Temporal trend in socio-economic inequalities in the uptake of cancer screening programs in France between 2005 and 2010: Results from the Cancer Barometer Surveys
AUTHORS	Kelly, David; Estaquio, Carla; Leon, Christophe; Arwidson, Pierre; Nabi, Hermann

VERSION 1 – REVIEW

REVIEWER	Lindsay Kobayashi Harvard T.H Chan School of Public Health, USA
REVIEW RETURNED	25-Apr-2017

GENERAL COMMENTS	This manuscript presents a study which aimed to assess the magnitude of socioeconomic inequalities in uptake of breast, cervical and colorectal cancer screening in France from 2005 to 2010. I would recommend the article being proofread, as I have found a few sentences with confusing wording (see specific comments below), some instances where words are capitalized that should not be (e.g. Odds ratio in the Methods), and other instances where a word should be capitalized (e.g. all mentions of the Tables in the text of the manuscript). It might help if a native English speaker performs the proofreading. The STROBE statement was included but not complete as several key elements of study design were missing, including the study hypotheses, the response rate, the method for how the study size was arrived at, the analytical sample sizes, and methods for addressing potential sources of bias. Abstract: The abstract is written partly in present tense and partly in past tense. Please correct to use a consistent tense throughout. Please state the correct sample size (the abstract states 4000 respondents, while the manuscript states 3820 respondents in 2005 and 3727 in 2010). Strengths and limitations: Page 6 line 11: The second bullet point makes an incorrect statement. Using the same sampling frame for two surveys does not minimize selection bias.

	Introduction: Some sentences in this section are repetitive (e.g. the final sentence of the second paragraph and the first sentence of the third paragraph essentially say the same thing)/ The literature review is vague in several places (e.g. what are the actual social and economic variables that are associated with breast cancer screening uptake?) and is lacking (only 25 references, not enough literature cited). The literature review must review the existing evidence on each of the different socioeconomic indicators that are included in this study. Page 7 line 44: What does this sentence mean: "No recurring variable was observed, with the exception of participation in other screening programs [7, 17]"? Could this be rephrased to be clearer (perhaps the authors mean that no consistent predictors of breast cancer screening uptake were observed across studies)? Page 8 line 8: Please provide more detail about this previous study that investigated socioeconomic inequalities in France over time. What was the time period, what was the sampling frame, which types of cancer screening were investigated, and what were the magnitudes of socioeconomic inequalities? This information is very important to establish the rationale for your study and how it fills a gap in knowledge given that an existing study on the same question already exists. Page 8 line 34: Please state the specific types of cancer screening that are under study in the objective. Methods: Page 9, paragraph 2: Please provide either percentages, or denominators along with the numbers for sample size and numbers of men and women in the study throughout the Methods section. Page 9, paragraph 2: Please provide the response rates for the surveys. Page 9 line 41: Please define baccalaureat for readers unfamiliar with the French education system Page 10 lines 13-17: Please provide the analytical sample sizes for each of the three cancer screening uptake outcomes Page 10 line 48: Univariate logistic regression is not the correct term since there are other covariates in the model, it should be multiple logistic regression Page 11 paragraph 2: Please describe in more detail how the RII is interpreted – e.g. what does a value of 0 mean, and what does a value of 1 mean? This calculation of the RII is different from that often used (see Regidor E. Measures of health inequalities: part 2. J Epidemiol Community Health 2004; and Sergeant JC, Firth D. Relative index of inequality: definition, estimation, and inference. Biostatistics 2006; and also reference 24 in your paper which seems to calculate the RII in a different way to your study).

	Results: Table 1: Are there any significant differences in participant characteristics between the 2005 and 2010 study samples? Please provide p-values from chi-squared tests. This is absolutely crucial since you are presenting proportions from two different cross-sectional samples of people, who may differ systematically or due to random variation. Table 2: Why is there a * next to 'difference' in in the Education section? What does the difference refer to for variables with >2 categories? Where are the p-trend values referred to in the title? Tables 3-5: Please provide the sample size included in each table. Why is age not adjusted for within each model? Differences in age distributions within the screening-eligible ranges between 2005 and 2010 could affect the results. Why is the RII given only for education and income? Would it not make sense to adjust for health insurance as a covariate, as that factor could influence the RII within education and income? Discussion: Page 13 line 46: The study objective stated in the first sentence of the Discussion is different from the study objective stated in the Introduction to the study and the Abstract. If the objective really is to compare inequalities across different socioeconomic markers, then the Introduction to the study must provide a comprehensive literature review on the evidence for each of the socioeconomic indicators included in this study, and give a rationale as to why the magnitude of inequalities would differ across each. Page 14 paragraph 2: Please explain why your study findings differed from Sicsic et al. It is very important that your studies found contrasting results, and it is crucial to understand why this occurred. Could it be due to differences in study design, sampling frame, or statistical methodology, for example? Page 16 paragraph 2: You did not mention the selection bias caused by sampling people who had landline telephones. In what direction could this bias your results, and what demographic groups would be most likely to have been missed due to this sampling method? Page 17 first sentence: No, the total sample population was not nearly 4000. Please state the actual sample size, and more importantly, the actual analytical sample sizes for each of the three screening types, given the age restrictions for each. Page 16: What about type I error due to multiple comparisons as a limitation of this study? Conclusion: The first sentence of this section doesn't give any real information. This section must say more than simply stating that this research makes a contribution. The second sentence makes an unwarranted conclusion, because this study cannot determine whether organized screening programs can reduce socioeconomic disparities in screening participation. That was not the study objective and you cannot discern this from the data that were available.

REVIEWER	Christina Fitzmaurice University of Washington, Seattle, USA
REVIEW RETURNED	28-Apr-2017

GENERAL COMMENTS	The authors present an analysis of the changes in screening rates of cervical, colorectal, and breast cancer between 2005 and 2010 by socioeconomic profile. The paper is clearly written and adds important evidence that can be used to for health policy. I have some minor suggestions on what can be changed in the manuscript to make it more clear: Abstract: Page 4 row 54/56: would replace "non-organized" with "opportunistic" Strength and limitations: Page 6 row 23: I do not think excluding a hospitalized population from a screening survey is a limitation since screening program do not target inpatients. Row 29: spelling error "Rrelatively" Introduction Page 7 row 41: rephrase. If you say variables were found to be significant across different studies it sounds as if the same variable were significant in different studies, which you say in the next sentence is not the case. Page 8 row 25: do you mean "reduces incidence rates" or do you mean mortality rates? If you actually mean incidence rates, you need to add an explanation on how secondary prevention programs result in reducing incidence Row 53: delete "two" in "Both two surveys" Row 57: Change "We used a two-stage random sampling design" to "A two-stage random sampling design was used" unless you actually did the survey (if that is the case, this should be made clear in the method section) Page 9 row 2: given an example of what "collective dwellings" are Row 55 and page 10 row 3: replace "undertaken" with "undergone" Page 10 row 8 and 56: replace "living in a couple" with "living with a significant other" or "living as a couple" Page 11 row 22: clarify how the RII is calculated. Row 29: isn't it the reverse? The odds of the least favorable to the most favorable? Page 12 row 55: it is interesting why the "inactive" group had increased participation in FOBT compared to the "employed" group. Could be discussed in the discussion. Page 13 row 21: Rephrase to something like "The magnitude of the income based RII decreased for all screening programs...."; A general comment with regard to the RII: it is unclear what the authors mean by a decreased "magnitude" of the RII. My interpretation is that if the RII decreases this means INCREASED inequality Discussion: Page 14 row 15: delete "globally" Row 20: explain what the potential reasons are for the differences between your study and the Sicsic study. Row 53: not sure if this comparison is relevant if this was done in a US population

	Page 15 row 7: it is confusing to say that the RII DECREASED if the number actually INCREASED. Either redefine the RII or rephrase as "disparities decreased based on the increase in RII" or something like that Row 28: Rephrase: "Breast and colorectal screening programs are organized at a national level and differences in absolute participation rates and relative inequalities decreased over time for all socio-economic variables" Row 43: explain why manual workers would be less aware of health marketing campaigns Page 16 row 17: Rephrase "In our study we used two almost identical datasets..." Row 34: again, I am not sure if excluding hospitalized patients from this survey is a limitation. Row 37: somebody who's native language is not French can still speak French Table 2: it would be interesting to also see the overall change in participation rates, not just by stratum, within each socioeconomic variable.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Lindsay Kobayashi
Harvard T.H Chan School of Public Health, USA

Please state any competing interests or state ‘None declared’: None declared

This manuscript presents a study which aimed to assess the magnitude of socioeconomic inequalities in uptake of breast, cervical and colorectal cancer screening in France from 2005 to 2010. I would recommend the article being proofread, as I have found a few sentences with confusing wording (see specific comments below), some instances where words are capitalized that should not be (e.g. Odds ratio in the Methods), and other instances where a word should be capitalized (e.g. all mentions of the Tables in the text of the manuscript). It might help if a native English speaker performs the proofreading.

OUR RESPONSE 3:We thank the reviewer for these helpful comments. The manuscript has been proofread and the grammar corrected accordingly.

The STROBE statement was included but not complete as several key elements of study design were missing, including the study hypotheses, the response rate, the method for how the study size was arrived at, the analytical sample sizes, and, methods for addressing potential sources of bias.

OUR RESPONSE 4:Thank you for raising the issues. The STROBE statement is now completed with all requested information.

Abstract:The abstract is written partly in present tense and partly in past tense. Please correct to use a consistent tense throughout.

OUR RESPONSE 5: The abstract is now entirely written in the past tense.

Please state the correct sample size (the abstract states 4000 respondents, while the manuscript states 3820 respondents in 2005 and 3727 in 2010).

OUR RESPONSE 6: The correct sample size has been provided in the revised version of the abstract : “Randomly selected participants aged 15 to 85 years, 3820 in 2005 and 3727 in 2010, were questioned on their participation in breast, cervical and colorectal cancer screening programs and their socio-economic profile.”

Strengths and limitations:
Page 6 line 11: The second bullet point makes an incorrect statement. Using the same sampling frame for two surveys does not minimize selection bias.

OUR RESPONSE 7:We agree. This statement has been removed.

Introduction:
Some sentences in this section are repetitive (e.g. the final sentence of the second paragraph and the first sentence of the third paragraph essentially say the same thing)/

OUR RESPONSE 8: Thank you for this observation. The redundant sentence of the second paragraph has been removed.

The literature review is vague in several places (e.g. what are the actual social and economic variables that are associated with breast cancer screening uptake?) and is lacking (only 25 references, not enough literature cited). The literature review must review the existing evidence on each of the different socioeconomic indicators that are included in this study.

OUR RESPONSE 9: We thank the reviewer for this observation. However, the report of the literature review is intentionally vague given that our main objective is not to identify socioeconomic indicators that are associated with cancer screening uptake. As we mentioned, there is no single indicator that has been found to be consistently associated with breast cancer screening uptake across the numerous studies identified, except the participation in other screening programs. We have tried to keep the information as synthetic as possible for the readers, but giving appropriate references in case they would like to have additional information. As requested, we added some new information about the actual social and economic variables associated with breast cancer screening uptake as follows. Please see our response 10 for more details about these changes.

Page 7 line 44: What does this sentence mean: “No recurring variable was observed, with the exception of participation in other screening programs [7, 17]”? Could this be rephrased to be clearer (perhaps the authors mean that no consistent predictors of breast cancer screening uptake were observed across studies)?

OUR RESPONSE 10:We thank the reviewer for this observation, her clarification is correct. The sentence has been reworded as follows:
“For breast cancer screening, various different social and economic variables were found to have an effect, including employment, living in a couple, occupation, education level, income, private health insurance, car/home ownership and rural residency. However, no single variable was consistently observed across studies except for participation in other screening programs.”

Page 8 line 8: Please provide more detail about this previous study that investigated socioeconomic inequalities in France over time. What was the time period, what was the sampling frame, which types of cancer screening were investigated, and what were the magnitudes of socioeconomic inequalities?

This information is very important to establish the rationale for your study and how it fills a gap in knowledge given that an existing study on the same question already exists.

OUR RESPONSE 11: Thank you for pointing out this lacking details. We provided now more details as follows:
“Only one study drawn from the 2006, 2008 and 2010 French Healthcare and Health Insurance surveys [6] has examined the temporal evolution in breast, cervical and colorectal cancer screening uptake along socio-economic strata in France to date. This study conducted among 10 000 participants found that those classified as unskilled workers were less likely to have undergone cervical cancer screening (OR = 1.64 [1.38-1.95]). The results also showed that women without (OR = 2.05 [1.68-2.51]) or receiving free complementary health insurance (OR = 1.79 [1.36-2.37]) were less likely to have undergone breast cancer screening.”

Page 8 line 34: Please state the specific types of cancer screening that are under study in the objective.

OUR RESPONSE 12: As requested we have now specified types of cancer screening in the objective as follows: “We aim therefore in the present study to identify the socio-economic inequalities which persist for uptake of breast, cervical and colorectal cancer screening”

Methods:
Page 9, paragraph 2: Please provide either percentages, or denominators along with the numbers for sample size and numbers of men and women in the study throughout the Methods section.

OUR RESPONSE 13:We revised these sections as requested.

Page 9, paragraph 2: Please provide the response rates for the surveys.

OUR RESPONSE 14: We have now provided the response rate for the surveys. “The response rates for 2005 and 2010 were 51.2% and 47.0%, respectively.”

Page 9 line 41: Please define Baccalauréat for readers unfamiliar with the French education system

OUR RESPONSE 15: As requested Baccalauréat is now defined as a “High-School Degree”.

Page 10 lines 13-17: Please provide the analytical sample sizes for each of the three cancer screening uptake outcomes

OUR RESPONSE 16: As requested, we included the sample size for each eligible screening population :“(Mammography n=1545, Cervical smear n=3065, FOBT n= 2647)” .

Page 10 line 48: Univariate logistic regression is not the correct term since there are other covariates in the model, it should be multiple logistic regression

OUR RESPONSE 17: We thank the reviewer for her vigilance. We revised this section accordingly.

Page 11 paragraph 2: Please describe in more detail how the RII is interpreted – e.g. what does a value of 0 mean, and what does a value of 1 mean? This calculation of the RII is different from that often used (see Regidor E. Measures of health inequalities: part 2. J Epidemiol Community Health 2004; and Sergeant JC, Firth D. Relative index of inequality: definition, estimation, and inference. Biostatistics 2006; and also reference 24 in your paper which seems to calculate the RII in a different way to your study).

OUR RESPONSE 18: Please, note that we calculated the RII as described by Mackenbach and Kunst (Mackenbach&Kunst. Measuring the magnitude of socio-economic inequalities in health: an overview of available measures illustrated with two examples from Europe. SocSci Med. 1997;44(6):757–71).We added this reference.As requested, we described in more detail how the RII is interpreted as follows:
"For ordinal variables of income and education level, we calculated the Relative Inequality Index (RII) as a measure of health inequality as described by Mackenback and Kunst [24]. Previous studies on health inequalities, including breast cancer screening uptake [4, 9], employed a similar methodology for examining temporal evolutions along ordered socio-economic strata [22, 24]. The RII is a regression-based measure that summarises the association between two variables. It is computed by ranking income and education values on a scale from the lowest, which is 0, to the highest, which is 1. Each income or education level value covers a range on this scale that is proportional to the number of participants who had that value and is given a new value on the scale corresponding to the cumulative midpoint of its range. The RII resembles relative risk in that it compares the probability of cancer screening uptake at the extremes of income and educational levels but it is estimated using the data on all income and education values and is weighted to account for the distribution of these value. Here the RII was fitted using logistic regression models. An RII of 0.5 for example indicates a lower probability of cancer screening uptake at the lower extreme of income and education levels compared to the higher extremes or vice versa."

Results:
Table 1: Are there any significant differences in participant characteristics between the 2005 and 2010 study samples? Please provide p-values from chi-squared tests. This is absolutely crucial since you are presenting proportions from two different cross-sectional samples of people, who may differ systematically or due to random variation.

OUR RESPONSE 19: We followed the reviewer's suggestion and provided p-values from chi-squared tests in Table 1.

Table 2: Why is there a * next to 'difference' in the Education section? What does the difference refer to for variables with >2 categories? Where are the p-trend values referred to in the title?

OUR RESPONSE 20: The asterisk was used to denote a footnote explaining how the difference had been calculated. The footnote was since removed. This information is now contained within the main body of text (page 11, line 41):
"The disparity within each socio-economic variable was calculated as the absolute difference between the AAR for the highest and lowest group within an ordinal or binary variable for the given year."
The p-trend is the p-value for the Chi2 test measuring the significance of the difference in participation rate between 2005 and 2010.
"The temporal evolution in the participation rate along each stratum between 2005 and 2010 was examined by adding an interaction with the year of screening."

Tables 3-5: Please provide the sample size included in each table. Why is age not adjusted for within each model? Differences in age distributions within the screening-eligible ranges between 2005 and 2010 could affect the results. Why is the RII given only for education and income? Would it not make sense to adjust for health insurance as a covariate, as that factor could influence the RII within education and income?

OUR RESPONSE 21: Thank you for asking these questions. We have added the sample size for each table accordingly: "Table 3 (n=742), (n=804), Table 4 (n=1571), (n=1514), Table 5 (n=1222) (n=1425)".

The data were weighted by age and other variables to replicate the actual population distribution across France and was not included as a variable in the adjustment model.
RIIs are calculated only for ordinal variables for which there are more than two categories, which is a standard in the literature.
Health insurance is one of the independent variables examined in the study, and so was not included as a covariate.

Discussion:
Page 13 line 46: The study objective stated in the first sentence of the Discussion is different from the study objective stated in the Introduction to the study and the Abstract. If the objective really is to compare inequalities across different socioeconomic markers, then the Introduction to the study must provide a comprehensive literature review on the evidence for each of the socioeconomic indicators included in this study, and give a rationale as to why the magnitude of inequalities would differ across each.

OUR RESPONSE 22:We agree. We revised the discussion sentence to conform to the study objective stated in the Introduction and Abstract sections.

Page 14 paragraph 2: Please explain why your study findings differed from Sicsic et al. It is very important that your studies found contrasting results, and it is crucial to understand why this occurred. Could it be due to differences in study design, sampling frame, or statistical methodology, for example?

OUR RESPONSE 23:We thank the reviewer for raising this issue. We provided some explanations that could account for the differences between our findings and those from Sicsic et al. as follows: “Several factors may explain why some of our findings differ from those by Sicsic et al. Their study was based on data collected using three modalities: telephone, face-to-face and self-administered questionnaires. The Cancer Barometer data used in our study was collected exclusively via telephone interview. In addition, the study by Sicsic et al. was based on three surveys carried out in 2006, 2008 and 2010, with therefore a two-year interval, whereas the Cancer Barometer survey was conducted at two points in time in 2005 and 2010. Another important difference between the two studies relies on their objectives and consequently on the methods used to reach them. The study by Sicsic et al. aimed to analyze the obstacles to and levers for breast, cervical, and colorectal cancer screening uptake and their trends over time, whereas the aim of our study was to identify the socio-economic inequalities which persist for uptake of breast, cervical and colorectal cancer screening, and to quantify these disparities over a 5 year period. Thus, Sicsic et al. pooled their three samples and did not conduct direct comparisons of associations between indicators of socioeconomic position and uptake of cancer screenings between periods.”

Page 16 paragraph 2: You did not mention the selection bias caused by sampling people who had landline telephones. In what direction could this bias your results, and what demographic groups would be most likely to have been missed due to this sampling method?

OUR RESPONSE 24: Thank you for this reminder. We did originally include this as a limitation, but summarized in the final edit to: “It shares the usual shortcomings of phone surveys. There is a potential selection bias, as residents of nursing homes or other medical institutions who did not possess a personal telephone line were not included in the samples.”
Indeed, those without a landline are likely to be socio-economically disadvantaged; therefore their exclusion from the study would likely overestimate screening participation rates.
We had added the following sentence: “The exclusion of the above subpopulations, which are likely to be more socio-economically disadvantaged, may have overestimated the screening participation rates in our study”

Page 17 first sentence: No, the total sample population was not nearly 4000. Please state the actual sample size, and more importantly, the actual analytical sample sizes for each of the three screening types, given the age restrictions for each.

OUR RESPONSE 25: We agree with the reviewer. The 2 sample sizes for 2005 and 2010 have now been added to the beginning of the manuscript and displayed in table 2.We have modified the sentence to read as follows:
“The respective analytical sample sizes in 2005 and 2010 for breast (n=742 , n=804), cervical (n=1571, n=1514) and colorectal (n=1222, n=1425) cancer screening may have been too small to capture disparities along socio-economic strata.”

Page 16: What about type I error due to multiple comparisons as a limitation of this study?

OUR RESPONSE 26: We agree with the reviewer that multiple comparisons might be an issue and therefore included this as a potential limitation as follows: “We undertook multiple comparisons in our study. Thus, we cannot exclude that some of the results we have observed are due to chance.”

Conclusion:
The first sentence of this section doesn’t give any real information. This section must say more than simply stating that this research makes a contribution. The second sentence makes an unwarranted conclusion, because this study cannot determine whether organized screening programs can reduce socioeconomic disparities in screening participation. That was not the study objective and you cannot discern this from the data that were available.

OUR RESPONSE 27: We agree with the first part of the reviewer’s comments and removed the first sentence. However, we kept the second sentence because it put our findings into perspective. We have been prudent in our conclusion by stating that “organized screening programs have the potential to reduce socio-economic disparities in participation” based upon the observations over time in the study.

Reviewer: 2
Christina Fitzmaurice
University of Washington, Seattle, USA
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below
The authors present an analysis of the changes in screening rates of cervical, colorectal, and breast cancer between 2005 and 2010 by socioeconomic profile. The paper is clearly written and adds important evidence that can be used to for health policy.

OUR RESPONSE 28:We thank the reviewer for these positive and encouraging comments.

I have some minor suggestions on what can be changed in the manuscript to make it more clear:
Abstract: Page 4 row 54/56: would replace "non-organized" with "opportunistic"

OUR RESPONSE 29: Thank you for this suggestion. We have revised the abstract accordingly.

Strength and limitations: Page 6 row 23: I do not think excluding a hospitalized population from a screening survey is a limitation since screening program do not target inpatients.

Row 29: spelling error "Rrelatively"

OUR RESPONSE 30: We thank the reviewer for her vigilance; the spelling error is duly corrected. We also removed the limitation concerning the exclusion of hospitalized population.

Introduction
Page 7 row 41: rephrase. If you say variables were found to be significant across different studies it sounds as if the same variable were significant in different studies, which you say in the next sentence is not the case.

OUR RESPONSE 31: Thankyou for pointing this out. We rephrased this section as follows:
“We reviewed several publications from France, UK, USA, Italy, Denmark, Korea and Argentina, which identified variables shown to have a significant effect on cancer screening uptake [6-19]. For breast cancer screening, various different social and economic variables were found to have an effect, including employment, living in a couple, occupation, education level, income, private health insurance, car/home ownership and rural residency. However, no single variable was consistently observed across studies except for participation in other screening programs [7,17] ”.

Page 8 row 25: do you mean "reduces incidence rates" or do you mean mortality rates? If you actually mean incidence rates, you need to add an explanation on how secondary prevention programs result in reducing incidence

OUR RESPONSE 32: Many thanks for this insightful suggestion. We agree and have therefore replaced “incidence” with “mortality”.

Row 53: delete "two" in "Both two surveys"

OUR RESPONSE 32: Thanks, duly deleted.

Row 57: Change "We used a two-stage random sampling design" to "A two-stage random sampling design was used" unless you actually did the survey (if that is the case, this should be made clear in the method section)

OUR RESPONSE 33: We have revised this section to read: “A two-stage random sampling design was used"

Page 9 row 2: given an example of what "collective dwellings" are

OUR RESPONSE 34: We have altered the phrase throughout the manuscript to refer more precisely to long-term residents of nursing homes or medical/social/psychiatric institutions:
“Residents of nursing homes or other medical institutions who did not possess a personal telephone line were excluded from the surveys”.

Row 55 and page 10 row 3: replace "undertaken" with "undergone"

OUR RESPONSE 35: It has been revised accordingly.

Page 10 row 8 and 56: replace "living in a couple" with "living with a significant other" or "living as a couple"

OUR RESPONSE 36: Thank you for this suggestion. We retained “living as a couple”.
Page 11 row 22: clarify how the RII is calculated.

OUR RESPONSE 37: The first reviewer asked the same question. We realized that we have not been clear in the description of the calculation of the RII.We revised this section as follows:
“For ordinal variables of income and education level, we calculated the Relative Inequality Index (RII) as a measure of health inequality as described by Mackenback and Kunst [24]. Previous studies on health inequalities, including breast cancer screening uptake [4, 9], employed a similar methodology for examining temporal evolutions along ordered socio-economic strata [22, 24]. The RII is a regression-based measure that summarises the association between two variables. It is computed by ranking income and education values on a scale from the lowest, which is 0, to the highest, which is 1. Each income or education level value covers a range on this scale that is proportional to the number of participants who had that value and is given a new value on the scale corresponding to the cumulative midpoint of its range. The RII resembles relative risk in that it compares the probability of cancer screening uptake at the extremes of income and educational levels but it is estimated using the data on all income and education values and is weighted to account for the distribution of these value. Here the RII was fitted using logistic regression models. An RII of 0.5 for example indicates a lower probability of cancer screening uptake at the lower extreme of income and education levels compared to the higher extremes or vice versa.”

Row 29: Isn't it the reverse? The odds of the least favorable to the most favorable?

OUR RESPONSE 38: Our response 37 and the new section about the description of the calculation of the RI clarify now this point.

Page 12 row 55: it is interesting why the "inactive" group had increased participation in FOBT compared to the "employed" group. Could be discussed in the discussion.

OUR RESPONSE 39: We now discussed this finding as follows in Interpretation of results:
“Those classified as inactive (retired, homemakers) may have more free time to attend for colorectal cancer screening, explaining therefore the increased FOBT participation. It is possible that a greater proportion of inactive people is retired and hence falls within the eligible population for FOBT screening (50 to 74 years), compared with younger employed group.”

Page 13 row 21: Rephrase to something like "The magnitude of the income based RII decreased for all screening programs...."; A general comment with regard to the RII: it is unclear what the authors mean by a decreased "magnitude" of the RII. My interpretation is that if the RII decreases this means INCREASED inequality

OUR RESPONSE 40: Thank you for your insight on this interpretation. The word magnitude might be misleading, as a decrease in magnitude may signal an increase in inequality. We have modified the sentences by removing the word magnitude as follow:
“The income-based RIIs decreased for all 3 screening programs in 2010, meaning a reduction in income-based inequalities, and remained significant only for cervical smear...”
And
“In 2010, the education-based RII decreased for mammography and became non-significant (RII=0.73, 0.23-2.24)…”

Discussion:
Page 14 row 15: delete "globally"

OUR RESPONSE 41:As requested, we removed the term “globally”

Row 20: explain what the potential reasons are for the differences between your study and the Sicsic study.

OUR RESPONSE 42: The first reviewer made the same comment. We provided in the revised version of the manuscript the potential reasons of the differences as follows:
“Several factors may explain why some of our findings differ from those by Sicsic et al. Their study was based on data collected using three modalities: telephone, face-to-face and self-administered questionnaires. The Cancer Barometer data used in our study was collected exclusively via telephone interview. In addition, the study by Sicsic et al. was based on three surveys carried out in 2006, 2008 and 2010, with therefore a two-year interval, whereas the Cancer Barometer survey was conducted at two points in time in 2005 and 2010. Another important difference between the two studies relies on their objectives and consequently on the methods used to reach them. The study by Sicsic et al. aimed to analyze the obstacles to and levers for breast, cervical, and colorectal cancer screening uptake and their trends over time, whereas the aim of our study was to identify the socio-economic inequalities which persist for uptake of breast, cervical and colorectal cancer screening, and to quantify these disparities over a 5 year period. Thus, Sicsic et al. pooled their three samples and did not conduct direct comparisons of associations between indicators of socioeconomic position and uptake of cancer screenings between periods.”

Row 53: not sure if this comparison is relevant if this was done in a US population

OUR RESPONSE 43: We agree that our findings are not directly comparable to those obtained in a US population. However, we tried in the discussion to put our results into perspective and in the context of the literature on this topic.

Page 15 row 7: it is confusing to say that the RII DECREASED if the number actually INCREASED. Either redefine the RII or rephrase as "disparities decreased based on the increase in RII" or something like that

OUR RESPONSE 44: The RII has been redefined in the text of the Methods section, see Response 37. By decreasing RII we refer to the magnitude of the inequality and not the numerical value of the RII. The inequality becomes lesser as the RII approaches a value of 1.0, similar to a relative risk or risk ratio. This is clearer on consultation of the associated figures and confidence intervals. We have modified the final paragraph in the Findings in the context of the literature section to refer to relative inequalities instead of RII values:
“The relative inequalities for income and education decreased for breast and colorectal cancer screening in our study, consistent with De Maio et al., which showed a reduction in the RII for breast cancer screening from 2005 to 2009 [19]. In the study by Kim et al.[9], the income-based relative inequalities tended to decrease slightly, while those for education remained constant over time. The relative inequalities for cervical cancer screening decreased for income and increased for education from 2005 to 2010 in our study, both remaining statistically significant”.

Row 28: Rephrase: "Breast and colorectal screening programs are organized at a national level and differences in absolute participation rates and relative inequalities decreased over time for all socio-economic variables"

OUR RESPONSE 45: Many thanks for this eloquent formulation. The sentence has been rephrased accordingly.

Row 43: explain why manual workers would be less aware of health marketing campaigns

OUR RESPONSE 46: Our hypothesis as stated in the manuscript is the lack of awareness in this group may be due to lower education, the two variables being closely correlated.

Page 16 row 17: Rephrase "In our study we used two almost identical datasets..."

OUR RESPONSE 47:We agree and revised this section accordingly.

Row 34: again, I am not sure if excluding hospitalized patients from this survey is a limitation.

OUR RESPONSE 48: We agree and removed this sentence.

Row 37: somebody who's native language is not French can still speak French

OUR RESPONSE 49: We agree and reworded the section as follows:
"The study includes only those who are francophone, excluding individuals unable to answer fluently in French."

Table 2: it would be interesting to also see the overall change in participation rates, not just by stratum, within each socioeconomic variable.

OUR RESPONSE 50: We provided now this information in Table 2, where the first row includes overall participation rates.

VERSION 2 – REVIEW

REVIEWER	Lindsay Kobayashi Harvard T.H Chan School of Public Health, USA
REVIEW RETURNED	23-Jun-2017

GENERAL COMMENTS	Thank you for inviting me to review the revised version of this manuscript, which has improved since its original submission. My remaining comments are as follows: General: I have still found some small grammatical errors and inconsistent tenses throughout the manuscript, and spelling errors in the tables (e.g. "Manuel" in Table 1 and "Pprofessional" in Table 2. I recommend further proofreading. Abstract: Page 2 lines 7-17: The Objectives should state the specific types of cancer screening under study Page 2 line 18: The tense should be "was" not "is" since the survey occurred in the past Page 2 line 23: There is a problem with this sentence, "A randomly selected inhabitants" does not make sense and the "A" is incorrectly bolded. I also recommend writing "n=3820" and "n=3727" so it is clear that you are referring to the sample size Page 2 line 35: I recommend stating the specific socio-economic variables under study so that the reader knows what you actually investigated Page 2 lines 44-52: For readers who are unfamiliar with the RII measure, I would recommend providing some information on what a smaller versus larger RII means, or at very least some information to aid the reader in interpreting this measure (either here in the Results

	section or in the Primary and Secondary Outcome Measures section). Page 2 line 49: The text states that the RII for education in cervical screening increased from 2005 to 2010, yet the RII in 2005 was 0.35 and in 2010 was 0.30 – is this not a decrease? Also, it is clear that decreases in the RIIs between 2005 and 2010 were non-significant, as shown in the Results Tables and from the final sentence in the Results section of the paper ("The p-trend for the temporal chance in the RIIs (adjusted model) measured by interaction term between 2005 and 2010, was found to be non-significant for all 3 screening programs for income and education level"). It seems incorrect then, to state in the Results of the Abstract that the RIIs decreased, as this implies statistical significance. Page 3 lines 3-8: The final sentence of the Conclusion is unwarranted, there is no evidence from this study that indicates that this is the case. Introduction: Page 5 line 22: "cervical cancer via mammography" – this does not read correctly, it seems like you are saying that mammography is a screening method to detect cervical cancer. In the second half of this sentence, it is difficult to tell what "respectively" refers to. This whole sentence is a bit confusing, I would recommend restructuring it so that it's clear which cancer type is screened for by which modality. Page 5 line 32 to page 6 line 6: This is much improved with more description of previous literature, but the directions of associations are not presented. For example, is income positively or negatively associated with screening uptake? And why might these associations exist? I think these are really complex associations that deserve more attention and explanation from the authors in setting up the rationale for their study. Page 6, final paragraph of Introduction: The reference [1] should be cited after the sentences that cite the results of that study Methods: The calculation of age-adjusted participation rates, as in Table 2, does not mean that the ORs in Table 3 do not need to be adjusted for age. These are two separate things. The ORs in Table 3 must be adjusted for age. Discussion: Lines 31-38: Why would the telephone modality of the Cancer Barometer Survey versus the telephone, face-to-face, and questionnaire modalities of Sicsic et al's study make a difference to reported screening uptake rates? In order to make this statement, the authors must provide a reference showing that survey modality affects reporting of screening uptake. I would not suspect that this is the case, as I have never seen evidence that survey modality has an effect on reporting of screening uptake rates. Further, the main reason why the results of this study and that by Sicsic et al might be the fact that this study compared rates of screening uptake from two different samples of people at two different points of time. The differences observed in screening rates between 2005 and 2010 in this study could in large part be an artefact due to comparing samples of different compositions. This is the biggest limitation of this study, although the authors do not emphasize this limitation enough. Page 14 lines 50-55: The RIIs for 2005 and 2010 were not

	significantly different, so it is not warranted to say that the RIIs decreased over time. At the very least, the authors must state that the deceases over time were not statistically significant. Page 15 lines 7-10: This sentence about manual workers being less aware about screening programs is unwarranted; there is no evidence from this study that this is the case, and the authors have not cited any literature to support this sentence. Limitations section: This paper is limited by the use of two different samples to compare changes in rates over time, as the results could be entirely due to differing sample composition at each time point. This limitation must be addressed.

REVIEWER	Christina Fitzmaurice University of Washington, USA
REVIEW RETURNED	14-Jun-2017

GENERAL COMMENTS	Thank you for addressing all of the reviewer comments. The only remaining comment I have is that the interpretation of the RII presented is still not quite clear ("An RII of 0.5 for example indicates a lower probability of cancer screening uptake at the lower extreme of income and education levels compared to the higher extremes or vice versa."). In the Mackenbach paper they use the following interpretation example "A large score on the RII implies large morbidity and mortality differences between high and low positions in the social hierarchy." You could use a similar phrase like "A large score on the RII implies large screening differences between high and low positions in the social hierarchy.".

VERSION 2 – AUTHOR RESPONSE

Reviewer: 2
Christina Fitzmaurice
University of Washington, USA
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Thank you for addressing all of the reviewer comments. The only remaining comment I have is that the interpretation of the RII presented is still not quite clear ("An RII of 0.5 for example indicates a lower probability of cancer screening uptake at the lower extreme of income and education levels compared to the higher extremes or vice versa."). In the Mackenbach paper they use the following interpretation example "A large score on the RII implies large morbidity and mortality differences between high and low positions in the social hierarchy." You could use a similar phrase like "A large score on the RII implies large screening differences between high and low positions in the social hierarchy."

OUR RESPONSE 1: We understand the point raised by the reviewer. In the Mackenbach paper, the socioeconomic indicators variables have been determined so that higher scores are consistent with increased risk of disease or health status. This is not the case in our study where higher scores indicate better participation in cancer screenings. To make it clear for readers, we modified the section to read: “An RII of 0.5 for example implies that participants in the most deprived group (those with lower incomes and educations levels) had a 50% lower probability of cancer screening uptake when compared to those in the least deprived group (those with higher incomes and education levels)”

Reviewer: 1
Lindsay Kobayashi
Harvard T.H Chan School of Public Health, USA
Please state any competing interests or state ‘None declared’: None declared

Please leave your comments for the authors below

Thank you for inviting me to review the revised version of this manuscript, which has improved since its original submission. My remaining comments are as follows:
General: I have still found some small grammatical errors and inconsistent tenses throughout the manuscript, and spelling errors in the tables (e.g. “Manuel” in Table 1 and “Pprofessional” in Table 2. I recommend further proofreading.

OUR RESPONSE 2: We thank the reviewer for her vigilance. We revised the manuscript accordingly.

Abstract:
Page 2 lines 7-17: The Objectives should state the specific types of cancer screening under study.

OUR RESPONSE 3: The objectives paragraph has been modified as suggested:

“Various socio-economic indicators have been identified as potential factors for disparities in breast, cervical and colorectal cancer screening uptake. Our study aimed to identify the socio-economic inequalities which persisted in screening uptake for these three cancer sites, and to quantify these disparities over a 5-year period.”

Page 2 line 18: The tense should be “was” not “is” since the survey occurred in the past

OUR RESPONSE 4: Thank you. Corrected to read as follows:

“The Cancer Barometer was a population-based survey”

Page 2 line 23: There is a problem with this sentence, “A randomly selected inhabitants” does not make sense and the “A” is incorrectly bolded. I also recommend writing “n=3820” and “n=3727” so it is clear that you are referring to the sample size

OUR RESPONSE 5: Thank you for identifying this mistake. Corrected to read as follows:

“A randomly selected sample of participants aged 15 to 85 years, n=3820 in 2005 and n=3727 in 2010,”

Page 2 line 35: I recommend stating the specific socio-economic variables under study so that the reader knows what you actually investigated

OUR RESPONSE 6: We thank the reviewer for this suggestion. We revised the section accordingly as follows:
"For each type of screening program, we calculated participation rates, odds ratios (OR) and relative inequality indices (RII) for participation, derived from logistic regression of the following socio-economic variables: income, education, occupation, employment and health insurance."

Page 2 lines 44-52: For readers who are unfamiliar with the RII measure, I would recommend providing some information on what a smaller versus larger RII means, or at very least some information to aid the reader in interpreting this measure (either here in the Results section or in the Primary and Secondary Outcome Measures section).

OUR RESPONSE 7: Many thanks for this suggestion. We provided a detailed explanation of RIIs and their interpretation is given in the main body of the article, as also requested by Reviewer 2. We modified the section to read "An RII of 0.5 for example implies that participants in the most deprived group (those with lower incomes and educations levels) had a 50% lower probability of cancer screening uptake when compared to those in the least deprived group (those with higher incomes and education levels)"

Page 2 line 49: The text states that the RII for education in cervical screening increased from 2005 to 2010, yet the RII in 2005 was 0.35 and in 2010 was 0.30 – is this not a decrease? Also, it is clear that decreases in the RIIs between 2005 and 2010 were non-significant, as shown in the Results Tables and from the final sentence in the Results section of the paper ("The p-trend for the temporal change in the RIIs (adjusted model) measured by interaction term between 2005 and 2010, was found to be non-significant for all 3 screening programs for income and education level"). It seems incorrect then, to state in the Results of the Abstract that the RIIs decreased, as this implies statistical significance.

OUR RESPONSE 8: We agree with the reviewer. After adjustment for age in logistic regression models as suggested by the reviewer (please see our response 13), the subsequent change in RIIs has resulted in a corresponding change to the Results section of the abstract, as well as in the Results section of the main body of the ms:
"RIIs for income remained significant for cervical smear in 2005 (RII=0.25, 0.13-0.48) and in 2010 (RII=0.31, 0.15-0.64). RIIs for education in mammography (RII=0.43, 95% CI 0.19-0.98) and cervical smear (RII=0.36, 95% CI 0.21-0.64) were significant in 2005 and remained significant for cervical smear (RII=0.40, 95% CI 0.22-0.74) in 2010."

Indeed, this avoids confusion over increase/decrease in RII values, by focusing on the significance of the RIIs.

Page 3 lines 3-8: The final sentence of the Conclusion is unwarranted; there is no evidence from this study that indicates that this is the case.

OUR RESPONSE 9: We fully agree this comment and removed this final sentence. We replaced it by the same sentence used for our conclusion in the Discussion section (please see page 17): "The findings suggest that organized cancer screening programs have the potential to reduce socio-economic disparities in participation"

Introduction:
Page 5 line 22: “cervical cancer via mammography” – this does not read correctly, it seems like you are saying that mammography is a screening method to detect cervical cancer. In the second half of this sentence, it is difficult to tell what “respectively” refers to. This whole sentence is a bit confusing, I would recommend restructuring it so that it’s clear which cancer type is screened for by which modality.

OUR RESPONSE 10: Thank you for identifying this editing mistake. So that each cancer type aligns with its screening method, this section has been revised as follows:
“…screening programs for breast, colorectal and cervical cancer via mammography, faecal occult blood test (FOBT) and cervical smear, respectively”

Page 5 line 32 to page 6 line 6: This is much improved with more description of previous literature, but the directions of associations are not presented. For example, is income positively or negatively associated with screening uptake? And why might these associations exist? I think these are really complex associations that deserve more attention and explanation from the authors in setting up the rationale for their study.

OUR RESPONSE 11: We agree with the reviewer. This section has been revised and the direction of the association clarified.

Page 6, final paragraph
of Introduction: The reference [1] should be cited after the sentences that cite the results of that study

OUR RESPONSE 12: Citation has been inserted as suggested after the final sentence of results.

Methods:
The calculation of age-adjusted participation rates, as in Table 2, does not mean that the ORs in Table 3 do not need to be adjusted for age. These are two separate things. The ORs in Table 3 must be adjusted for age.

OUR RESPONSE 13: We agree with the reviewer and have duly adjusted for age in regression models. Results in Tables 3-5 have been updated accordingly.

Discussion:
Lines 31-38: Why would the telephone modality of the Cancer Barometer Survey versus the telephone, face-to-face, and questionnaire modalities of Sicsic et al’s study make a difference to reported screening uptake rates? In order to make this statement, the authors must provide a reference showing that survey modality affects reporting of screening uptake. I would not suspect that this is the case, as I have never seen evidence that survey modality has an effect on reporting of screening uptake rates.

OUR RESPONSE 14: There is some evidence that social desirability bias is frequent in face-face or telephone interviews when compared to self-administered questionnaires, particularly for sensitive topics (Tourangeau and Yan 2007; Belli, Traugott, and Beckmann 2001; Jones and Forrest 1992; Presser and Stinson 1998). Nevertheless, we have removed this statement as a possible explanation from the text.

Further, the main reason why the results of this study and that by Sicsic et al might be the fact that this study compared rates of screening uptake from two different samples of people at two different points of time. The differences observed in screening rates between 2005 and 2010 in this study could in large part be an artefact due to comparing samples of different compositions. This is the biggest limitation of this study, although the authors do not emphasize this limitation enough.

OUR RESPONSE 15: We thank the reviewer for this raising this possibility. For the reviewer information, Sicsic et al study also used different subsamples as stated in their article "The sampling database was renewed in 2010; thus, the 2006, 2008 and 2010 ESPS samples comprised different subpopulations." The essence of National health Surveys such the "Health Survey for England", the "US National Health Interview Survey", the "Canada Health Survey", and the "French Cancer Barometer" is to be conducted at different points in time and with different samples of people (in contrast to cohort studies), with weighting strategies to ensure the representativeness of the samples.

Nevertheless, we accept that the differing sample distributions could be a limitation of our study and have included this point in the limitation section of the main body of the article as follows:

"Our study used two separate sample populations, whose distributions in Table 2 differed significantly for all of the socio-economic indicators and several covariates. The difference in sample distributions may have accounted for the observed differences in screening participation."

Page 14 lines 50-55: The RIIs for 2005 and 2010 were not significantly different, so it is not warranted to say that the RIIs decreased over time. At the very least, the authors must state that the deceases over time were not statistically significant.

OUR RESPONSE 16: Thank-you for drawing out attention to this inaccuracy. We have amended the discussion to take into account the non-significant change in the RIIs to read:

"The relative inequalities for income and education decreased for breast and colorectal cancer screening in our study, albeit non-significantly. This is somewhat consistent with DeMaio et al., which showed a reduction in the RII for breast cancer screening from 2005 to 2009 [19]. In the study by Kim et al.[9], the income-based relative inequalities tended to decrease slightly, while those for education remained constant over time. The relative inequalities for cervical cancer screening persisted for both income and education from 2005 to 2010 in our study, both remaining statistically significant. This is partially consistent with the De Maio et al.[19], where the social gradient decreased for income but increased for education between 2005 and 2010."

Page 15 lines 7-10: This sentence about manual workers being less aware about screening programs is unwarranted; there is no evidence from this study that this is the case, and the authors have not cited any literature to support this sentence.

OUR RESPONSE 17: Manual workers constitute the occupation class with the lowest level of education in the study (85% inferior to Baccalauréat). Lower education is known to be correlated with health literacy, which in turn has the potential to reduce awareness of and participation in colorectal cancer screening. The following references "Kobayashi et al. Prev Med. 2014;61:100-5; Dolan et al. J Clin Oncol 2004, 22(13):2617-2622; Ricardo-Rodrigues et al. Eur J Cancer Prev 2015, 24(4):305-312" have been added to read: "Thus they may have been less aware of the health marketing campaigns for colorectal cancer screening and the recommendation for FOBT, due to the negative effect of lower education on health literacy."

Limitations section: This paper is limited by the use of two different samples to compare changes in rates over time, as the results could be entirely due to differing sample composition at each time point. This limitation must be addressed.

OUR REPSONSE 18: We thank the reviewer for raising this issue. Please see our response 15 regarding the same comment. We have added the following paragraph to account for this limitation:

"Our study used two separate sample populations, whose distributions in Table 2 differed significantly for all of the socio-economic indicators and several covariates. The difference in sample distributions may have accounted for the observed differences in screening participation."

VERSION 3 – REVIEW

REVIEWER	Lindsay Kobayashi Harvard T.H. Chan School of Public Health, USA
REVIEW RETURNED	17-Aug-2017

GENERAL COMMENTS	Thank you for inviting me to re-review this manuscript, which has improved since the last revision. I have a few remaining essential revisions that must be addressed before this article can be accepted: General comment: The manuscript uses a mix of British and American English throughout, please proofread and revise. General comment: Please include a section on research ethics and participant consent. Introduction: Page 5: I appreciate that the authors have added a literature review to the Introduction, but I would recommend that it be more targeted in terms of focusing on the specific socioeconomic variables under study and other related variables. It currently is a bit scattered in focus, for example, the mention of a GP consultation as a predictor of colorectal cancer screening. Page 5: This sentence is incorrect: "This study conducted among 10 000 participants found that those classified as unskilled workers were less likely to have undergone cervical cancer screening (OR = 1.64 [1.38-1.95])." The OR is greater than 1.00, which indicates that unskilled workers are more likely to have undergone screening, not less likely to have undergone screening (than skilled workers? the reference group must also be stated). The following two sentences about breast cancer screening also make the same mistake. Please revise. Methods: Page 9 and Tables 3-5: In the Methods on page 9, the calculation of the p-values for trends in inequality is described as "The p-value for significance of the evolution of the odds ratios between 2005 and 2010 was calculated using an interaction term between the socio-economic variable and the time period, in order to be consistent with

	the methodology of previous studies on the topic [22, 23]", while the same p-values presented in Tables 3-5 are described as "Calculated by the interaction term for the change in Adjusted OR and Adjusted RII between 2005 and 2010". Which is correct? And, regardless of which one is correct, it is still difficult to understand how this p-value was calculated, as presumably there are separate models for 2005 and 2010, in which case how could changes in effect estimates over time be entered into the model as interacted independent variables? Discussion: Page 12: There is a period missing at the end of this paragraph. Page 13: It is concerning that this study and Sicsic found such disparate results with respect to increases or decreases in screening uptake rates over time. The explanation given by the authors for these differences is not sufficient. Having differing time intervals (which were similar, as both captured 2010 and the Sicsic study began in 2006) or different study objectives should not result in finding different trends in participation rates. There must be differences in sample selection between the two studies, which may explain the results. Are there any other data sources that could be used to verify whether screening rates in France have actually decreased or increased over time? Page 16: The differences in the distributions of socioeconomic variables between 2005 and 2010 is highly concerning particularly given the study methods using the RII, as the value of the RII is dependent on the distribution of the socioeconomic variable in the study population. The authors do not make enough of this limitation – how confident can we be that reductions in inequality over time are not simply due to changes in socioeconomic distributions rather than an actual reduction in inequality? Page 16: This conclusion is too strongly worded, I recommend including the word "may": E.g. "…organized cancer screening programs may have the potential…" All tables: Please proof-read, I have found further typos (e.g. "standardization" in Table 1 and "standardisation" in Table 2, and inconsistent use of capitalized letters) Table 1: The title of the table should indicate what was standardized in these estimates. Table 2: Please double check the overall participation rate for mammography in 2005 (88.0%): the participation rates within all of the socioeconomic subgroups is less than 88.0%, so how could the overall participation rate be 88.0%? Also, the overall rates of 88.0% in 2005 and 2010 in 2010 do not support an increase in overall mammography uptake, as concluded by the authors.

VERSION 3 – AUTHOR RESPONSE

Reviewer comments:
Thank you for inviting me to re-review this manuscript, which has improved since the last revision. I have a few remaining essential revisions that must be addressed before this article can be accepted.

OUR RESPONSE 1: We thank the reviewer for acknowledging that our manuscript has improved since the last revision.

General comment: The manuscript uses a mix of British and American English throughout, please proofread and revise.

OUR REPONSE 2: The manuscript has been revised to use British English throughout

General comment: Please include a section on research ethics and participant consent.

OUR REPONSE 3: We thank the reviewer for raising this issue. We added now a section on research ethics and participant consent as follows: “The study protocol included a formal request to participate, explaining the objectives of the study that was delivered by mail before the first telephone call. Informed consent was obtained at the start of the telephone interview, in accordance with the guidelines of the French Data Protection Authority (CNIL).” (Please see page 8, study population section, first paragraph)

Introduction:
Page 5: I appreciate that the authors have added a literature review to the Introduction, but I would recommend that it be more targeted in terms of focusing on the specific socioeconomic variables under study and other related variables. It currently is a bit scattered in focus, for example, the mention of a GP consultation as a predictor of colorectal cancer screening.

OUR REPONSE: We thank the reviewer for her suggestion. The introduction is now focused on the specific socioeconomic variables under study. (Please see page 5, Introduction section, and last paragraph).

Page 5: This sentence is incorrect: “This study conducted among 10 000 participants found that those classified as unskilled workers were less likely to have undergone cervical cancer screening (OR = 1.64 [1.38-1.95]).” The OR is greater than 1.00, which indicates that unskilled workers are more likely to have undergone screening, not less likely to have undergone screening (than skilled workers? the reference group must also be stated). The following two sentences about breast cancer screening also make the same mistake. Please revise.

OUR RESPONSE: We thank the reviewer for giving us the opportunity to clarify this section for readers. In fact, the interpretation is correct but we agree that it might be confusing. The study by Sicsic and Franc aimed to study the failure to undertake cancer screening. The reference group was intermediate profession (OR=1). Therefore, unskilled workers were more likely to have NOT undergone cancer screening (OR=1.64) i.e. less likely to have undergone screening. Nevertheless, the sentence has been amended to include this clarification and the direction of the OR as requested:
“This study conducted among 10 000 participants found that those classified as unskilled workers were more likely to have not undergone cervical cancer screening (OR = 1.64 [1.38-1.95]) when compared to those with an intermediate profession.”
Same correction for breast cancer screening, where the reference group is “having a private health insurance”:
“The results also showed that women without (OR = 2.05 [1.68-2.51]) or receiving free complementary health insurance (OR = 1.79 [1.36-2.37]) were more likely to have not undergone breast cancer screening when compared to those with a private complementary health insurance.”

Methods:
Page 9 and Tables 3-5: In the Methods on page 9, the calculation of the p-values for trends in inequality is described as “The p-value for significance of the evolution of the odds ratios between 2005 and 2010 was calculated using an interaction term between the socio-economic variable and the time period, in order to be consistent with the methodology of previous studies on the topic [22, 23]”, while the same p-values presented in Tables 3-5 are described as “Calculated by the interaction term for the change in Adjusted OR and Adjusted RII between 2005 and 2010”. Which is correct? And, regardless of which one is correct, it is still difficult to understand how this p-value was calculated, as presumably there are separate models for 2005 and 2010, in which case how could changes in effect estimates over time be entered into the model as interacted independent variables?

OUR REPONSE: We understand the point raised by the reviewer. In fact, we failed to indicate in the methods section that we created a pooled dataset of the two surveys. Thus, the trend of relative index of inequalities or inequalities across socioeconomic indicators strata for each survey was then estimated and compared using a two-way interaction term composed of each socioeconomic variable and year of the survey. This is now clarified in the revised version (please pages 10 and 11 and in tables 3-5).

Discussion:
Page 12: There is a period missing at the end of this paragraph.

OUR REPONSE: We double checked the first paragraph of the discussion section and found no missing word.

Page 13: It is concerning that this study and Sicsic found such disparate results with respect to increases or decreases in screening uptake rates over time. The explanation given by the authors for these differences is not sufficient. Having differing time intervals (which were similar, as both captured 2010 and the Sicsic study began in 2006) or different study objectives should not result in finding different trends in participation rates. There must be differences in sample selection between the two studies, which may explain the results. Are there any other data sources that could be used to verify whether screening rates in France have actually decreased or increased over time?

OUR REPONSE: To be honest, we were disappointed when we were asked for a third revision round. Finally, we were happy with that since it allows us to correct some mistakes that went unnoticed in previous rounds. We want therefore to thank the reviewer for her vigilance. Based on her last comment, we double checked overall participation rates to screening programs. We found that the rate for mammography in 2005 was wrongly reported. It is 72.1% rather 88.0% has reported previously. Please note that the only point of comparison between our study and that by Sicsic and Franc concerns overall participation rates in screening programs. Indeed, despite an increase in mammography from 2005 to 2010, it was not statistically significant at the level of 5%, which partly reconcile our findings with those of Sicic and Franc. Indeed, Sicsic and Franc found that “the screening rate for breast cancer decreased between 2006 and 2010, from 77.6% in 2006 to 74.0% in 2010”, but the difference was not statistically significant”. They also found that “colorectal cancer screening uptake increased significantly between 2006 and 2010, from 18.2% in 2006 to 38.9% in 2010”. This is consistent with our result showing that colorectal cancer screening uptake significantly increased from 34.0% in 2005 to 51% in 2010. Finally, they found that “the screening rate for cervical cancer significantly decreased from 75.3% in 2006 to 71.9% in 2010”. For cervical cancer, we found that the rate was stable between 2006 (79.7) and 2010 (81.4). In definitive, differences in sampling, sample sizes, number of data collection phases, and in desirability bias may explain differences in participation rates. We therefore rewrote the section “Findings in the context of the literature” accordingly. (Please discussion section, page 13).

Page 16: The differences in the distributions of socioeconomic variables between 2005 and 2010 is highly concerning particularly given the study methods using the RII, as the value of the RII is dependent on the distribution of the socioeconomic variable in the study population. The authors do not make enough of this limitation – how confident can we be that reductions in inequality over time are not simply due to changes in socioeconomic distributions rather than an actual reduction in inequality?

OUR RESPONSE: We agree. The section about this limitation reads now as follows: “Our study used two separate sample populations, whose distributions in Table 2 differed significantly for all of the socio-economic indicators and several covariates. The difference in sample distributions may have accounted for the observed differences in screening participation rates. Thus, we cannot rule out that reductions observed in inequalities over time are not simply due to changes in socioeconomic distributions rather than an actual reduction in social inequalities in screening participation”

Page 16: This conclusion is too strongly worded, I recommend including the word “may”: E.g. “…organized cancer screening programs may have the potential…”

OUR REPONSE: Revised as suggested.

All tables: Please proof-read, I have found further typos (e.g. “standardization” in Table 1 and “standardisation” in Table 2, and inconsistent use of capitalized letters)

OUR RESPONSE: Revised as suggested.

Table 1: The title of the table should indicate what was standardized in these estimates.

OUR RESPONSE: A footnote describing the data weighting strategy has been added.

Table 2: Please double check the overall participation rate for mammography in 2005 (88.0%): the participation rates within all of the socioeconomic subgroups is less than 88.0%, so how could the overall participation rate be 88.0%? Also, the overall rates of 88.0% in 2005 and 2010 in 2010 do not support an increase in overall mammography uptake, as concluded by the authors.

OUR RESPONSE: Thank you for identifying this error. The correct value of 71.2% is now displayed in Table 2.

VERSION 4 – REVIEW

REVIEWER	Lindsay Kobayashi Harvard T. H. Chan School of Public Health, USA
REVIEW RETURNED	31-Oct-2017

GENERAL COMMENTS	Thank you for inviting me to re-review this manuscript, which has improved since the last revision. There are a few minor revisions that should be made before this article could be accepted for publication: General: I have still found several typos and grammatical mistakes, I recommend having an external person with native English language skills proofread the article. General: There are several places where confidence intervals are presented, but the significance level of the confidence intervals is not given (e.g. in the Introduction, where ORs for reference #6 are shown, and in the final paragraph of the Results section). Abstract, conclusion section: Should read “statistically significant”, not “statistically significance” Page 8, “Measures” section: There is an unclosed bracket at the end of the sentence describing the educational attainment levels. Page 9, paragraph 2: The authors state there was a dummy variable entered for year of survey as “2010 vs. 2006”, but the surveys were actually conducted in 2010 and 2005, not 2006, correct? Page 10, first sentence: Same error, the survey is incorrectly stated as occurring in 2006, rather than 2005. Page 13, second paragraph: There is an unclosed quotation mark after the number 2010. Page 17, final paragraph: Small sample sizes can lead to low precision of estimates, not “false observations”

VERSION 4 – AUTHOR RESPONSE

Reviewer comments:

Thank you for inviting me to re-review this manuscript, which has improved since the last revision. There are a few minor revisions that should be made before this article could be accepted for publication:

OUR RESPONSE 1: We thank the reviewer for helping us to improve the quality of our manuscript.

General: I have still found several typos and grammatical mistakes; I recommend having an external person with native English language skills proofread the article.

OUR RESPONSE 2: As suggested the manuscript has been proofread by an external personal with native English language skills.

General: There are several places where confidence intervals are presented, but the significance level of the confidence intervals is not given (e.g. in the Introduction, where ORs for reference #6 are shown, and in the final paragraph of the Results section).

OUR RESPONSE 3: We thank the reviewer for this suggestion. We revised the manuscript as suggested

Abstract, conclusion section: Should read “statistically significant”, not “statistically significance”

OUR RESPONSE 4: This spelling mistake has been corrected to read “statistically significant”.

Page 8, “Measures” section: There is an unclosed bracket at the end of the sentence describing the educational attainment levels.

OUR RESPONSE 5: We thank the reviewer for her vigilance. It has been corrected.

Page 9, paragraph 2: The authors state there was a dummy variable entered for year of survey as “2010 vs. 2006”, but the surveys were actually conducted in 2010 and 2005, not 2006, correct?

OUR RESPONSE 6: We thank the reviewer for detecting this mistake; 2005 is indeed the correct survey year. We corrected this typo.

Page 10, first sentence: Same error, the survey is incorrectly stated as occurring in 2006, rather than 2005.

OUR RESPONSE 7: It has been corrected. Thank you

Page 13, second paragraph: There is an unclosed quotation mark after the number 2010.

OUR RESPONSE 8: Thank you. The quotation mark has been removed.

Page 17, final paragraph: Small sample sizes can lead to low precision of estimates, not “false observations”

OUR RESPONSE 9: We thank the reviewer for this suggestion. The sentence has been amended to read: “…leading to low precision of estimates.”