PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Economic crisis and smoking behaviour: Prospective cohort study in Iceland
AUTHORS	McClure, Christopher ; Valdimarsdttir, Unnur; Hauksdóttir, Arna; Kawachi, Ichiro

VERSION 1 - REVIEW

REVIEWER	Silvano Gallus Department of Epidemiology Istituto di Ricerche Farmacologiche Mario Negri The reviewer declares no conflicts of interest
REVIEW RETURNED	15-May-2012

THE STUDY	The final message is not clear from the Abstract: Is the pro-cyclical or the counter-cyclical direction (or both) observed in the present study? Most of the ORs reported in the Abstract come from analyses on relapse stratified by sex, and are therefore based on an extremely low number of subjects. In many cross-tabulations cells have counts that are less than 5, which makes the applicability of large sample theory questionable. Therefore the usual asymptotic methods for analyzing these data are unreliable, and exact tests or exact logistic regression models are needed to derive statistical significance or to appropriate confidence intervals, respectively. Moreover, the use of several covariates for these data is not recommended. At least one relevant paper (Gallus et al. 2011), specifically focusing on the aim of the present study, is not considered in the present study. See Comments to Authors for additional details
GENERAL COMMENTS	The objective of the present before/after study is to describe the effects of the economic crisis on smoking behaviour in Iceland. The aim is extremely interesting, particularly since scanty data are available on the issue. Moreover, this is the first study analysing the issue using a proper study design (prospective cohort study). The main limitation is given by the relatively low sample size of the cohort (see below). The following major point should be addressed by authors: • The main limitation of the present study is given by the relatively low sample size. Major findings are based on 56 ex-smokers who relapsed and 160 smokers who quit, only. Moreover, most of the ORs reported in the Abstract come from analyses on relapse stratified by sex, and are therefore based on an extremely low number of subjects. In many cross-tabulations cells have counts that

	are less than 5, which makes the applicability of large sample theory questionable. Therefore the usual asymptotic methods for analyzing these data are unreliable, and exact tests or exact logistic regression models are needed to derive statistical significance or to appropriate confidence intervals, respectively. Moreover, the use of several covariates for these data is not recommended. Therefore, I strongly suggest to substantially simplify tables and analyses, by i) using one single model (age and sex adjusted, or selecting a maximum of 2 to 3 covariates), ii) avoiding the presentation of analyses stratified by sex, iii) substantially collapsing exposure categories (e.g., authors should consider a maximum of 3 categories for age), iv) avoiding further stratification of household income (among high/low income at baseline), v) showing only the most informative results. • The paper by Gallus et al. (Smoking in Italy 2008-2009: a rise in prevalence related to the economic crisis? Prev Med. 2011;52:182-3), specifically focusing on the aim of the present study, should be carefully considered by authors both in the Introduction and Discussion sections. • The final message is not clear: on the one hand, the decreased smoking prevalence (which may partly or totally explained by secular trends) and the low relapse rate of male former smokers with a reduction in income are compatible with a pro-cyclical mechanism of smoking (i.e., economic crisis results in a decrease of smoking prevalence); on the other hand, stress is associated to increased relapse and economic crisis increases psychological stress, thus suggesting that smoking is counter-cyclical. Is the pro-cyclical or the counter-cyclical direction (or both) observed in the present study? According to that line of reasoning, the two sentences of the Conclusions in the Abstract appear to be in contrast. • The measure “change in economic status” is questionable. Couldn’t authors consider a measure which i) takes into account inflation, and ii) is not based on one single cut-off? For example, authors could consider a drop in income once the inflation-adjusted income decreased and a rise in income once the inflation adjusted income increased. • Authors should carefully re-read the entire manuscript for the presence of a few typos. Also in the Abstract “smoking secession” should be replaced by “smoking cessation”. Moreover, please, replace each occurrence of “ratio of odds” (???) with “odds ratio”. • Authors should make clear what is reported in various tables (e.g., in table 1 it is not stated that numbers and percentages are reported). More importantly, all the tables should be double-checked because of problems in reporting numbers and percentages. How is that in Table 1, for example, there are 45 married/cohabiting and 7 single/divorced ex smokers who relapsed, and in Table 2 the corresponding numbers are 44 and 8, respectively? Other example: how is that the sum of percentages of ex-smokers who relapsed does not add up to 100% (in case it is for the presence of 2 missing values, the percentages should be re-calculated excluding these, and a footnote should explain that there are 2 missing values)? Moreover, it is strange to me that in Table 4 differences according to calendar year of means for relapsed are not significant for men and significant for women. Please, double check!

REVIEWER	Benjamin A. Shaw Associate Professor and Chair Department of Health Policy, Management, and Behavior School of Public Health University at Albany USA
REVIEW RETURNED	22-May-2012

THE STUDY	The study design is appropriate, but the sample size is troubling. Type II error is probable, and I question the representativeness of the sample, as the overall response rate is really only 38%
RESULTS & CONCLUSIONS	The study design is appropriate, but the sample size is troubling. Type II error is probable, and I question the representativeness of the sample, as the overall response rate is really only 38%
GENERAL COMMENTS	Overall, this is a good manuscript that addresses an important topic competently. My major concerns are as follows: 1) Further development is needed with regard to justifying the main research questions. As mentioned above, prior research on individual level economic circumstances and smoking behavior needs to be acknowledged more thoroughly. Also, the potential role of stress in the association between income and smoking is not discussed adequately in the Introduction. I was not even aware that this study included research questions about stress until the Methods section. 2) The sample size numbers presented in the Methods section do not match what is reported in Figure 1. Also, 4092 is the number of respondents to the 2009 survey, but in the text is refers to the 2007 survey for this sample. 3) As mentioned above, more reflection is needed with regard to the representativeness of this sample, as the overall response rate is only 38% (3755/9807). 4) The operationalization of income gains and losses is quite crude. How is real change distinguished from random variation between waves? 5) It is not clear why baseline income and baseline stress are not included in the hazard models. 6) Why is the analysis of relapse conducted in steps, with separate models with and without stress, while the analysis of smoking cessation is not conducted with these same steps? 7) In Table 2, the estimates associated with perceived stress appear in the first 3 columns, but according to the footnotes for this table, these estimates should appear in the last three columns of this table

VERSION 1 – AUTHOR RESPONSE

Reviewer: Silvano Gallus
Department of Epidemiology
Istituto di Ricerche Farmacologiche Mario Negri

Comment 1: The final message is not clear from the Abstract: Is the pro-cyclical or the counter-cyclical direction (or both) observed in the present study?
Response 1: Our main message is one of the procyclicality of smoking behavior. As the decrease in smoking rates might be entirely the consequence of secular trends, we do not contend our main message of procyclicality with support from a reduction in overall smoker prevalence, but from the following elaborations: first, we find that those individuals who experienced a reduction in income were less likely to relapse compared to those who maintained stable income; secondly, we find that those who experienced an increase in income were significantly more likely to relapse compared to those with stable income; and thirdly, we find that the foregoing two findings hold true even when adjusting for change in stress levels. As we mention in our Discussion section (paragraph 6), our finding regarding stress needs to be interpreted with caution since some degree of social desirability bias is likely to be involved when smokers cite an increase in stress as a reason for relapse (or failure to quit).
According to the reviewer's suggestions, changes to the manuscript have been to the Abstract and the Conclusions in order to clarify our final message.

Comment 2: The main limitation of the present study is given by the relatively low sample size. Major findings are based on 56 ex-smokers who relapsed and 160 smokers who quit, only. Moreover, most of the ORs reported in the Abstract come from analyses on relapse stratified by sex, and are therefore based on an extremely low number of subjects. In many cross-tabulations cells have counts that are less than 5, which makes the applicability of large sample theory questionable. Therefore the usual asymptotic methods for analyzing these data are unreliable, and exact tests or exact logistic regression models are needed to derive statistical significance or to appropriate confidence intervals, respectively. Moreover, the use of several covariates for these data is not recommended.
Therefore, I strongly suggest to substantially simplify tables and analyses, by i) using one single model (age and sex adjusted, or selecting a maximum of 2 to 3 covariates), ii) avoiding the presentation of analyses stratified by sex, iii) substantially collapsing exposure categories (e.g., authors should consider a maximum of 3 categories for age), iv) avoiding further stratification of household income (among high/low income at baseline), v) showing only the most informative results.
Response 2: We thank the reviewer for the insightful comments on how we might be able to simplify our analyses, as well as the presentation of our results. Accordingly, we have addressed each point made below.
Regarding point i): we have simplified the model covariates to only age and sex per the reviewer's suggestions; additionally, the covariate of adults in household has been removed from household income (tables 2, 3) models.
Regarding point ii): as our main findings are predominantly sex-specific, we have to disagree with the suggestion to remove the presentation of sex-stratified results. Additionally, smoking outcomes have been reported to be quite gender-specific; the reviewer suggested an article presenting important sex-specific results. Nonetheless, we have made additional efforts to simplify the results (see below).
Regarding point iii): we concur with the reviewer's remarks on the collapsing of exposure categories. Thus, the following changes have been made to the analysis. Age categories have been collapsed to only three categories: (1) 18-39, (2) 40-59, and (3) ≥ 60 years of age. Marital status has been collapsed to only two categories: (1) single and (2) married/committed. As the classifications of employment used in the analysis and the results operate through different mechanisms relating to health outcomes, we have not collapsed this variable further. However, we have removed the secondarily informative results from the tables (e.g. student, homemaker).
Regarding point iv): we would very much like to retain the analyses stratified by household income. We believe that analyzing the odds of relapse and cessation according to – (1) income level post-collapse and (2) income level change from pre- to post-collapse – is quite important in the understanding of our main findings, as this contrast shows that it is not just income status playing a pivotal role in smoking behavior, but mainly the change in income following the economic collapse in

Iceland. Regarding point v): we agree with this reviewer's point. Thus, we have removed the following strata from Tables 2, 3: Age, Marital Status, Employment status in 2009, and Education; though not from the text.
Furthermore, we have removed the right three columns from Tables 2, 3, which pertain to additional adjustments for changes in stress levels. However these findings are included in the Results section.

Comment 3: At least one relevant paper (Gallus et al. 2011), specifically focusing on the aim of the present study, is not considered in the present study.
Response 3: We appreciate the author drawing more attention to this relevant paper. We have added the citation in the Introduction and the Discussion sections (as follows; see Track Changes in text).

Comment 4: The objective of the present before/after study is to describe the effects of the economic crisis on smoking behaviour in Iceland. The aim is extremely interesting, particularly since scanty data are available on the issue. Moreover, this is the first study analysing the issue using a proper study design (prospective cohort study). The main limitation is given by the relatively low sample size of the cohort (see below).
Response 4: We greatly appreciate the kind words about our study. Regarding our study's central limitation of low sample size and the potential generalizability caused by such a limitation, we have added the following remarks to the text. When comparing our cohort to national data in Iceland, we see comparable trends in smoking rates as follows: National smoking rates – 2007: 23%; 2009: 19%. Cohort smoking rates – 2007: 19%; 2009: 16%. We have discussed this in the Discussion chapter (paragraph 1). Secondly, we have also discussed the uncontrollable issues associated with the small proportion of the population that smokes and, even further, relapses or quits smoking. This was discussed in more depth in the Study Limitations subsection of the Discussions section. In analyses conducted by the Public Health Institute of Iceland (the cohort proprietors), responders from 2007 were compared to non-responders in 2007 (footnote 1). Their findings indicate a similar socio-economic background of 2007 responders to the total Icelandic population. However, as the 2009 responders have not yet been compared to the general population, it is possible that there was systematic attrition of respondents from 2007 to 2009 – though additional forthcoming papers utilizing this cohort show a similar demographic background between responders in 2007 and 2009.

Comment 5: The paper by Gallus et al. (Smoking in Italy 2008-2009: a rise in prevalence related to the economic crisis? Prev Med. 2011;52:182-3), specifically focusing on the aim of the present study, should be carefully considered by authors both in the Introduction and Discussion sections.
Response 5: Previously addressed above.

Comment 6: The final message is not clear: on the one hand, the decreased smoking prevalence (which may partly or totally explained by secular trends) and the low relapse rate of male former smokers with a reduction in income are compatible with a pro-cyclical mechanism of smoking (i.e., economic crisis results in a decrease of smoking prevalence); on the other hand, stress is associated to increased relapse and economic crisis increases psychological stress, thus suggesting that smoking is counter-cyclical. Is the pro-cyclical or the counter-cyclical direction (or both) observed in the present study? According to that line of reasoning, the two sentences of the Conclusions in the Abstract appear to be in contrast.
Response 6: As previously addressed in an aforementioned response (Response 1), we believe our main conclusions are consistent with the procyclical nature of smoking.

Comment 7: The measure "change in economic status" is questionable. Couldn't authors consider a measure which i) takes into account inflation, and ii) is not based on one single cut-off? For example, authors could consider a drop in income once the inflation-adjusted income decreased and a rise in income once the inflation adjusted income increased.

Response 7: We appreciate the reviewer's critique of and insight on this point. We did initially attempt to adjust income and income change by inflation, but due to the way in which the data was collected, it was not feasible. The income variables exist as categorical responses, with ranges of incomes – for example, ISK 75–141,000. In order to calculate inflation-adjusted income, we took the midpoints of each income range and applied the CPI ratio (2007 vs 2009) to that midpoint for each case. The results of this adjustment on these income ranges made it impossible to examine income changes, because the adjusted midpoint value for each case did not change sufficiently to move individuals across different income categories during the two-year follow-up window.

Comment 8: Authors should carefully re-read the entire manuscript for the presence of a few typos. Also in the Abstract "smoking secession" should be replaced by "smoking cessation". Moreover, please, replace each occurrence of "ratio of odds" (???) with "odds ratio".
Response 8: This has been done; thank you for bringing this to our attention. See Tracked Changes.

Comment 9: Authors should make clear what is reported in various tables (e.g., in table 1 it is not stated that numbers and percentages are reported).
More importantly, all the tables should be double-checked because of problems in reporting numbers and percentages. How is that in Table 1, for example, there are 45 married/cohabiting and 7 single/divorced ex smokers who relapsed, and in Table 2 the corresponding numbers are 44 and 8, respectively? Other example: how is that the sum of percentages of ex-smokers who relapsed does not add up to 100% (in case it is for the presence of 2 missing values, the percentages should be re-calculated excluding these, and a footnote should explain that there are 2 missing values)? Moreover, it is strange to me that in Table 4 differences according to calendar year of means for relapsed are not significant for men and significant for women. Please, double check!
Response 9: Appropriate changes to the tables have been made according to the reviewer's concerns and suggestions. Please see Tracked Changes for edits. Regarding the concern about inconsistent numbers of married/cohabiting and single/divorced individuals in tables, the explanation is as follows: Table 1 represents an individual's status in 2007; Tables 2/3 represent an individual's status in 2009. We have checked that the numbers are accurate – an additional heading has been made to Tables 2/3 to clarify this issue. Regarding the concern about stress levels for men not being significant: we double checked the p-values and all are accurate.

Reviewer: Benjamin A. Shaw
Associate Professor and Chair
Department of Health Policy, Management, and Behavior
School of Public Health
University at Albany
USA

Comment 1: The study design is appropriate, but the sample size is troubling. Type II error is probable, and I question the representativeness of the sample, as the overall response rate is really only 38%
Response 1: We have addressed the issue of sample size and low response in a previous response.

Comment 2: Both the Introduction and Discussion could benefit from a more thorough appreciation for

previous research on the individual-level association between economic well-being and smoking (e.g., Shaw, Agahi, and Krause, 2011, and the work of M. Siahpush).
Response 2: We appreciate the reviewer's suggestions regarding additional citations to the literature. See Track Changes for inclusion.

Comment 3: Overall, this is a good manuscript that addresses an important topic competently. My major concerns are as follows:

1) Further development is needed with regard to justifying the main research questions. As mentioned above, prior research on individual level economic circumstances and smoking behavior needs to be acknowledged more thoroughly. Also, the potential role of stress in the association between income and smoking is not discussed adequately in the Introduction. I was not even aware that this study included research questions about stress until the Methods section.
Response 3: Changes have been made to the Introduction to improve upon the inadequacies of our discussion on the main research questions, e.g. income, stress, and smoking. See Tracked Changes for edits/additions.

Comment 4: 2) The sample size numbers presented in the Methods section do not match what is reported in Figure 1. Also, 4092 is the number of respondents to the 2009 survey, but in the text is refers to the 2007 survey for this sample.
Response 4: Clarification to the text (Methods) and Figure 1 have been accordingly made. See Tracked Changes.

Comment 5: 3) As mentioned above, more reflection is needed with regard to the representativeness of this sample, as the overall response rate is only 38% (3755/9807).
Response 5: Please see previous responses to the first reviewer's comments above.

Comment 6: 4) The operationalization of income gains and losses is quite crude. How is real change distinguished from random variation between waves?
Response 6: We agree about the limitation of our income change categorization – please see Response #7 to reviewer #1. Unfortunately, we cannot look at more refined categories due to the way in which data were collected. We have acknowledged this limitation in the Discussion.

Comment 7: 5) It is not clear why baseline income and baseline stress are not included in the hazard models.
Response 7: We have made changes to the models; baseline income and stress are now included.

Comment 8: 6) Why is the analysis of relapse conducted in steps, with separate models with and without stress, while the analysis of smoking cessation is not conducted with these same steps?
Response 8: Additional adjustments for stress in the analysis of smoking cessation are now included in the Results.

Comment 9: 7) In Table 2, the estimates associated with perceived stress appear in the first 3 columns, but according to the footnotes for this table, these estimates should appear in the last three columns of this table.
Response 9: The presentation of the additional adjustments for stress changes (the last three columns) have been removed from Tables 2/3, but are now mentioned in the Results section.

Footnote 1: The Directorate of Health (Iceland). Division of Health Determinants. http://www2.lydheilsustod.is/media/lydheilsa/heilsufarskonnun/Meginmal.pdf. Retrived on June 6th, 2012. [in Icelandic]

VERSION 2 – REVIEW

REVIEWER	Silvano Gallus, Sc.D. Department of Epidemiology Istituto di Ricerche Farmacologiche Mario Negri 20156 Milan, Italy Conflict of interest: none
REVIEW RETURNED	19-Jul-2012

RESULTS & CONCLUSIONS	Although the presentation of findings improved after the first revision, in my opinion the limited sample size (but not only the sample size, see additional comments to authors) does not allow to derive reliable estimates and consequently firm conclusions. I believe however that given the limited information available on the issue, a revised version of the present manuscript may be accepted for publication on BMJ Open.
GENERAL COMMENTS	The presentation of findings improved after the first revision. This notwithstanding, the limited sample size does not allow to derive reliable estimates and consequently firm conclusions. Major points: 1) The main conclusion of the present study derives from estimates (OR=4.02; OR=0.37, shown in the Abstract), based on sub-samples of 31 male former smokers who relapsed and 82 male former smokers who quit, only. If we also consider the debatable assumptions authors had to consider in measuring changes in income, it is clear that a conclusive evidence is not warranted. From the Abstract, a reader has no possibility to understand this. I strongly suggest to add a sentence to the conclusion section of the Abstract clarifying that the main findings are based on a relatively low number of subjects, thus the interpretation of these findings has to be taken with caution. 2) I accept the response of authors to my previous point recommending to remove sex-specific results. But at this point, in Table 2 and Table 3, numbers should be given not only overall, but also by sex. 3) There are at least 2 inconsistencies that should be clarified: 3A) In table 1 those (men and women combined) who relapse among low household income at baseline are 8 and those who relapse among high household income at baseline are 14. How is that in Table 2 they become 30 and 44, respectively? This inconsistency is evident also for those who quit smoking (22 and 31 in Table 1 97 and 128, respectively in Table 3) 3B) Subjects who relapsed may be categorized by household income at baseline. In Table 2 there are 23+7=30 ex-smokers with low income at baseline who relapsed in 2009 and 28+16=44 ex-smokers with high income at baseline who relapsed in 2009. How is that the sum adds to 74, thus exceeding the total number of smokers who relapsed (56)? This inconsistency is evident also for those who quit smoking (Table 1 and Table 3).

REVIEWER	Benjamin A. Shaw Associate Professor and Chair Department of Health Policy, Management, and Behavior University at Albany

	USA
REVIEW RETURNED	28-Jul-2012

GENERAL COMMENTS	This revised manuscript is improved in many respects, but some significant concerns remain. 1) I am still not certain of this manuscript's major contribution. In the Article Summary, a major focus is said to be the role of stress. However, in the Conclusion of the Abstract, and in the Results section of both the Abstract and the main body of the manuscript, the role of stress is either not discussed at all, or mentioned only briefly. Stress is only briefly mentioned in the Introduction as well. So, it seems that the authors are not clear on to what degree they want to focus on stress. If a focus on stress is intended, then further elaboration justifying this focus is needed in the Introduction, and the Results section should also include a more well-developed presentation of the results that involve stress. 2) Moreover, how the authors wish to combine and integrate findings of the impact of macroeconomic changes from 2007 to 2009 with the impact of individual level income changes is not clear. The authors mention that most prior research has been at the ecological level. By contrast, their focus is on the individual level. This, by itself, is a contribution. But it is not clear if similar findings would emerge at a different period of time, or whether the individual-level effects that were found are unique to a period of economic crisis at the ecological level. 3) When measuring changes in stress from 2007 to 2009, how can small changes be distinguished from measurement error? 4) In the Results section, associations between employment status and smoking are presented. But, employment status is not presented as a measure in the Method section. Moreover, in the Article Summary, I was lead to believe that employment was not going to be addressed in this study (see the Strengths and limitations section). 5) Also in the Results section, the authors report that females were less likely than males to quit smoking. However, they only present the predictors of quitting for females, not males. 6) Additionally, I am confused by the last paragraph in the results section. How is it that stress change did not predict relapse, but that among females that relapsed, there was a significant increase in stress scores?

VERSION 2 – AUTHOR RESPONSE

Reviewer: Silvano Gallus, Sc.D.
Department of Epidemiology
Istituto di Ricerche Farmacologiche Mario Negri
20156 Milan, Italy

Comment 1: The main conclusion of the present study derives from estimates (OR=4.02; OR=0.37,

shown in the Abstract), based on sub-samples of 31 male former smokers who relapsed and 82 male former smokers who quit, only. If we also consider the debatable assumptions authors had to consider in measuring changes in income, it is clear that a conclusive evidence is not warranted. From the Abstract, a reader has no possibility to understand this. I strongly suggest to add a sentence to the conclusion section of the Abstract clarifying that the main findings are based on a relatively low number of subjects, thus the interpretation of these findings has to be taken with caution.
Response 1: We agree with the reviewer and have added a statement to both the Abstract (Conclusions), the Article Summary box (Strengths and limitations of this study), while elaborating in the Discussion section (Study limitations).

Comment 2: I accept the response of authors to my previous point recommending to remove sex-specific results. But at this point, in Table 2 and Table 3, numbers should be given not only overall, but also by sex.
Response 2: We agree with the reviewer's suggestions and present both the overall & sex-specific results.

Comment 3A: In table 1 those (men and women combined) who relapse among low household income at baseline are 8 and those who relapse among high household income at baseline are 14. How is that in Table 2 they become 30 and 44, respectively? This inconsistency is evident also for those who quit smoking (22 and 31 in Table 1 97 and 128, respectively in Table 3)
Response 3A: These inconsistencies have been corrected (see track changes in the Tables). Yet, the reported odds ratios are accurate and based on the correct number of events.
Though the inconsistencies have been fixed, we must clarify to the reviewer that the strata labeled "Lower income in 2009" in Tables 2 and 3 are combined categories, which we explained in the Methods section; "lower income in 2009" is computed from combining the low and middle incomes. Thus, regarding the final point of Comment 3A, the number of individuals in the lower incomes at baseline in Table 3 – for example – is 102, not 22. This explains the partial inconsistencies stated by the reviewer.
Finally, it must be stated that the totals from Table 1 for income and Tables 2 & 3 do not match, as there are missing values from 2009, i.e. not all subjects responded to the household income measures in 2009. We have added a footnote to both Tables 2 & 3 connoting this.

Comment 3B: Subjects who relapsed may be categorized by household income at baseline. In Table 2 there are 23+7=30 ex-smokers with low income at baseline who relapsed in 2009 and 28+16=44 ex-smokers with high income at baseline who relapsed in 2009. How is that the sum adds to 74, thus exceeding the total number of smokers who relapsed (56)? This inconsistency is evident also for those who quit smoking (Table 1 and Table 3).
Response 3B: These inconsistencies have been corrected (see track changes), which we also addressed in further detail above – in Response 3A.

Reviewer: Benjamin A. Shaw
Associate Professor and Chair
Department of Health Policy, Management, and Behavior
University at Albany
USA

Comment 1: I am still not certain of this manuscript's major contribution. In the Article Summary, a major focus is said to be the role of stress. However, in the Conclusion of the Abstract, and in the Results section of both the Abstract and the main body of the manuscript, the role of stress is either not discussed at all, or mentioned only briefly. Stress is only briefly mentioned in the Introduction as well. So, it seems that the authors are not clear on to what degree they want to focus on stress. If a focus on stress is intended, then further elaboration justifying this focus is needed in the Introduction, and the Results section should also include a more well-developed presentation of the results that involve stress.

Response 1: Thank you for pointing this out. We believe that the major focus of our paper is on the "natural experiment" which occurred in Iceland which provided us with an opportunity to examine the association of economic crisis on smoking relapse/cessation behaviors. To our knowledge, there are very few studies which have addressed the question of income change on changes in smoking behavior. Thus we believe our data addresses an important gap in the literature. Our findings are novel and potentially interesting because they run counter to the statically observed inverse association between income and smoking behavior (i.e. higher income = less smoking). Our results suggest that the typically observed inverse correlation between income & smoking is confounded by other unobserved factors, and that the marginal effect of an increase in income is to increase the risks of smoking (through relapse and lowered odds of quitting).
In light of the above, we have removed the stress-related focus from the Article Summary (see track changes). Additionally, the narrative in our Discussion is structured around this novelty, not stress-specifically, thus we have not made any changes to the body of the text.

Comment 2: Moreover, how the authors wish to combine and integrate findings of the impact of macroeconomic changes from 2007 to 2009 with the impact of individual level income changes is not clear. The authors mention that most prior research has been at the ecological level. By contrast, their focus is on the individual level. This, by itself, is a contribution. But it is not clear if similar findings would emerge at a different period of time, or whether the individual-level effects that were found are unique to a period of economic crisis at the ecological level.
Response 2: We thank the reviewer for drawing our attention to this ambiguity. As our findings are based upon a cohort that was exposed to a unique and severe economic crisis, we are unable to boldly generalize our findings to other normal situations. Thus, we have made additions to the text (see Study limitations in the Discussion section) cautioning the reader of the potential difficulties in generalizing our findings.

Comment 3: When measuring changes in stress from 2007 to 2009, how can small changes be distinguished from measurement error?
Response 3: We agree with the reviewer that stress is measured with error, and hence, it can be difficult to distinguish small changes from noise. As we clarified above, we would like to de-emphasize the discussion of stress in our article. We have included an analysis which incorporated the potentially mediating role of stress on income change and smoking behavior; however, we acknowledge that we cannot conclusively argue that stress did not play a role because of measurement error. Please see track changes for additions.
Comment 4: In the Results section, associations between employment status and smoking are presented. But, employment status is not presented as a measure in the Method section. Moreover, in the Article Summary, I was lead to believe that employment was not going to be addressed in this study (see the Strengths and limitations section).
Response 4: We have now included employment status in the Method section. However, the Article Summary states that "we were unable to assess the effects of an unemployment change on smoking habits" (i.e. we lacked statistical power to examine people's smoking habits as they transitioned from employed -> unemployed), although we were able to examine the association between static employment status and smoking. Perhaps this confusion is led by an unclear statement, thus we have

made changes to the Article Summary to reduce future confusion (please see Strengths and limitations section).

Comment 5: Also in the Results section, the authors report that females were less likely than males to quit smoking. However, they only present the predictors of quitting for females, not males.
Response 5: In the Results section pertaining to smoking cessation, only significant odds ratios for females are presented, as male odds ratios were not significant. However, women were less likely to quit compared to men. We state in the text as follows: “Females were less likely to quit smoking in 2009 (OR = 0.65; 95% CI: 0.45, 0.93), compared to males.”; i.e. compared to males (the referent group), females were less likely to relapse.

Comment 6: Additionally, I am confused by the last paragraph in the results section. How is it that stress change did not predict relapse, but that among females that relapsed, there was a significant increase in stress scores?
Response 6: While there was a significant increase in mean stress scores from 2007 to 2009 among relapsed females, when the association was examined using logistic regression, adjusted for underlying confounders (e.g. age, baseline stress), and compared to those with a stable stress score, the association is not significant. We have elaborated in the text further (see track changes in Stress and smoking subsection of the Results).

VERSION 3 – REVIEW

REVIEWER	Benjamin A. Shaw Associate Professor and Chair Department of Health Policy, Management, and Behavior University at Albany, School of Public Health USA No competing interests.
REVIEW RETURNED	29-Aug-2012

GENERAL COMMENTS	While I feel that this manuscript has continued to improve, I still think more could be done to integrate the national-level analyses of the economy-smoking relationship, with the individual level analyses. In my opinion, the Objective section of the Abstract should mention both of these levels. Also, while the individual level analyses regarding relapse are consistent with the national level trends in smoking (i.e., financial loss = less smoking), I think that more needs to be discussed regarding why the individual level analyses regarding quitting are not consistent with this pattern. That is, how is it possible that smoking rates went down at the national level, but at the individual level, people who lost income were not more likely to quit?

VERSION 3 – AUTHOR RESPONSE

Comment 1: While I feel that this manuscript has continued to improve, I still think more could be done to integrate the national-level analyses of the economy-smoking relationship, with the individual level analyses. In my opinion, the Objective section of the Abstract should mention both of these levels.

Response 1: We thank the reviewer for the encouraging response. We agree with his suggestion to better integrate the national & individual levels of analysis in the Abstract and Objectives of the paper. We have substantially re-written the Abstract and Objectives (please see track changes).

Comment 2: Also, while the individual level analyses regarding relapse are consistent with the national level trends in smoking (i.e., financial loss = less smoking), I think that more needs to be discussed regarding why the individual level analyses regarding quitting are not consistent with this pattern. That is, how is it possible that smoking rates went down at the national level, but at the individual level, people who lost income were not more likely to quit?

Response 2: The reviewer raises a very good point. It is possible that smoking cessation and smoking relapse are "asymmetric" behaviors with different triggers. Thus, a former smoker whose income drops may be less tempted to start smoking again because of the reduced affordability of cigarettes. On the other hand, someone who is already smoking may be less sensitive to an income drop (higher income inelasticity) –i.e. he is unable to quit his ongoing behavior because of the offsetting increase in stress (although our data on self-reported stress did not support this).

Regarding the discrepancy between national declines in smoking and the fact that smokers who lost income were not more likely to quit, our argument is that macro-level data and individual-level patterns are often driven by a different set of causes. Thus, the overall decline in national smoking rates could be either due to the procyclical nature of smoking (i.e. recessions are good for health), or it may simply reflect a continuation of trends already in place prior to the recession (i.e. national anti-smoking campaigns, changes in societal norms and acceptability of smoking, etc). In other words, national averages are driven by more than the group of smokers whose incomes decreased after the crisis.

We have added these points to the Discussion (please see pp. 8-9).

We thank Professor Shaw for his continuing helpful suggestions.