

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Determinants of Regular Smoking Onset in South Africa using Duration Analysis
AUTHORS	Vellios, Nicole; Walbeek, Corne

VERSION 1 - REVIEW

REVIEWER	Ce Shang IHRP, UIC, Chicago, IL, US
REVIEW RETURNED	10-Feb-2016

GENERAL COMMENTS	<p>This study analyzes the determinants of smoking onset in South Africa using duration analysis, focusing on the effects of prices. It is well-written and adds to the evidence on the determinants of smoking initiation in low- and middle- income countries. With some clarifications, I think this paper could make a great contribution to the literature. My comments are listed below.</p> <p>1, page 2, lines 42-46, this sentence is not clear. I think since the authors did not introduce any background information regarding the effects of higher price in increasing cessation in South Africa, this sentence is better written as "higher cigarette excise taxes will ultimately reduce smoking prevalence by increasing cigarette retail prices and discouraging young people from starting smoking."</p> <p>2, page 4, line 52, please define LMIC and UMIC.</p> <p>3. page 5 lines 3-5. The authors need to be careful with the interpretation of findings from China. There is evidence showing that the tax increases in China were failed to pass to prices because the tax increase was absorbed by producers. Therefore, there has not been a significant increase in cigarette prices in China in recent years.</p> <p>4, page 5 lines 49-60. For some reason, the manuscript includes previous review comments (likely from a submission to Tobacco Control). I share with their reviewer 1 the comment that it is not clear how the authors pooled 3 waves of longitudinal data to conduct duration analysis. The authors' response to the comment does not address the concern. I think the authors should specify that the same individuals only are entered to the study sample once. Then, the authors expended this sample to the analytical sample in person-year format based on their smoking history.</p> <p>5, page 7 Table 1, why did the authors choose to use 2010 exchange rates rather than the exchange rate of the year?</p> <p>6, page 7 lines 47-59, The price data was pooled using two different sources. It may be useful to add in a period dummy (1970-1989=0 and 1990-2012=1) as a control for this change.</p> <p>7, page 8, line 41, dropping those aged 48 or older is a little arbitrary. Why did the authors chose 48 as a cutoff, can they explain the rationale. Eg. Because the age and the period when price data is available?</p>
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	<p>8, page 9, line 46, I think “coefficient” should be changed to “odds ratio”. In the context of logistic regressions, coefficient usually refers to log-odds.</p> <p>9, page 9 line 50, the probability conversion. This formula is only right when the event is rare. Because smoking initiation in the person-year sample is rare, this formula works fine.</p> <p>10, I think it would be useful that the authors also report elasticity estimates and compare those to other estimates in the literature.</p> <p>11, I think the authors might want to drop the analyses regarding the effect of parent smoking on children’s smoking initiation. The reason is that you would be looking at a much smaller and potentially different sample (it made a difference in the female sample). The link between parents smoking and children smoking is well established, and this variable is very likely correlated with parents’ education, mother’s death, rural/urban difference, and race.</p>
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REVIEWER	Dean Lillard Ohio State University United States
REVIEW RETURNED	11-Feb-2016

GENERAL COMMENTS	<p>Review of “Determinants of Smoking Onset in South Africa using Duration Analysis” Manuscript Number: bmjopen-2016-011076</p> <p>Overview</p> <p>In this paper Vellios and Walbeek investigate whether South African youth are less likely to start to smoke regularly when they must pay more to buy cigarettes. They use data on lifetime smoking events collected by the National Income Dynamics Study (NIDS). Those data ask respondents “Do you smoke cigarettes?”; “Did you ever smoke cigarettes regularly?”; and “How old were you when you first smoked cigarettes regularly?” They use these data to construct indicators of each respondent’s smoking status in each year of life. They then combine these panel data with time series data on cigarette prices and (time-invariant) demographic factors of respondents and their parents that are available in NIDS.</p> <p>Using a discrete time hazard specification, the authors find that males are less likely to start (regular) smoking when cigarettes cost more and that cigarette prices do not alter a woman’s decision to start (regular) smoking.</p> <p>Comments -Contribution</p> <p>This paper contributes marginally to a fairly robust literature. Despite the claims of the authors (citing Guindon 2014), there is quite convincing evidence that the probability of smoking initiation is lower when cigarette prices and taxes are higher (c.f. Lillard et al. 2013).</p> <p>But the paper still contributes because it uses panel data for South Africa. There are far fewer studies of how price affect smoking initiation that use panel data in the developing world context.</p> <p>The paper is well written. It uses appropriate data and a standard study design to test a straightforward but policy relevant hypothesis.</p> <p>I have no major critiques of the paper but plenty of minor comments</p>
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	<p>- all of which the authors can easily and quickly address.</p> <p>Specific comments (in no particular order)</p> <p>1. Cite studies to support the validity and reliability of retrospectively reported data</p> <p>Since many readers are unaware of published research that validate such data, it is important to cite the most recent literature. For example, see Lillard et al (2014) and references cited therein.</p> <p>2. Provide robustness check to exclude unobserved determinants of smoking behavior</p> <p>As the authors acknowledge, the cigarette price data vary only temporally. In such cases, one must confront the possibility that unmeasured factors determine smoking initiation and that those factors follow a trend similar to the trend in cigarette prices (taxes). For example, one might expect smoking initiation decisions to vary with unemployment rates or the trend in per capita income. At least in the developed world, cigarette consumption rises when unemployment rises. So if the unemployment rate among South African male youth is falling as cigarette taxes/prices are rising, then the estimated coefficient on the cigarette price captures both a price effect and</p> <p>the effect of lower unemployment. Alternatively, the coefficient might be biased downwards if cigarette prices are rising as per capita incomes are rising. In either case, the interpretation of the results remains open because the authors fail to control for those other factors (which also vary temporally). An easy fix is to include several of those factors and report the robustness of the results (perhaps in a footnote).</p> <p>3. Missing covariates</p> <p>Why do the authors not control for household income? I understand that they observe household income in NIDS in only three years. However, it is important to account for differences in household income across respondents who “look the same” in terms of the demographic factors the authors include (race, urban/rural residence, parental education, literate, mother’s mortality).</p> <p>One way to do this is to average household income across all years one observes (as a proxy for “permanent” household income) and include that measure plus an indicator of the number of years over which one averaged the income.</p> <p>4. Can the authors provide evidence that cigarette prices do not vary nationally?</p> <p>The authors assert (p. 8) “Since cigarette prices in South Africa are set at a national level, there is little/no variation in prices across regions.” I am skeptical of this statement. In the former Soviet Union, the government set “official” prices of all goods. But the price people actually paid for those goods varied tremendously across regions.</p> <p>Do the authors have data on the price people actually paid in each area of the country? Or is it the case that they only have data on average prices in each year?</p>
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Mind you, the paper's results still stand if it is the latter. But the authors should be up front about what price data they actually have. Even better would be a reference to a study that documents regional price variation in the official and black market/street price of cigarettes (if such a study exists). Because the average will (almost certainly) attenuate estimated price effects towards zero, reference to such a study would allow the reader to crudely gauge how much attenuation bias is present in the reported results.

5. Sample restrictions

In almost every country in the world (including South Africa) few people start smoking after age 30 (for patterns in both developed and developing countries, see Lillard and Christopoulou 2015). Therefore the authors should probably drop non smokers who haven't started smoking by age 30.

Because non smokers older than 30 are probably a selected sample, keeping them in the sample is likely biasing the estimated coefficients toward zero. At the very least, the authors should run their models on two samples (10-pick an upper age and 10-30). They can decide which sample is the "main" sample. They should, however, report results from both samples. They should report results using the main sample in text and results using the other sample in a footnote.

The sample will necessarily exclude people who were non smokers and age 26 or older in 1970. Therefore, the oldest respondent they must exclude would be 2012 respondents age 68 and older

(who were non smokers).

6. Errors associated with retrospectively reported data

The use of retrospectively reported data carries two possible errors - recall and sample selection. The recall errors show up in the form of heaping in the distribution of the age people said they start to smoke regularly. (the study cited in point 1 contains relevant references). Bar and Lillard (2012) document that US smokers generally recall the age they started pretty well - even at older ages. Consequently, there is no obvious reason to restrict the sample to people who are younger than 49 when interviewed (Side note: The choice of that age is apparently completely arbitrary. The authors should justify it.).

Although Bar and Lillard (2012) offer a statistical procedure to adjust for recall bias, an easy (rather crude) adjustment is to simply code up a dummy variable for the year a person turns age 16 and the year he/she turns age 18. Include both dummy variables and the interaction between those dummy variables and the cigarette price.

The second type of error associated with retrospectively reported data arises because smokers are more likely to die (at every age) than non smokers. Consequently, when using retrospectively reported data to infer smoking behavior in earlier years, one may have to create weights to adjust the proportion of (eventual) smokers and non smokers in the sample one uses.

Christopoulou et al (2011), provide a statistical adjustment and/or evidence to support the use of a given age cutoff. I recommend the

	<p>authors use the latter approach since differential mortality is unlikely to severely bias their results. Still, the authors should acknowledge the possible bias.</p> <p>7. Results on parental smoking are a correlation only</p> <p>The authors cite literature on the probability a child starts to smoke conditional on having a parent who smokes (or smoked). They imply that the literature has established that this relationship is causal. But that point has not been established. In fact, statistically stronger studies (using IV estimation) reject the hypothesis that a child is more likely to start smoking if his/her parent smokes (or smoked). See Lillard (2011) and references cited therein.</p> <p>The authors should edit the appropriate text to clearly indicate the correlational nature of that result.</p> <p>8. In Concluding remarks it would be nice for the authors to circle back and put their elasticity estimates into the context of those reported in the wider literature. The Chaloupka and Warner chapter in the Handbook on Health Economics (2000) is rather dated now but the authors can use the evidence they cite and evidence cited in more recent studies.</p> <p>References</p> <p>Bar, H. and Lillard, D. 2012. "Accounting for Heaping in Retrospectively Reported Event Data - A Mixture-Model Approach." <i>Statistics in Medicine</i>. 31(27): 3347-3365.</p> <p>Christopoulou, R., Han, J., Jaber, A., and Lillard, D. 2011. "Dying for a Smoke: How Much Does Differential Mortality of Smokers Affect Estimated Life-Course Smoking Prevalence?" <i>Preventive Medicine</i>, 52: 66-70.</p> <p>Lillard, D. 2011. "Keeping it in the Family? If Parents Smoke Do Children Follow?" <i>Journal of Applied Social Science Studies</i>. 131: 277-286.</p> <p>Lillard, Dean R. and Christopoulou, Rebekka (eds.). 2015. "Life-course smoking behavior: Patterns and national context in ten countries." Oxford University Press.</p> <p>Lillard, D. R., Molloy, E. and Sfeakas, A. 2013. "Smoking Initiation and the Iron Law of Demand." <i>Journal of Health Economics</i>, 32: 114-127.</p> <p>Lillard, D. R., Christopoulou, R., and Gil-Lacruz, A. 2014. "Validation of a Method for Reconstructing Historical Rates of Smoking Prevalence." <i>American Journal of Epidemiology</i>. Vol. 179(1): 15-19. doi: 10.1093/aje/kwu184.</p>
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REVIEWER	Fabrice Etilé Paris School of Economics France
REVIEW RETURNED	11-Feb-2016

GENERAL COMMENTS**Determinants of Smoking Onset in South Africa using Duration Analysis**

This well-written research article addresses the important issue of price-based smoking prevention policies in middle-income countries. It exploits individual-level data from nationally representative South-African data. Using retrospective information on age of smoking onset, the authors implement a duration analysis of the hazard of starting as a function of the time-varying price of tobacco and a set of individual-level control variables. Age of starting is found to be price elastic for men but not for women.

Given the comments made by the other reviewers, I will focus my review on a few other points.

1. Is it possible that a mortality bias affect your sample in a country where the AIDS epidemic has been disastrous? If the propensity to get infected by HIV is correlated with smoking through, for instance, risk preferences, then you will have a bias. You have to discuss this issue.
2. Price data (Pg7). Does the CSS report only the “average price of a pack of 20 cigarettes in the popular price range”? Or, does it also report the minimal price? My concern here is that smokers, especially young smokers, may easily substitute brands. Hence, the low price-elasticity may just reflect the fact that young smokers now start with lower quality brands than in the 70s.
3. The endogeneity of the main policy variable has to be discussed.
 - a. Prices may be endogenous, as producers set their prices as a function of consumer tastes and consumers’ expected price response. Hence, unobserved taste variations may simultaneously affect the demand and the prices. One solution could be to use the excise tax as an instrument for the prices (using a 2 stage residual inclusion approach, see Terza et al., 2008).
 - b. Alternatively, I would like to see estimation results with the excise tax instead of the price.
 - c. Last, even the excise tax can be endogenous if it varies with some social norm that has a direct impact on demand. It is actually quite difficult to disentangle the impact of tax hikes from the impact of the information shocks (media coverage) that generally occur at the same moment.
4. Smuggling has to be discussed, as it may induce actual price variations across regions. Would it be possible that smuggling has increased over time?
5. Is it also possible that South-African youths start smoking with other products than tobacco?
6. Given the low rate of smoking initiation (39% for men, 11% for women), I am a bit surprised by the similarity of the results between the standard model and the split-population model. What are the predicted split probabilities? You can estimate a model where the split probability would depend on the time-invariant covariates and the average price over the period of observation. See for instance Douglas & Hariharan (1994).
7. You should mention on Pg8 (Methods) that an individual drops from the sample once s-he starts smoking.
8. Is there only one observation per household in each regression? i.e. do you have households with two men/women (or more). In that case, you should cluster the standard errors at the household level.
9. You cannot control for other policies that occurred simultaneously. Hence, Pg. 9, you should mention that you probably identify only an upper bound on the impact of prices.
10. The effects of parental education and parental smoking are interesting, but some third factors – for instance a gene-grounded propensity to act impulsively - may simultaneously explain parental

	<p>education/smoking and individual smoking. 11. You can update reference 20.</p> <p>Bibliography Terza, J. V., Basu, A., Rathouz, P. J. (2008). Two-stage residual inclusion estimation: addressing endogeneity in health econometric modeling. <i>Journal of Health Economics</i>, 27(3), 531-543.</p> <p>Douglas, S., Hariharan, G. (1994). The hazard of starting smoking: estimates from a split population duration model. <i>Journal of Health Economics</i>, 13(2), 213-230.</p> <p>Etilé, F., Jones, A. M. (2011). Schooling and smoking among the baby boomers—an evaluation of the impact of educational expansion in France. <i>Journal of Health Economics</i>, 30(4), 811-831.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Ce Shang
 Institution and Country: IHRP, UIC, Chicago, IL, USA.
 Competing Interests: none declared.

Comment: This study analyzes the determinants of smoking onset in South Africa using duration analysis, focusing on the effects of prices. It is well-written and adds to the evidence on the determinants of smoking initiation in low- and middle- income countries. With some clarifications, I think this paper could make a great contribution to the literature. My comments are listed below.

Response: Thank you.

Comment 1: page 2, lines 42-46, this sentence is not clear. I think since the authors did not introduce any background information regarding the effects of higher price in increasing cessation in South Africa, this sentence is better written as “higher cigarette excise taxes will ultimately reduce smoking prevalence by increasing cigarette retail prices and discouraging young people from starting smoking.”

Response: We have changed the sentence to: “*Higher cigarette excise taxes, which lead to higher retail prices, reduce smoking prevalence by encouraging smokers to quit and by discouraging young people from starting smoking.*”

Comment 2: page 4, line 52, please define LMIC and UMIC.

Response: Done.

Comment 3: page 5 lines 3-5. The authors need to be careful with the interpretation of findings from China. There is evidence showing that the tax increases in China were failed to pass to prices because the tax increase was absorbed by producers. Therefore, there has not been a significant increase in cigarette prices in China in recent years.

Response:

The paper on China does not mention anything about producers absorbing tax increases. Since we are reporting the results of that paper, we are not in a position to evaluate those findings. In their abstract they indicate the following: “*We find results, consistent with some previous empirical evidence, that Chinese smoking is not strongly related to the price of cigarettes*”. If we leave out the result of this paper, then it looks odd because we speak about the results of all the other 4 papers. One of the authors of the China paper is reviewer 2 (Lillard) who did not have a problem with the way this was worded. As such we would prefer to keep the wording as is.

Comment 4: page 5 lines 49-60. For some reason, the manuscript includes previous review comments (likely from a submission to Tobacco Control). I share with their reviewer 1 the comment that it is not clear how the authors pooled 3 waves of longitudinal data to conduct duration analysis. The authors' response to the comment does not address the concern. I think the authors should specify that the same individuals only are entered to the study sample once. Then, the authors expended this sample to the analytical sample in person-year format based on their smoking history.

Response: This is an important clarification. We have added in this sentence: *"The same individuals across waves are entered only once in the study sample."*

Comment 5: page 7 Table 1, why did the authors choose to use 2010 exchange rates rather than the exchange rate of the year?

Response: The primary reason for indicating the USD price of cigarettes is to make the paper more intelligible for international readers. The exchange rate for 2010 was \$1 = R7.33 which is very close to the average between 2003 and 2012, which although volatile, has averaged at \$1 = R7.35. Since we used 2010 as the base for the real values of cigarettes, it seemed sensible to use the exchange rate of that year for consistency purposes.

Comment 6: page 7 lines 47-59. The price data was pooled using two different sources. It may be useful to add in a period dummy (1970-1989=0 and 1990-2012=1) as a control for this change.

Response: The sources are not different but the explanation of the data sources was not worded well, which introduced some confusion. Thank you for pointing this out. We have reworded the explanation from *"Data on average cigarette prices are derived from two sources. The price data for 1970 to 1989 were taken from the Central Statistical Services' (CSS) Report on Prices, while 1990-2012 price data were obtained from Statistics South Africa (previously called the CSS)"* to *"Data on average cigarette prices are derived from Statistics South Africa, previously known as Central Statistical Services (CSS). The price data for 1970 to 1989 were taken from the CSS's Report on Prices, while 1990-2012 price data were obtained directly from Statistics South Africa"*. The data collection from these 2 sources is the same, so there is no need to add in a period dummy as a control since there was no change.

Comment 7: page 8, line 41, dropping those aged 48 or older is a little arbitrary. Why did the authors chose 48 as a cutoff, can they explain the rationale. Eg. Because the age and the period when price data is available?

Response: We concur with the reviewer that age 48 seems arbitrary. While we do have price data before 1972, we are less confident about the voracity and consistency of the price data. The reason is that the inflation of the 1970s and 1980s has made the CPI values of the 1960s so small that they are not picking up the modest inflation that marked that decade. For example, the CPI value for 1960 is 1.5 (base Dec 2012 = 100). The CPI value for 1961 and 1962 and 1963 is also 1.5. This suggests that the inflation rate was 0%, which is not true. Because of the CPI effect, the real prices for the 1960s the early 1970s are subject to measurement error.

Even so, we reran the regressions up to 1961 (we do not have any price data before 1961). The results were remarkably similar. The expanded regression (with 11 additional years) for males is shown below (n=112 035) compared to the initial regression below (table 2 model 1) (n= 96 298). Given that the results are so similar, and our concerns raised about the price, we prefer to use the data that does not use prices from the 1960s.

Response: We thought about this comment carefully and we concur with the reviewer that a comparison between model 2 and model 4 for females does indicate that the smaller sample is different to the larger sample. For males there are no significant differences in the sample, based on the econometric results. As far as we know, there are no studies in South Africa that have investigated the relationship between parents' smoking and children's smoking. While this association sounds intuitive and has been established in other countries, it is an important result for South Africa. We are thus faced with a trade-off between academic rigour and a finding that is relevant and important for a particular country.

While the other reviewers commented on the discussion on the impact of parental smoking on children smoking, they did not suggest that we delete models 4 and 5. Based on another reviewer's comments, we have reworded the sentences related to parental smoking so that the results read as correlations, rather than causations. Given the potentially important impact of this result for South Africa, we would prefer to keep these models in the paper.

Reviewer: 2

Reviewer Name: Dean Lillard
Institution and Country: Ohio State University, United States.
Competing Interests: None declared

Please leave your comments for the authors below

Nice paper.
See detailed comments in the attached review.

Overview Comment: In this paper Vellios and Walbeek investigate whether South African youth are less likely to start to smoke regularly when they must pay more to buy cigarettes. They use data on lifetime smoking events collected by the National Income Dynamics Study (NIDS). Those data ask respondents "Do you smoke cigarettes?"; "Did you ever smoke cigarettes regularly?"; and "How old were you when you first smoked cigarettes regularly?" They use these data to construct indicators of each respondent's smoking status in each year of life. They then combine these panel data with time series data on cigarette prices and (time-invariant) demographic factors of respondents and their parents that are available in NIDS.

Using a discrete time hazard specification, the authors find that males are less likely to start (regular) smoking when cigarettes cost more and that cigarette prices do not alter a woman's decision to start (regular) smoking.

Comment: This paper contributes marginally to a fairly robust literature. Despite the claims of the authors (citing Guindon 2014), there is quite convincing evidence that the probability of smoking initiation is lower when cigarette prices and taxes are higher (c.f. Lillard et al. 2013).

But the paper still contributes because it uses panel data for South Africa. There are far fewer studies of how price affect smoking initiation that use panel data in the developing world context.

Response: Thank you for the positive comments.

Comment: The paper is well written. It uses appropriate data and a standard study design to test a straightforward but policy relevant hypothesis. I have no major critiques of the paper but plenty of minor comments - all of which the authors can easily and quickly address.

Specific comments (in no particular order)

Comment 1: Cite studies to support the validity and reliability of retrospectively reported data. Since many readers are unaware of published research that validate such data, it is important to cite the most recent literature. For example, see Lillard et al (2014) and references cited therein.

Response: We have added this sentence to the methods section: *“Although recall error is a significant risk, the literature indicates that historical smoking prevalence estimates reported retrospectively are valid”*, with reference: Lillard DR, Christopoulou R, Gil-Lacruz A. Letters to the Editor. Re: “Validation of a Method for Reconstructing Historical Rates of Smoking Prevalence” American Journal of Epidemiology 2014;**180**(6):656-59.

Comment 2: Provide robustness check to exclude unobserved determinants of smoking behaviour. As the authors acknowledge, the cigarette price data vary only temporally. In such cases, one must confront the possibility that unmeasured factors determine smoking initiation and that those factors follow a trend similar to the trend in cigarette prices (taxes). For example, one might expect smoking initiation decisions to vary with unemployment rates or the trend in per capita income. At least in the developed world, cigarette consumption rises when unemployment rises. So if the unemployment rate among South African male youth is falling as cigarette taxes/prices are rising, then the estimated coefficient on the cigarette price captures both a price effect and the effect of lower unemployment. Alternatively, the coefficient might be biased downwards if cigarette prices are rising as per capita incomes are rising. In either case, the interpretation of the results remains open because the authors fail to control for those other factors (which also vary temporally). An easy fix is to include several of those factors and report the robustness of the results (perhaps in a footnote).

Response: The author makes a valid observation. We considered two variables that could potentially influence smoking behaviour, i.e. unemployment and GDP per capita. In South Africa we do not have consistent unemployment time series data for the following reasons: (1) between 1976 and 1994, some parts of South Africa were not monitored because these homelands were “independent” under the Apartheid system; (2) the definition of unemployment has varied over time. Currently SA uses a narrow definition of unemployment but in the past SA used a broad definition, which included discouraged work seekers, and (3) In the past, the unemployment rate in rural areas was typically under-estimated and under-reported.

The bottom line is that unemployment rates are not an appropriate measure of social well-being. Instead, we use GDP per capita as a possible variable which captures these unobserved determinants. We find that the correlation between the real price of cigarettes and the real per capita GDP is quite substantial. GDP per capita was decreasing in the 1980s, as was the real price of cigarettes, and from the mid-1990s both variables were increasing. We found that an increase in the real per capita GDP was associated with a decrease in smoking initiation. This result is counterintuitive and probably spurious. Including the per capita GDP in the analysis reduces the impact of price on smoking initiation (for males), but not completely. There is a very large literature that indicates the positive relationship between income and smoking behaviour.

Thus, while we acknowledge that unobserved determinants of smoking behaviour could impact the results, the results need to be plausible. Per capita GDP is clearly not a plausible unobserved variable.

Comment 3: Missing covariates

Why do the authors not control for household income? I understand that they observe household income in NIDS in only three years. However, it is important to account for differences in household income across respondents who “look the same” in terms of the demographic factors the authors include (race, urban/rural residence, parental education, literate, mother’s mortality).

One way to do this is to average household income across all years one observes (as a proxy for “permanent” household income) and include that measure plus an indicator of the number of years over which one averaged the income.

Response: We do not include household income as household income at time of interview is unlikely to be similar to household income at the time a person is deciding to smoke or not. We have added this paragraph to the methods section: *“Parents’ education is used as a proxy for household income at the time when the respondent was a child or teenager. Given the well-established relationship between education and income, parents’ education is likely to be a better indicator of household income at the time the person was susceptible to starting smoking than current income levels. The variable is coded as either parent’s highest level of education.”* In South Africa the relationship between education and income is particular strong.

Comment 4:

Can the authors provide evidence that cigarette prices do not vary nationally? The authors assert (p. 8) “Since cigarette prices in South Africa are set at a national level, there is little/no variation in prices across regions.” I am skeptical of this statement. In the former Soviet Union, the government set “official” prices of all goods. But the price people actually paid for those goods varied tremendously across regions.

Do the authors have data on the price people actually paid in each area of the country? Or is it the case that they only have data on average prices in each year?

Mind you, the paper’s results still stand if it is the latter. But the authors should be up front about what price data they actually have. Even better would be a reference to a study that documents regional price variation in the official and black market/street price of cigarettes (if such a study exists). Because the average will (almost certainly) attenuate estimated price effects towards zero, reference to such a study would allow the reader to crudely gauge how much attenuation bias is present in the reported results.

Response:

We have recently been able to obtain average prices by province from Statistics South Africa, for the period December 2001 to December 2005 and January 2008 to the end of the period of interest. There are modest (but in some cases, significant) differences in the average prices between provinces. Thank you for pointing this out. We redid the whole analysis using the provincial price data. Where there were no provincial price data (i.e. pre 2001 and 2006-07), we used national price data. The difference is very modest and the results did not change substantially. The overall “story” is the same as before.

We have incorporated the slight change in price data in the description in the Data section. We have updated tables 2 and 3. We have deleted the sentence “*Since cigarette prices in South Africa are set at a national level, there is little/no variation in prices across regions.*” We have added in the sentence: “*National prices are used for all years except 2002-2005 and 2008–2012, where we use provincial prices (these are the only years where prices by province is available).*”

In addition, we have added this sentence to the limitations: “*For the years 2002-2005 and 2007 – 2012, we match cigarette prices by province to current province where the respondent resides at the time of interview. Since province at time of interview, and province at time of making the decision to start smoking may not be the same, there is likely to be some measurement error.*”

In the paper we also state that: “*The data used in the analysis refer to the average price of a pack of 20 cigarettes in the popular price range (which comprises about 70% of the market).*” Unfortunately there is no data in South Africa on black market cigarettes, but some of these brands would be captured by stats SA’s “economy” brands which we do have information on. We reran the regressions using economy prices, and found that the estimates are very similar to the most popular price category. We have added in this sentence: “*We tested the robustness of the results using low-price/discounted brands (which comprise 10-15% of the market), and found that the estimates were similar to those of the popular price category brands.*”

Comment 5: Sample restrictions

Because non smokers older than 30 are probably a selected sample, keeping them in the sample is likely biasing the estimated coefficients toward zero. At the very least, the authors should run their models on two samples (10-pick an upper age and 10-30). They can decide which sample is the “main” sample. They should, however, report results from both samples. They should report results using the main sample in text and results using the other sample in a footnote.

The sample will necessarily exclude people who were non smokers and age 26 or older in 1970. Therefore, the oldest respondent they must exclude would be 2012 respondents age 68 and older (who were non smokers).

Response: We fully understand the first paragraph but we do not understand the second. We suspect that we have not explained the methodology precisely enough and we have subsequently enhanced the explanation as follows: *“We include individuals aged 48 and younger at the time of interview, but only follow individuals until age 30, since very few people start smoking after age 30”*.

Consider the following example: A person born in 1972 would be 40 in 2012. He would be 10 in 1982 at which point he would be subject to the risk of smoking. In 2002, he would be 30. If he had not started smoking by then, he would be most unlikely to start subsequently. For ages 31+ he would not be observed.

Comment 6: Errors associated with retrospectively reported data

The use of retrospectively reported data carries two possible errors - recall and sample selection. The recall errors show up in the form of heaping in the distribution of the age people said they start to smoke regularly. (the study cited in point 1 contains relevant references). Bar and Lillard (2012) document that US smokers generally recall the age they started pretty well - even at older ages. Consequently, there is no obvious reason to restrict the sample to people who are younger than 49 when interviewed (Side note: The choice of that age is apparently completely arbitrary. The authors should justify it.).

Although Bar and Lillard (2012) offer a statistical procedure to adjust for recall bias, an easy (rather crude) adjustment is to simply code up a dummy variable for the year a person turns age 16 and the year he/she turns age 18. Include both dummy variables and the interaction between those dummy variables and the cigarette price.

The second type of error associated with retrospectively reported data arises because smokers are more likely to die (at every age) than non smokers. Consequently, when using retrospectively reported data to infer smoking behavior in earlier years, one may have to create weights to adjust the proportion of eventual smokers and non smokers in the sample one uses.

Christopoulou et al (2011), provide a statistical adjustment and/or evidence to support the use of a given age cutoff. I recommend the authors use the latter approach since differential mortality is unlikely to severely bias their results. Still, the authors should acknowledge the possible bias.

Response: We concur with the reviewer that age 48 seems arbitrary. While we do have price data before 1972, we are less confident about the voracity and consistency of the price data. The reason is that the inflation of the 1970s and 1980s has made the CPI values of the 1960s so small that they are not picking up the modest inflation that marked that decade. For example, the CPI value for 1960 is 1.5 (base Dec 2012 = 100). The CPI value for 1961 and 1962 and 1963 is also 1.5. This suggests that the inflation rate was 0%, which is not true. Because of the CPI effect, the real prices for the 1960s the early 1970s are subject to measurement error.

Even so, we reran the regressions up to 1961. We do not have any price data before 1961. The results were remarkably similar. The expanded regression (with 11 additional years) for males is shown below (n=112 035) compared to the initial regression below (table 2 model 1) (n= 96 298). Given that the results are so similar, and our concerns raised about the price, we prefer to use the data that does not use prices from the 1960s.

- We deleted “Children of non-smoking parents are substantially less likely to initiate regular smoking than those who have at least one parent who smokes” and replaced with “Regular smoking among parents is positively correlated to smoking initiation among children.”
- “For both males and females (model (5) of Tables 2 and 3), having at least one parent who smokes **significantly increases** the probability of initiating regular smoking (OR=1.935, 95% CI: 1.600, 2.340 for males and OR=2.075, 95% CI: 1.380, 3.120 for females)” to “For both males and females (model (5) of Tables 2 and 3), having at least one parent who smokes **is correlated with** the probability of initiating regular smoking (OR=1.935, 95% CI: 1.600, 2.340 for males and OR=2.075, 95% CI: 1.380, 3.120 for females).”
- “The results indicate that regular smoking initiation is **positively related** to regular parental smoking” to “The results indicate that regular smoking initiation is **correlated with** regular parental smoking.
- We have deleted this sentence: “Children of non-smoking parents are less likely to initiate regular smoking than children of regular smoking parents.”
- Deleted: “An increase in the price of cigarettes decreases regular smoking prevalence among parents, which in turn decreases regular smoking initiation among their children.”

Comment 8: In Concluding remarks it would be nice for the authors to circle back and put their elasticity estimates into the context of those reported in the wider literature. The Chaloupka and Warner chapter in the Handbook on Health Economics (2000) is rather dated now but the authors can use the evidence they cite and evidence cited in more recent studies.

Response: While we agree with the reviewer, it is difficult to compare elasticity estimates from smoking initiation studies, primarily because different studies use different time origins (age at which one assumes a person is at risk of initiating smoking). In addition, even if one looks just at studies that use the same timer origin as we do (age 10), elasticity estimates are not always reported.

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Reviewer: 3

Reviewer Name: Fabrice Etilé
 Institution and Country: Paris School of Economics, France
 Competing Interests: None declared
 Referee report BMJopen-2016-011076

Comment: This well-written research article addresses the important issue of price-based smoking prevention policies in middle-income countries. It exploits individual-level data from nationally representative South-African data. Using retrospective information on age of smoking onset, the

authors implement a duration analysis of the hazard of starting as a function of the time-varying price of tobacco and a set of individual-level control variables. Age of starting is found to be price elastic for men but not for women. Given the comments made by the other reviewers, I will focus my review on a few other points.

Comment 1: Is it possible that a mortality bias affect your sample in a country where the AIDS epidemic has been disastrous? If the propensity to get infected by HIV is correlated with smoking through, for instance, risk preferences, then you will have a bias. You have to discuss this issue.

Response: In Wave 1 of the NIDS dataset, only 1.22% of people indicate that they have HIV/Aids. This is a gross underestimation. This is probably because of 2 reasons: the way the question is asked is not forthcoming (see snippet of questionnaire below). Individuals are first asked if they have any major health conditions of which HIV/Aids is not one of them. Individuals are then asked if they have any other illnesses, to which they can answer yes or no. The ones that answer yes are then asked what the illnesses are. This indirect method resulted in massive under-reporting. Secondly, because of the stigma attached to HIV/Aids, it is likely that many respondents did not answer truthfully.

INTERVIEWER READ OUT: Now we would like to ask you about some particular health conditions.							
	J13 Have you ever been told by a doctor, nurse or health care professional that you have [...] (If No move to the next condition)		J14 In which year were you diagnosed with this condition?	J15 Are you currently taking medication for this condition? (If Yes, move to the next condition)		J16 Do you still have this condition?	
	Yes	No		Yes	No	Yes	No
1. Tuberculosis / TB	1	2		1	2	1	2
2. High blood pressure	1	2		1	2	1	2
3. Diabetes or high blood sugar	1	2		1	2	1	2
4. Stroke	1	2		1	2	1	2
5. Asthma	1	2		1	2	1	2
6. Heart Problems	1	2					
7. Cancer	1	2					

J17 	Do you have any other major illnesses or disability not mentioned above?	Yes	1
		No → SKIP TO J19	2
		Refused → SKIP TO J19	8
		Don't know → SKIP TO J19	9
J18	If yes, what are they? Interviewer: Do not read out Multiple mentions allowed	Physically handicapped	01
		Problems with sight, hearing or speech	02
		Psychological or psychiatric disorder	03
		HIV/AIDS	04
		Epilepsy/ fits	05
		Emphysema	06
		Alzheimer's disease	07
		Other (Specify)	08
		Refused	88

Comment 2: Price data (Pg7). Does the CSS report only the “average price of a pack of 20 cigarettes in the popular price range”? Or, does it also report the minimal price? My concern here is that smokers, especially young smokers, may easily substitute brands. Hence, the low price-elasticity may just reflect the fact that young smokers now start with lower quality brands than in the 70s.

Response: For the period prior to 2001 the CSS reports only the average price. After 2001 we have access to a much more detailed price data set, which allows one to determine the spread of cigarette prices. Before 2004, and by extrapolation, also before 2001, the recorded prices of cigarettes were very tightly distributed around the mean. There was no discount or low price market. As indicated in a response to the second reviewer, we have provincial data for the period 2002-2005 and 2008-2010.

At the request of the reviewer we redid the regressions for discount brands (using the average price for the period 1970-2000 and using the more disaggregated price series for the period 2001 – 2005 and 2008-2010). We interpolated the 2006-07 prices. For a small number of province-year combinations we had to make some assumptions, because these low-price brands were not monitored by Statistics South Africa in some years (presumably because these low-price cigarettes are so relatively unimportant that Statistics South Africa felt that they did not justify being monitored).

With these caveats in mind, the analysis was redone and the results were qualitatively identical as when we used the popular price range. We have included this sentence to the results section: “We

tested the robustness of the results using low-price/discounted brands (which comprise 10-15% of the market), and found that the estimates were similar to those of the popular price category brands”.

Comment 3a: The endogeneity of the main policy variable has to be discussed.

Prices may be endogenous, as producers set their prices as a function of consumer tastes and consumers' expected price response. Hence, unobserved taste variations may simultaneously affect the demand and the prices. One solution could be to use the excise tax as an instrument for the prices (using a 2 stage residual inclusion approach, see Terza et al., 2008).

Response 3a: When using survey data, the problem of price endogeneity is less of a concern because no individual tobacco user or potential user consumes enough to influence the market price. In our case, we are looking at potential users. Even taken as a group, they likely have little to no influence on the market price. In addition, price endogeneity is a particular concern if one uses self-reported prices. We do not.

Comment 3b: Alternatively, I would like to see estimation results with the excise tax instead of the price.

Response 3b: We do not agree with the referee. People make their decisions based on the prices that they see. Very few people are aware of the excise tax amount per pack. However, they are very aware of the price that they have to pay.

Comment 3c: Last, even the excise tax can be endogenous if it varies with some social norm that has a direct impact on demand. It is actually quite difficult to disentangle the impact of tax hikes from the impact of the information shocks (media coverage) that generally occur at the same moment.

Response 3c: We have considered this. Before 1994, the excise tax, which is levied as a nominal specific tax, was increased sporadically through government decision. This decision was driven by revenue reasons rather than public health reasons. Throughout this period it is difficult to see how these tax increases varied with a social norm. Smoking was regarded as normal, and the government had no tobacco control policy in place. In 1994, the government announced, relatively unexpectedly, that it aimed to set the total tax burden at 50% of the average retail price. Other than lobbying by the public health community, there was no strong anti-tobacco social norm in South Africa at that time. Since 1997, when the 50% target was achieved, the excise tax was increased *formulaically* on an annual basis to maintain the 50% tax burden. As such, we do not believe that there is evidence that the excise tax is strongly correlated to social norms.

In terms of tobacco control policy, South Africa is quite different from countries in Europe and North America. South Africa has used one tool extremely well: increasing the excise tax, and for many years (about 1995-2005) South Africa was regarded as a world leader in using the excise tax as a tobacco control tool. The other tobacco control tools have not been applied to the same extent. In fact we are behind. For example, there has never been comprehensive anti-tobacco advertising on anything close to the scale of that in the US and Australia. We have a ban on advertising and promotion (since 2001), but the warning labels are small, and not pictorial.

Tobacco use has decreased sharply in SA, and amongst some groups tobacco use is frowned upon (but it has always been frowned upon amongst such groups), but amongst the majority of the population, the decrease is driven by the sharply increasing price, much more than because of changing social norms.

Comment 4: Smuggling has to be discussed, as it may induce actual price variations across regions. Would it be possible that smuggling has increased over time?

Response: This is an important point. Thank you for highlighting this. We have added in this paragraph at the end of the discussion: *“South Africa should continue to increase the price of cigarettes through excise taxes. However, if consumers are able to buy cheaper illicit cigarettes, the impact of price increases is likely to be reduced. Governments should therefore implement measures to reduce the illicit trade of cigarettes as outlined by the WHO’s Framework Convention on Tobacco Control’s Protocol to Reduce Illicit Trade.”*

Regarding the issue of whether smuggling has increased over time, the industry creates a narrative that smuggling/illicit trade is high and rising. This narrative was initiated in 2006 without strong

evidence to support it (see Van Walbeek and Shai, 2015). This does not imply that illicit trade is not a problem, but it is greatly exaggerated by the industry.

Comment 5: Is it also possible that South-African youths start smoking with other products than tobacco?

Response: We assume that the reviewer is referring to cannabis, primarily. Yes, this is possible, however we are unable to track this as the NIDS dataset does not ask about smoking any other products.

Comment 6: Given the low rate of smoking initiation (39% for men, 11% for women), I am a bit surprised by the similarity of the results between the standard model and the split-population model. What are the predicted split probabilities? You can estimate a model where the split probability would depend on the time-invariant covariates and the average price over the period of observation. See for instance Douglas & Hariharan (1994).

Response: We have looked at the reference suggested but are unclear about what exactly the reviewer is suggesting. Can the reviewer provide a further explanation on this comment and how one would estimate the predicted split probabilities on Stata?

Comment 7: You should mention on Pg8 (Methods) that an individual drops from the sample once s- he starts smoking.

Response: We have added in this sentence to the methods section: *“An individual drops from the sample once he/she starts smoking.”*

Comment 8: Is there only one observation per household in each regression? i.e. do you have households with two men/women (or more). In that case, you should cluster the standard errors at the household level.

Response: There could be more than one observation per household. We do not think it is necessary to cluster the standard errors at the household level as we account for the effects of parental smoking in a subsample where we have information on parental smoking.

Comment 9: You cannot control for other policies that occurred simultaneously. Hence, Pg. 9, you should mention that you probably identify only an upper bound on the impact of prices.

Response: In the methods section we state that: *“In the initial analysis, we tested the impact of tobacco control policy changes in 1995 (warning labels on tobacco packaging and advertising material, and ban on smoking on public transport) and 2001 (tobacco advertising and sponsorship ban, prohibition of smoking in all public and work places, and prohibition of distributing free cigarettes and the sale of single cigarettes), but the impact was found to be insignificant.”*

We have added this sentence to the results section: *“These price effects are likely to be upper limits because we do not control for other tobacco control interventions of 1995 and 2001”.*

Comment 10: The effects of parental education and parental smoking are interesting, but some third factors – for instance a gene-grounded propensity to act impulsively - may simultaneously explain parental education/smoking and individual smoking.

Response: Agreed. However, there are no variables in the NIDS dataset that measure a person’s risk preferences.

Comment 11: You can update reference 20.

Response: Done

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VERSION 2 – REVIEW

REVIEWER	Ce Shang IHRP, UIC US
REVIEW RETURNED	29-Mar-2016

GENERAL COMMENTS	Thank you for responding to my comments. I have no further comments. Thank you.
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REVIEWER	Dean Lillard Department of Human Sciences 235B Campbell Hall 1787 Neil Avenue Columbus, OH 43210
REVIEW RETURNED	20-Apr-2016

GENERAL COMMENTS	<p>Comment 2: omitted determinants of smoking behavior</p> <p>My original comments called the author's attention to the fact that their identification strategy relies on variation in cigarette prices that is (mostly) temporal. I asked the authors to provide robustness checks to allay concerns that their result do not actually capture variation in other omitted variables that follow a trend similar to the trend in cigarette prices. They include per capita GDP as a potential omitted variable.</p> <p>They write, "...we use GDP per capita as a possible variable which captures these unobserved determinants. We find that the correlation between the real price of cigarettes and the real per capita GDP is quite substantial. GDP per capita was decreasing in the 1980s, as was the real price of cigarettes, and from the mid-1990s both variables were increasing."</p> <p>According to them, the results of that model were:</p> <p>"... an increase in the real per capita GDP was associated with a decrease in smoking initiation." They also state: "Including the per capita GDP in the analysis reduces the impact of price on smoking initiation (for males), but not completely."</p> <p>That is fine.</p> <p>But they then reach for some argument to justify not reporting these results to the readers.</p>
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	<p>They write: "This result is counterintuitive and probably spurious."</p> <p>Where, "This result" refers to the finding that higher per capita GDP reduces (the probability of) smoking initiation.</p> <p>They also state:</p> <p>"There is a very large literature that indicates the positive relationship between income and smoking behaviour. Thus, while we acknowledge that unobserved determinants of smoking behaviour could impact the results, the results need to be plausible. Per capita GDP is clearly not a plausible unobserved variable."</p> <p>These statements do not make sense.</p> <p>The authors defend themselves by citing a "large literature that indicates the positive relationship between income and smoking behaviour" but fail to note or distinguish the "large" literature showing the relationship between income (education) and smoking behavior is not constant over time - especially in developing countries.</p> <p>It is unreasonable to assert that per capita GDP is not a "plausible" unobserved variable.</p> <p>Therefore I would strongly suggest the following:</p> <ol style="list-style-type: none">1. Report the sensitivity of their results when they include per capita GDP as an additional control variable (footnote or extra column in Table 2)2. The authors should refer to the literature that shows that the relationship between smoking and per capita income and education vary across cohorts and time. The authors can find references to that literature in Christopoulou, R., Lillard, D. and Balmori, J. R. 2013. "Smoking behavior of Mexicans: patterns by birth-cohort, gender, and level of education." <i>International Journal of Public Health</i>, 58(3): 335-343. <p>Comment 3: omitted individual/household covariates - the response of the authors satisfies me.</p> <p>Comment 5 Sample restrictions The authors response was find. I am not particularly persuaded by the logic they use to justify excluding observations before 1970 (especially because the results in the "full" sample (using observations from 1961 to 2012) are essentially identical to those from the restricted sample (using observations from 1970 to 2012). Personally I would make the former the "main" results and refer to the latter results as robustness checks. But I don't feel strongly.</p> <p>To clarify for the authors. Even using 1970-2012 as their starting year, the authors unnecessarily restrict their sample. (but only marginally)</p>
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	<p>To be at risk a person needed to be 10 years old in 1970. That means he would be 52 in 2012. They exclude people who are age 49-52 in 2012.</p> <p>Comment 6 errors associated with retrospectively reported data</p> <p>6.1 Heaping - attenuation bias</p> <p>The inclusion of the indicators for age 16 and age 18 are insufficient to control for the attenuation bias associated with heaping.</p> <p>Instead one should include 1) dummy variable for age 16, 2) dummy variable for age 18, 3) dummy variable for age 16*cigarette price, and 4) dummy variable for age 18*cigarette price.</p> <p>The coefficient on the interaction terms captures the attenuation bias from heaping.</p> <p>6.2 Differential mortality of smokers and non smokers</p> <p>The authors write: "While we agree that smokers are more likely to die than non-smokers, we are unsure how one would go about creating weights to adjust for this. Can the reviewer please provide some guidance as we have not come across this in the literature before."</p> <p>I refer them to the paragraph they copied into their response: Christopoulou et al (2011), provide a statistical adjustment and/or evidence to support the use of a given age cutoff. I recommend the authors use the latter approach since differential mortality is unlikely to severely bias their results. Still, the authors should acknowledge the possible bias.</p> <p>If they had read Christopoulou et al (2011) they would have understood the statistical adjustment and been able to download the stata code (and data) they need to implement it.</p> <p>I would note: Reviewer 3's Comment 1 highlights raises an important and related point I hadn't considered in my previous comments. Since HIV prevalence was high in sub Saharan Africa, the question of differential mortality of smokers and non smokers may be especially important if smokers were more likely to develop HIV (because they are risk takers).</p> <p>Comment 8 circle back to literature in Chaloupka and Warner 2000 and recent literature</p> <p>I am completely unsatisfied with the authors' response.</p> <p>They write:</p> <p>"While we agree with the reviewer, it is difficult to compare elasticity estimates from smoking initiation studies, primarily because different studies use different time origins (age at which one assumes a person is at risk of initiating smoking). In addition, even if one looks</p>
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	<p>just at studies that use the same timer origin as we do (age 10), elasticity estimates are not always reported."</p> <p>This response is a dodge. I continue to believe that this paper will be more useful if they place and discuss their findings in the context of previous studies. They can qualify that discussion in the above ways. I am unconvinced that they should be excused from adding this discussion simply because one or more of the published studies exclude 10-12 year olds while their does not.</p> <p>A couple of minor comments: They should cite my 2013 Journal of Health Economics paper because it isn't included in the 2013 Guindon review. (Lillard, D. R., Molloy, E. and Sfekas, A. 2013. "Smoking Initiation and the Iron Law of Demand." Journal of Health Economics, 32: 114-127.)</p> <p>Throughout the paper they sometimes refer to data in the singular. Data are plural.</p> <p>PS Here is a brand new paper that might interest you: A Novel Indicator of Life-Course Smoking Prevalence in the United States Combining Popularity, Duration, Quantity, and Quality of Smoking http://ajph.aphapublications.org/doi/abs/10.2105/AJPH.2016.303130?af=R</p>
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REVIEWER	Fabrice Etilé Paris School of Economics, France
REVIEW RETURNED	05-Apr-2016

GENERAL COMMENTS	<p>My answers to your responses are in italics below.</p> <p>1. Is it possible that a mortality bias affect your sample in a country where the AIDS epidemic has been disastrous? If the propensity to get infected by HIV is correlated with smoking through, for instance, risk preferences, then you will have a bias. You have to discuss this issue.</p> <p>Even if you cannot correct for this bias, you still have to mention it in the limitations of the paper. If the distribution of (risk) preferences has changed over time, then this may affect the price elasticity of your estimates. As a consequence, one has to be cautious in using these estimates to derive the benefits of price-based tobacco control policies.</p> <p>2. Price data (Pg7). Does the CSS report only the "average price of a pack of 20 cigarettes in the popular price range"? Or, does it also report the minimal price? My concern here is that smokers, especially young smokers, may easily substitute brands. Hence, the low price-elasticity may just reflect the fact that young smokers now start with lower quality brands than in the 70s.</p> <p>OK.</p>
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	<p>3. The endogeneity of the main policy variable has to be discussed.</p> <p>a. Prices may be endogenous, as producers set their prices as a function of consumer tastes and consumers' expected price response. Hence, unobserved taste variations may simultaneously affect the demand and the prices. One solution could be to use the excise tax as an instrument for the prices (using a 2 stage residual inclusion approach, see Terza et al., 2008).</p> <p>Your response 3a is not correct because your price effects are fundamentally derived by exploiting the variations in smoking and prices across time (and also provinces). It is the relationship between expected yearly/provincial smoking (once purged of the impact of individual covariates) and prices that matters. Tobacco producers set their prices partly as a function of consumer characteristics that are unobserved by the econometrician, and that may vary systematically from one year to another (e.g. the risk preferences mentioned in my first comment). Hence, either you try to implement the robustness test I propose, or you mention this as a potential limitation.</p> <p>b. Alternatively, I would be interested in seeing estimation results with the excise tax instead of the price.</p> <p>I am satisfied with your response. Note however that using the excise tax is a means of directly identifying the impact of public policies, without having to worry with producers' strategic price response to the policy. Recent papers using Empirical IO techniques illustrate this point, see Bonnet and Réquillart (2013) or Allais et al. (2015).</p> <p>c. Last, even the excise tax can be endogenous if it varies with some social norm that has a direct impact on demand. It is actually quite difficult to disentangle the impact of tax hikes from the impact of the information shocks (media coverage) that generally occur at the same moment.</p> <p>I do not fully understand your argument, but I am not asking you for further modifications to the text. You say on the one hand that "we do not believe that there is evidence that the excise tax is strongly correlated to social norms" and on the other hand that "South Africa was regarded as a world leader in using excise tax as a tobacco control tool". Does that mean that the government increased the excise tax for budgetary reasons only, but 'sold' this to the World as evidence of its commitment to tobacco control?</p> <p>4. Smuggling has to be discussed, as it may induce actual price variations across regions. Would it be possible that smuggling has increased over time?</p> <p>OK</p> <p>5. Is it also possible that South-African youths start smoking with other products than tobacco?</p> <p>OK</p> <p>6. Given the low rate of smoking initiation (39% for men, 11% for women), I am a bit surprised by the similarity of the results between the standard model and the split-population model. What are the predicted split probabilities? Is it also possible to estimate a model where the split probability would depend on the time-invariant covariates and the average price over the period of observation? See for instance Douglas & Hariharan (1994).</p> <p>The estimated split-probabilities are given by $e(\text{curep})$ after the estimation. To go further, I suppose that you would have to program the Douglas and Hariharan estimator. See Forster and Jones (2001).</p> <p>7. You should mention on Pg8 (Methods) that an individual drops from the sample once s-he starts smoking.</p> <p>OK</p>
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	<p>8. Is there only one observation per household in each regression? i.e. do you have households with two men/women (or more). In that case, you should cluster the standard errors at the household level. If clustering at the household-level does not change the results, why not doing it?</p> <p>9. You cannot control for other policies that occurred simultaneously. Hence, Pg. 9, you should mention that you probably identify only an upper bound on the impact of prices. I am satisfied with your response.</p> <p>10. The effects of parental education and parental smoking are interesting, but some third factors – for instance a gene-grounded propensity to act impulsively - may simultaneously explain parental education/smoking and individual smoking. You may perhaps mention this point in a footnote.</p> <p>11. You can update reference 20. I am satisfied with your response.</p> <p>References</p> <p>Allais, O., Etilé, F., & Lecocq, S. (2015). Mandatory labels, taxes and market forces: An empirical evaluation of fat policies. <i>Journal of Health economics</i>, 43, 27-44.</p> <p>Bonnet, C., & Réquillart, V. (2013). Tax incidence with strategic firms in the soft drink market. <i>Journal of Public Economics</i>, 106, 77-88.</p> <p>Forster, M., & Jones, A. M. (2001). The role of tobacco taxes in starting and quitting smoking: duration analysis of British data. <i>Journal of the Royal Statistical Society: Series A (Statistics in Society)</i>, 164(3), 517-547.</p>
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VERSION 2 – AUTHOR RESPONSE

Review for bmjopen-2016-011076.R1

Comment 2: omitted determinants of smoking behavior

My original comments called the author's attention to the fact that their identification strategy relies on variation in cigarette prices that is (mostly) temporal. I asked the authors to provide robustness checks to allay concerns that their result do not actually capture variation in other omitted variables that follow a trend similar to the trend in cigarette prices. They include per capita GDP as a potential omitted variable.

They write, "...we use GDP per capita as a possible variable which captures these unobserved determinants. We find that the correlation between the real price of cigarettes and the real per capita GDP is quite substantial. GDP per capita was decreasing in the 1980s, as was the real price of cigarettes, and from the mid-1990s both variables were increasing." According to them, the results of that model were: "... an increase in the real per capita GDP was associated with a decrease in smoking initiation." They also state: "Including the per capita GDP in the analysis reduces the impact of price on smoking initiation (for males), but not completely."

That is fine.

But they then reach for some argument to justify not reporting these results to the readers.

They write:

"This result is counterintuitive and probably spurious."

Where, "This result" refers to the finding that higher per capita GDP reduces (the probability of) smoking initiation.

They also state:

"There is a very large literature that indicates the positive relationship between income and smoking

behaviour. Thus, while we acknowledge that unobserved determinants of smoking behaviour could impact the results, the results need to be plausible. Per capita GDP is clearly not a plausible unobserved variable."

These statements do not make sense.

The authors defend themselves by citing a "large literature that indicates the positive relationship between income and smoking behaviour" but fail to note or distinguish the "large" literature showing the relationship between income (education) and smoking behavior is not constant over time - especially in developing countries.

It is unreasonable to assert that per capita GDP is not a "plausible" unobserved variable.

Therefore I would strongly suggest the following:

1. Report the sensitivity of their results when they include per capita GDP as an additional control variable (footnote or extra column in Table 2)
2. The authors should refer to the literature that shows that the relationship between smoking and per capita income and education vary across cohorts and time. The authors can find references to that literature in Christopoulou, R., Lillard, D. and Balmori, J. R. 2013. "Smoking behavior of Mexicans: patterns by birth-cohort, gender, and level of education." *International Journal of Public Health*, 58(3): 335-343.

Response: In our previous response we indicated that per capita GDP gave us counter-intuitive coefficients. Although we did not explain it at the time, we are of the opinion that per capita GDP is an inappropriate measure for the underlying growth in income for South Africa. South Africa has the highest income inequality in the world, with a Gini-coefficient of around 0.7. The Gini-coefficient has been increasing over the period of analysis. Even though South Africa is categorised as an upper-middle income country by the World Bank, only a small percentage of citizens are wealthy, with the majority living in poverty. A better measure of per capita income in South Africa would be median income, but this measure is not available on an annual basis in South Africa, especially not in the pre-2000 period. Between 1976 and 1994, parts of South Africa were not monitored because these were "independent" homelands.

If we were to use official per capita or median GDP figures, the data would be subject to significant shocks as these various homelands became "independent" (and their population was excluded from SA, and when they were incorporated in 1994. Given the increase in inequality, the average income in South Africa has grown far more rapidly than median income. For these reasons, per capita GDP in South Africa is a fairly meaningless indicator of income of the average/median person.

We considered using a consumer confidence index to capture a possible omitted variable, but this was not possible wither, since the South African confidence index (measured by the Bureau of Economic Research) does not cover the years that we need: for whites, the index goes back to 1975. Blacks were added to the sample in 1982 and the other racial groups in 1994. Instead we use gross domestic capital formation as a percentage of GDP as a measure of confidence as a proxy for confidence in the economy. We have added in this paragraph to the methods section:

"In order to account for the fact that there may be unobserved determinants of smoking behaviour, we considered including unemployment and per capita GDP in the regression equation.²⁶ However, given measurement errors over time in unemployment and per capita GDP, and the fact that per capita GDP is quite a distorted measure of average income in South Africa, we include a proxy for confidence in the economy, calculated as gross domestic capital formation as a percentage of GDP." The results are included in the regressions tables. An increase in confidence is associated with a decrease in smoking initiation for males and females (but only significant for males).

Comment 5 Sample restrictions

The authors response was find. I am not particularly persuaded by the logic they use to justify excluding observations before 1970 (especially because the results in the "full" sample (using observations from 1961 to 2012) are essentially identical to those from the restricted sample (using

observations from 1970 to 2012). Personally I would make the former the "main" results and refer to the latter results as robustness checks. But I don't feel strongly.

To clarify for the authors. Even using 1970-2012 as their starting year, the authors unnecessarily restrict their sample. (but only marginally)

To be at risk a person needed to be 10 years old in 1970. That means he would be 52 in 2012. They exclude people who are age 49-52 in 2012.

Response: We prefer to use the restricted sample (1970 – 2012) only as we are not confident in the accuracy of cigarette prices prior to 1970. The clarification on restricting the sample is noted. We have changed "at the time of the interview" to "in 2008" in 3 places.

Comment 6 errors associated with retrospectively reported data

6.1 Heaping - attenuation bias

The inclusion of the indicators for age 16 and age 18 are insufficient to control for the attenuation bias associated with heaping.

Instead one should include 1) dummy variable for age 16, 2) dummy variable for age 18, 3) dummy variable for age 16*cigarette price, and 4) dummy variable for age 18*cigarette price.

The coefficient on the interaction terms captures the attenuation bias from heaping.

Response: We have now included a dummy variable for age 16*cigarette price, and a dummy variable for age 18*cigarette price in all the models, except for model 1 for males, where the model would not converge. We also cluster at the household level, which we had not done previously (as per another reviewer's suggestion). The coefficients after these adjustments change marginally; these changes have been affected in the document.

6.2 Differential mortality of smokers and non smokers

The authors write: "While we agree that smokers are more likely to die than non-smokers, we are unsure how one would go about creating weights to adjust for this. Can the reviewer please provide some guidance as we have not come across this in the literature before."

I refer them to the paragraph they copied into their response: Christopoulou et al (2011), provide a statistical adjustment and/or evidence to support the use of a given age cutoff. I recommend the authors use the latter approach since differential mortality is unlikely to severely bias their results. Still, the authors should acknowledge the possible bias.

If they had read Christopoulou et al (2011) they would have understood the statistical adjustment and been able to download the stata code (and data) they need to implement it.

Response: We apologise for not reading the literature recommended carefully. We have done so now, and have included this sentence to the methods section, together with a reference: "By limiting the age cohort to exclude older generations, we reduce the differential mortality bias, since smoking-related mortality differences among a younger age cohort are small."²¹

21. Christopoulou R, Han J, Jaber A, et al. Dying for a smoke: How much does differential mortality of smokers affect estimated life-course smoking prevalence? *Preventive Medicine* 2011;52:66-70.

Comment: I would note: Reviewer 3's Comment 1 highlights raises an important and related point I hadn't considered in my previous comments. Since HIV prevalence was high in sub Saharan Africa, the question of differential mortality of smokers and non smokers may be especially important if smokers were more likely to develop HIV (because they are risk takers).

Response: We have added this to the limitations section: "Thirdly, we do not account for a HIV-related

mortality bias. In a country where the prevalence of the AIDS epidemic is high, it is possible that a mortality bias might affect the sample if the propensity of HIV infection is correlated with smoking through risk preferences.”

Comment 8 circle back to literature in Chaloupka and Warner 2000 and recent literature.
I am completely unsatisfied with the authors' response.

They write:

"While we agree with the reviewer, it is difficult to compare elasticity estimates from smoking initiation studies, primarily because different studies use different time origins (age at which one assumes a person is at risk of initiating smoking). In addition, even if one looks just at studies that use the same timer origin as we do (age 10), elasticity estimates are not always reported."

This response is a dodge. I continue to believe that this paper will be more useful if they place and discuss their findings in the context of previous studies. They can qualify that discussion in the above ways. I am unconvinced that they should be excused from adding this discussion simply because one or more of the published studies exclude 10-12 year olds while their does not.

Response: We have added this paragraph to the discussion section:

"We estimate the price elasticity of initiation is between -0.21 and -0.47 for males and -0.07 to -0.28 for females, depending on the specification. The price elasticity is significant for males only, a similar finding to Cawley, Markowitz and Tauras (2006). In general, our price elasticities of initiation are lower compared to other studies. Cawley, Markowitz and Tauras (2006) find a price elasticity of initiation of -1.2 for males.²⁶ Laxminarayan and Deolalikar (2004) consider smoking initiation among Vietnamese males and find a price elasticity of initiation of -1.18. Zhang et al (2006) find an elasticity of initiation with respect to cigarette price is -3.36 for males and females in Canada.^{6 27}"

A couple of minor comments:

Comment: They should cite my 2013 Journal of Health Economics paper because it isn't included in the 2013 Guindon review. (Lillard, D. R., Molloy, E. and Sfekas, A. 2013. "Smoking Initiation and the Iron Law of Demand." Journal of Health Economics, 32: 114-127.)

Response: We have added in this sentence to the introduction: "Two papers, which are not covered in Guindon's review, find that cigarette taxes and prices affect smoking initiation decisions in the US and in Canada."^{3 4}

3. Lillard DR, Molloy E, Sfekas A. Smoking Initiation and the Iron Law of Demand. Journal of Health Economics 2013;32:114-27.

4. Auld MC, Zarrabi M. Long-Term Effects of Tobacco Prices Faced by Adolescents. Forum for Health Economics & Policy 2015;18(1).

Comment: Throughout the paper they sometimes refer to data in the singular. Data are plural.

Response: Noted, and changes made accordingly.

PS Here is a brand new paper that might interest you:

A Novel Indicator of Life-Course Smoking Prevalence in the United States Combining Popularity, Duration, Quantity, and Quality of Smoking

<http://ajph.aphapublications.org/doi/abs/10.2105/AJPH.2016.303130?af=R>

Thank you for alerting us to this new publication. While interesting, we have not included a reference to this paper in our paper.

Reviewer: 3

Reviewer Name: Fabrice Etilé

Institution and Country: Paris School of Economics, France

Competing Interests: None

My answers to your responses are in italics below.

1. Is it possible that a mortality bias affect your sample in a country where the AIDS epidemic has been disastrous? If the propensity to get infected by HIV is correlated with smoking through, for instance, risk preferences, then you will have a bias. You have to discuss this issue.

Even if you cannot correct for this bias, you still have to mention it in the limitations of the paper. If the distribution of (risk) preferences has changed over time, then this may affect the price elasticity of your estimates. As a consequence, one has to be cautious in using these estimates to derive the benefits of price-based tobacco control policies.

Response: We have added this to the limitations section: "Thirdly, we do not account for a HIV-related mortality bias. In a country where the prevalence of the AIDS epidemic is high, it is possible that a mortality bias might affect the sample if the propensity of HIV infection is correlated with smoking through risk preferences."

Comment 3: The endogeneity of the main policy variable has to be discussed.

Comment 3a: Prices may be endogenous, as producers set their prices as a function of consumer tastes and consumers' expected price response. Hence, unobserved taste variations may simultaneously affect the demand and the prices. One solution could be to use the excise tax as an instrument for the prices (using a 2 stage residual inclusion approach, see Terza et al., 2008).

Your response 3a is not correct because your price effects are fundamentally derived by exploiting the variations in smoking and prices across time (and also provinces). It is the relationship between expected yearly/provincial smoking (once purged of the impact of individual covariates) and prices that matters. Tobacco producers set their prices partly as a function of consumer characteristics that are unobserved by the econometrician, and that may vary systematically from one year to another (e.g. the risk preferences mentioned in my first comment). Hence, either you try to implement the robustness test I propose, or you mention this as a potential limitation.

Response: We have added this to the limitations section: "Fifthly, the price coefficients may be biased if prices are endogenous. Producers may set their prices as a function of consumer tastes and consumers' expected price responses."

Comment 3b: Alternatively, I would be interested in seeing estimation results with the excise tax instead of the price.

I am satisfied with your response. Note however that using the excise tax is a means of directly identifying the impact of public policies, without having to worry with producers' strategic price response to the policy. Recent papers using Empirical IO techniques illustrate this point, see Bonnet and Réquillart (2013) or Allais et al. (2015).

Response: We present the estimation results for males below. Column 1a uses the price of cigarettes, while column 1b uses the excise tax of cigarettes. The coefficient on excise tax (0.956) is smaller than the coefficient on the price (0.986). This implies that impact of a R1 increase in the excise tax has a greater impact on the probability of smoking initiation than a R1 increase in the retail price.

Throughout the period the tobacco industry has over-shifted the excise tax, often significantly so. For example, between 1994 and 2010 the real excise tax increased by R6.84 (from R1.82 to R8.66), while the real retail price increased by R17.43 (from R7.66 to R25.09). Given the much larger increase in the real retail price than the increase in the excise tax, this difference in the coefficients on excise and price are in the appropriate direction.

The coefficients and the standard errors on the other covariates differ at the third decimal, if at all.

While we find these results interesting, we would prefer to report the coefficients of the price of cigarettes, since these are the prices that consumers are faced with.

(1a) (1b)

Logit Logit

Price of cigarettes 0.986***
 (0.003)
 Excise tax on cigarettes 0.956***
 (0.008)
 Rural 1.000 1.000
 Urban 1.224*** 1.229***
 (0.056) (0.056)
 Parents: Primary / no education 1.000 1.000
 Parents: Incomplete secondary education 1.050 1.050
 (0.051) (0.051)
 Parents: Complete secondary education 0.972 0.972
 (0.078) (0.078)
 Parents: At least some tertiary education 0.782*** 0.784***
 (0.077) (0.077)
 Illiterate 1.000 1.000
 Literate 0.628*** 0.630***
 (0.032) (0.032)
 Mother alive when respondent was 15 1.000 1.000
 Mother died before respondent was 15 1.075 1.070
 (0.101) (0.102)
 Neither parent has ever smoked
 Either parent was ever a smoker

Duration dependency (Temporal dummies) Yes Yes
 Duration dependency (Fourth order polynomial) No No
 Controls for race Yes Yes
 Controls for duration dependency & race group interactions Yes Yes
 Controls for heaping No No
 Number of person-period observations 96 298 96 272
 Pseudo R-squared 0.0995 0.100
 Price elasticity of initiation -0.19***
 Excise elasticity of initiation -0.20***
 Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

Comment 3c: Last, even the excise tax can be endogenous if it varies with some social norm that has a direct impact on demand. It is actually quite difficult to disentangle the impact of tax hikes from the impact of the information shocks (media coverage) that generally occur at the same moment.

I do not fully understand your argument, but I am not asking you for further modifications to the text. You say on the one hand that “we do not believe that there is evidence that the excise tax is strongly correlated to social norms” and on the other hand that “South Africa was regarded as a world leader in using excise tax as a tobacco control tool”. Does that mean that the government increased the excise tax for budgetary reasons only, but ‘sold’ this to the World as evidence of its commitment to tobacco control?

Response: In 1994, the government announced that it aimed in increase the total tax burden to 50% of the average retail price (at that point the tax burden was about 33% of the retail price). This decision was based on two reasons: (1) the government had been lobbied by a small group of public health advocates for more than a decade to increase the excise tax on tobacco for public health purposes. Although these advocates were very vocal, they did not have strong support from the public or the media at that point. The support for strong tobacco control legislation came later. (2) In the early 1990s the government of South Africa faced a dire fiscal situation with a very large budget deficit and rapidly growing government debt. In order to raise money, the government investigated all kinds

of revenue generating sources, of which excise taxes was one.

Comment 6: Given the low rate of smoking initiation (39% for men, 11% for women), I am a bit surprised by the similarity of the results between the standard model and the split-population model. What are the predicted split probabilities? Is it also possible to estimate a model where the split probability would depend on the time-invariant covariates and the average price over the period of observation? See for instance Douglas & Hariharan (1994).

The estimated split-probabilities are given by $e(\text{curep})$ after the estimation. To go further, I suppose that you would have to program the Douglas and Hariharan estimator. See Forster and Jones (2001).
 Response: The $\text{Pr}(\text{never start smoking})$ is 0.324 for males and 0.404 for females. Although we expected the probability of never starting to smoke to be higher for females, the estimate is in the right direction. We present the regressions below:

Comment 8: Is there only one observation per household in each regression? i.e. do you have households with two men/women (or more). In that case, you should cluster the standard errors at the household level.

If clustering at the household-level does not change the results, why not doing it?

Response: Since there is more than one observation per household, we have now clustered the standard errors at the household level except for the split population models where we get the following error message: "spsur_ll failed to compute scores required by the robust option". We also included a dummy variable for age 16*cigarette price, and a dummy variable for age 18*cigarette price in all the models (as per another reviewer's suggestion). The coefficients after these two adjustments change marginally. We have added this to the methods section: "Standard errors are clustered at the household level."

Comment 10: The effects of parental education and parental smoking are interesting, but some third factors – for instance a gene-grounded propensity to act impulsively - may simultaneously explain parental education/smoking and individual smoking.

You may perhaps mention this point in a footnote.

Response: We have added this to the limitations section: "Fourthly, it is possible that the effect of parental smoking may be attenuated by a gene-grounded propensity to act impulsively."

References

Allais, O., Etilé, F., & Lecocq, S. (2015). Mandatory labels, taxes and market forces: An empirical evaluation of fat policies. *Journal of Health economics*, 43, 27-44.

Bonnet, C., & Réquillart, V. (2013). Tax incidence with strategic firms in the soft drink market. *Journal of Public Economics*, 106, 77-88.

Forster, M., & Jones, A. M. (2001). The role of tobacco taxes in starting and quitting smoking: duration analysis of British data. *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 164(3), 517-547.

VERSION 3 - REVIEW

REVIEWER	Fabrice Etilé Paris School of Economics France
REVIEW RETURNED	03-Jun-2016

GENERAL COMMENTS	I do not understand your answer to my comment 8. If the command does not allow for clustering at the household-level, you can
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	bootstrap the estimates using Stata's bootstrap command with the option cluster(household_identifier). You will then obtain correct standard errors.
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VERSION 3 – AUTHOR RESPONSE

Please thank Dr Elite on our behalf for the advice. We have now bootstrapped the standard errors on the spsurv model and updated the estimates.

Correction: *Determinants of regular smoking onset in South Africa using duration analysis*

Vellios N, van Walbeek C. Determinants of regular smoking onset in South Africa using duration analysis. *BMJ Open* 2016;6:e011076. The corresponding author of this paper is 'Nicole Vellios' (not 'Dr Nicole Vellios').

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BMJ Open 2016;6:e011076corr1. doi:10.1136/bmjopen-2016-011076corr1



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