BMJ - Decision on
Manuscript ID
BMJ.2017.041510.R
1

Body:

07-Dec-2017

Dear Mr. Ulep

Manuscript ID BMJ.2017.041510.R1 entitled "The health, poverty and financial consequences of a cigarette price increase among 500 million male smokers in 13 low and middle-income countries"

Thank you for sending us your revised paper. We sent it back to some of the original reviewers and also sought additional expert opinions. These convince us that some important problems remain with the paper. We are not certain it is right for us, but after extensive discussion we elected to give you a chance to respond to the latest round of reviews.

We hope very much that you will be willing and able to revise your paper in response to reviewer comments so that we will be in a better position to understand your study and decide whether the BMJ is the right journal for it. We are looking forward to reading the revised version and, we hope, reaching a decision.

Yours sincerely,

Elizabeth Loder, MD, MPH
BMJ Editorial Team
eloder@bmj.com

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In your response please provide, point by point, your replies to the comments made by the reviewers, explaining how you have dealt with them in the paper.

** Comments from the external peer reviewers**

Reviewer: 1

Recommendation:

Comments:
I am happy with the revisions made which have greatly improved the paper.

Additional Questions:
Please enter your name: Christopher Millett

Job Title: Professor of Public Health

Institution: Imperial College

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

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Reviewer: 2

Recommendation:

Comments:
The authors have address my minor comments. I have no additional comments.

Additional Questions:
Please enter your name: Eric Leas

Job Title: Posdoctoral Fellow

Institution: Stanford Prevention Research Center, Stanford Medicine

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

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Reviewer: 3

Recommendation:

Comments:
I applaud the careful and thorough application of methods and literature to an important problem. The paper clearly demonstrates the potential for large decreases in smoking and the associate health and financial costs from a 50% increase in cigarette taxes for the low to middle income countries of the world. However, I have one fundamental problem with the analysis.

* Fundamentally, from the perspective of a smoker, economics tells us that a price increase is followed by a reallocation of resources. For some smokers, a 50% increase in taxes would move them to a corner solution with respect to smoking (i.e., they quit), and some, as you state in the paper, would reduce their smoking but not quit. In both cases, the smoker substitutes towards other goods but is effectively poorer (the income effect). How smokers reallocate is crucial to long-term health, medical, and financial considerations. If smokers reallocate to other vices (e.g., binge drinking), then the results of this paper are overstated to the extent that binge drinking causes excess mortality and increased medical expenditures. If smokers reallocate to healthier choices, then the results are understated. No evidence on the substitutability of other forms of health investment are considered in the projections of this paper.

* A 50% increase in cigarette taxes is large, and the estimates of price elasticities used in this paper come from studies that actually observe much smaller increases. Put differently, we have little evidence on how smokers would respond to such a large tax increase. It is reasonable to think that such an increase in taxes may lead to a black market for cigarettes, which would imply fewer smokers actually quitting than the assumed elasticities.

* Furthermore, there are good reasons to believe that the biological effect of smoking on longevity is overstated by as much as 50%. Correlated behaviors and confounding factors matter, but Doll (2004) ignores them. The results in Doll 2004 are conditional on only age and birth cohort. Darden, Gilleskie, and Strumpf ("Smoking and Mortality: New Evidence from a Long Panel" - Forthcoming in the International Economic Review) show that failure to account for correlated behaviors and confounding factors leads to biased estimates of the effect of smoking on longevity.

*Recent evidence from the US using the TUS data suggest that the price elasticity of demand for cigarettes is between -0.02 and -0.05, i.e., very inelastic (See Callison and Kaestner, 2014).

* The relative effects of a 50% tax increase by income are potentially interesting because the results suggest that low income individuals receive the greatest benefit, but these results are by assumption given the parameters used. I do not see the value added from this paper.

* Many smokers want to quit. Some do not. For those who do not want to quit, a 50% tax increase is not welfare improving.

* It is claimed that smoking cessation rates are low in LMICs are low, but most smokers are below the age of 35 in these countries, and the longevity effects in Doll (2004) are not noticeable until age 50. This leaves a long time for smokers to quit. Perhaps the cumulative amount of cessation is not trivial?

Additional Questions:
Please enter your name: Michael Darden

Job Title: Associate Professor

Institution: George Washington University

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

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Reviewer: 4

Recommendation:

Comments:
Using available estimates from many low/middle income countries, this paper simulates the gains in population health and national revenue, and the reduction in the risks for impoverishment due to smoking by tobacco tax increase.

The main purpose of this paper is not clear. For example, in abstract, conclusion "higher tobacco taxes support SDG targets on NCDs, poverty and financial protection against illness": this is already mentioned in Objectives as "Higher tobacco excise are required to achieve SDG targets." So the conclusion is a priori.

Because the associations between tobacco tax increase and the outcomes evaluated have been already established by previous studies. So the primary purpose of this paper is to provide the simulated results of the actual magnitudes of the impacts. However, the estimated values presented, eg 15.5 million men who can avoid catastrophic health expenditures due to 50% price increase, are not intuitively reflect the magnitudes of the impacts, as the authors selected specific 13 countries and I cannot understand the representativeness of these 13 countries in the world. So I think the estimates should better be presented as rates by country or by different country income levels is more useful. For example, the conclusion like "among low income countries, on average the 50%

tobacco price increase could be linked to the XX% reduction in the impoverished people." This kind of information may be useful when considering health policies in each nation.

Page 3: "the impact of a practicable 50% cigarette price increase": Please explain more about why the authors think that 50% increase is practicable. Practicability may vary across nations because of many conditions including policy conditions, baseline prices and so on.

Please explain the key procedures and concepts of extended cost effectiveness analysis.

The GATS and "similar nationally representative surveys" are the primary data used to estimate in this paper. Please provide more information with which readers can evaluated the accuracy and external validity of them.

The estimates have the assumptions: quitting is only a function of price elasticity of demand for cigarette, age and income. Please justify more about this assumption. To my knowledge, quitting can be strongly influenced by the levels of nicotine addiction, social influences, social norms for tobacco smoking, physical accesses to tobacco, and baseline tobacco prices and many other factors. Actually as far as I see Appendix page 13, baseline price is used for the estimation: the equation had "price").

Treatment cost should vary across countries. Did the authors consider the variation when calculating annual treatment costs?

In Turkey, the price is already $10.30. Is it realistic to increase the price 50% more?

For many countries income quintiles are defined by the variables other than income such as education. How valid is this methods? What is the population share of each quintile category? How balanced?

Many of discussion part does not stem from the findings of this study but explains the potential policy implications.

Page 14: discussion on the impact of price increase in reduction of addiction is far from the arguments potentially derived from the results of this study.

Some estimates for Armenia is very small and not understandable. Some tables omit the Armenian data without any explanation.

I found some typos:
Line 14 in Abstract: two "was"
Footnote of Table 1

Additional Questions:
Please enter your name: Naoki Kondo

Job Title: Associate Professor

Institution: The University of Tokyo

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

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Reviewer: 5

Recommendation:

Comments:
November 18th , 2017
Managing Editor
BMJ

Dear colleagues,

I am writing to review "The health, poverty and financial consequences of a cigarette price increase among 500 million male smokers in 13 low and middle-income countries," manuscript ID BMJ.2017.041510.R1.

The paper is an ambitious large-scale estimate of how a 50% increase in cigarette prices across the largest developing countries would result in 450 million years of life gained (mostly in China, and disproportionately among poorer people), $157 billion in averted treatment costs, reductions in catastrophic health expenditures, and increases in tax revenues from tobacco (most of which come from richer people). The authors argue that the decrease in catastrophic health expenditures among near-poor lead to reductions in poverty and therefore contribute towards achieving poverty reduction goals.

Specific comments are below, but the main revision that I would request is with regards to the conclusion on poverty reduction attributable to the increased tobacco taxes. As I explain in point #4 below, I would argue that the current conclusion with regards to poverty rates is incorrect and needs revision. In addition, the apparent assumption that excise taxes are entirely passed through to consumers through higher prices needs justification or revision (see point #2 below). Finally, I would request that the authors make an attempt to incorporate quantitative evidence on cigarette tax avoidance to try to at least give an order-of-magnitude estimate on how avoidance affects the conclusions on both health and tax revenue outcomes.

The paper will help stimulate an important discussion on the broad-based benefits of substantial increases in cigarette taxes. With some modifications, I recommend the paper for publication. The quantitative analysis is sound (even if I disagree with the framing of one of the conclusions), the piece is well written, and the topic is important and of general interest to the health and development community.

More specific comments are as follows:

1) The issue of tax compliance is not mentioned until the end of the paper (page 12). Are there quantitative estimates of cigarette tax compliance (ideally as a function of country income and tax level) which might be used to enrich the model and not assume 100% compliance?

2) Page 7 and Table 1: "The absolute increase in the median excise tax needed to achieve a 50% price increase was … $1.10 in Colombia…". Given that Colombia's price is $2.20, this paragraph makes it seem like the analysis is assuming that cigarette producers will pass the entire tax to consumers (so that a 50% price increase comes about from an excise tax equal to 50% of the price). This needs justification, since microeconomic principles suggest that the tax incidence will be borne by both consumers and producers, and a 50% price increase will require an excise tax larger than 50% of the price. The exception is if you have very inelastic demand (in which case the producer can pass nearly all the cost of the tax to the consumers). The numbers in this paragraph suggest that taxes equal 50% of prices throughout, whereas it seems the tax would have to be calculated taking the demand elasticity into account.

3) I see that one of the robustness tests is to use country-specific elasticity estimates instead of a global average of -0.4. The text needs to better explain why country-specific elasticities are not the preferred model. Is the downside that the confidence in these country-specific elasticities is low?

4) Page 10: The connection between tax hikes and reduction in income poverty does not make sense, in my view. The argument in the paper is that tax hikes lead to smoking cessation, and among the near-poor that means avoided catastrophic OOP costs. The authors then suggest that since income level net of these catastrophic costs is below the poverty line, we can call this a reduction in the poverty rate. There are (at least) two issues with this conclusion. First, poverty lines are based on income, not income net of health expenditures. That means that on a purely technical level, these statements are not correct. Secondly, averted OOP costs are one-time effective boosts in income, not a permanent increase in income and therefore not a permanent decrease in the country's poverty rate. One could make the argument that decreased smoking will increase incomes by increasing productivity, but that is indirect and not the argument being made in this section of the paper.

5) Page 11: "… practicable tax hikes could avoid about 2.4% of the income poverty by averting OOP treatment costs." Following my comment above, this sentence seems to be wrong both technically and confusing one-time income shocks with changes in poverty rates.

6) The authors admit that their analysis only looks at the extensive margin (cessation). However on page 5 the paper mentions that there are studies indicating that "about half (10%) [of reduced smoking] is attributable to quitting by current smokers and half to fewer cigarettes smoked." The paper could better explain why this fact is not incorporated to study the intensive margin (cigarettes smoked among non-quitters), and its consequences on health and expenditures on the tax among poor and non-poor.

7) Following the previous comment, it would seem that incorporating the intensive margin would make the findings even more pro-poor in that the non-quitters in the bottom quintiles would reduce consumption due to the taxes, thus making the tax even less regressive, and increasing the benefit of the tax in terms of health. The estimated tax revenue, however, would be smaller if one assumes the non-quitters will scale back consumption.

8) Page 12: I appreciate the authors mentioning tax compliance issues, but these seem pretty central to the quantitative conclusion and I would like to see an enrichment of the quantitative model based con observed compliance around the world (at least as a robustness check). Given that these will reduce the health and revenue benefits, giving a magnitude of the effect is important for the conclusions of this paper.

9) Pages 12 & 14: The conclusions and implications could say more about other benefits of the tax that are not quantified in this paper. These include reducing in second-hand smoke injuries to family members and other people, and environmental damage from cigarette butts that is being quantified in other studies (Slaughter E, Gersberg RM, Watanabe K, et al Toxicity of cigarette butts, and their chemical components, to marine and freshwater fish Tobacco Control 2011;20:i25-i29.).

Smaller comments:

1) Page 5: Typo "We excluded the marginal health benefits ACCRUED"

2) Page 5: Typo "…due to of fewer cigarettes…"

I hope these comments are useful to the authors in their revision process,

Gordon C. McCord
University of California, San Diego

Additional Questions:
Please enter your name: Gordon C McCord

Job Title: Assistant Professor

Institution: University of California, San Diego

Reimbursement for attending a symposium?: No

A fee for speaking?: No

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Reviewer: 6

Recommendation:

Comments:
This paper calculates various impacts of a 50% increase in cigarette prices in 13 low and middle-income countries. The main outcome measures are life years gained, averted treatment costs, catastrophic healthcare expenditures and poverty, and tax revenues. The outcomes are calculated by income quintiles.

The study does not conduct original empirical research into the impact of taxes on any of the outcomes. Instead, it pieces together estimates and assumptions about the price elasticity of smoking with estimates of the number of smokers, epidemiologic estimates of the life years lost from smoking/ life years gained from cessation, treatment costs, and catastrophic healthcare expenditures. Obviously, a standard-length medical journal article cannot be expected to provide a complete discussion of all of the inputs into these complex calculations. However, it is crucially important that the analysis uses a sound conceptual framework and carefully explores the limitations of the evidence-base for the calculations. I have a number of concerns about the conceptual framework and the evidence base.

Conceptual framework

The study conducts what it calls an "extended cost-effectiveness analysis." The paper fails to explain how this method is related to the standard conceptual framework for cost-effectiveness analysis (CEA) of interventions in health and medicine. The lack of a conceptual framework from CEA (or some other source) raises a number of important concerns.

Standard CEA compares the incremental opportunity costs of the resources used in an intervention to the incremental health gains (life years gained or QALYs gained). There are a number of standard references for CEA, including the report of the Second Panel on Cost-Effectiveness in Health and Medicine (Neumann et al., 2017, Oxford University Press). Welfare economics provides the conceptual foundation for CEA as a tool to determine whether societal resources are in their most highly valued use. (For more discussion, see the chapter by Meltzer, Basu and Sculpher in the Second Panel's report).

The study fails to identify or discuss the perspective of its analysis. To a large extent, but not always, it seems to adopt the societal perspective. The study should clarify its perspective. It would also be quite helpful to follow the Second Panel's recommendation and develop an Impact Inventory "which lists the consequences – including health and non-health consequences – across all of the sectors (e.g. healthcare, education, criminal justice system) affected by an intervention." (Owens et al. Second Panel's report, pp. 76-77). The study should

then identify which of the impacts it considers in its analysis and which it omits, and then explain the conceptual basis for those choices.

It is difficult to fit a tax increase into the CEA framework, but these conceptual problems are not discussed in the paper. Administering and collecting taxes involves relatively low resource costs, yet a tax increase the size considered in this study has enormous financial implications. As a first approximation, the cigarette tax increase transfers money from smokers to others in society, for example if the new cigarette tax revenues allows other tax rates to be lowered. The Second Panel on CEA recommends that: "Costs that are transferred from one section of the population to another should not be included." Following this recommendation, cigarette tax revenues should not be included because they represent such a transfer. On pages 215-16 of the Second Panel's report, Basu provides more discussion.

The study fails to conduct a standard analysis of the distributional consequences of the tax increase. The study estimates that the tax increase imposes a new tax burden of $122 billion. $15 billion of the new tax burden falls on the lowest income quintile and $29 billion of the new tax burden falls on the highest income quintile. The standard method to determine the vertical equity or regressive/progressive features of a tax system compares the tax burden to income. The average income of the top income quintile is much more than double the average income of the bottom income quintile. As a result, the $15 billion burden on the bottom income-quintile is a higher percentage of that group's income. In short, the study's estimates imply that the cigarette tax increase is regressive. A comment below discusses different plausible assumptions about how the price-elasticity varies across income groups. Under these assumptions, the tax increase is even more regressive. For more discussion of the distributional consequences of cigarette tax increases, see Colman and Remler (Journal of Policy Analysis and Management 2008).

The study fails to consider important impacts of the cigarette tax increase. The second Panel recommends that "All resources within the formal healthcare sector that lead to total healthcare costs should be accounted for over the lifetime of the lifetime of the patients under each intervention." (Basu, p. 206). In the study's analysis, the intervention is the tax increase, which is being compared to the alternative, which is the status quo of current taxes. Basu's discussion explains that healthcare costs should be accounted for during the additional life-years produced by the intervention over the alternative.

By the same reasoning used in the second Panel's report, the study's calculations of catastrophic health care expenditures and poverty should also consider what happens under the alternative, including what happens during the additional life-years produced by the tax increase. For example, the tax increase will reduce the number of people who suffer catastrophic health care expenditures due to smoking-related diseases. But in the counter-factual world with the tax increase, some of those people will suffer catastrophic health care expenditures due to non-smoking-related diseases. The study should calculate the net reduction in catastrophic expenditures, or at least acknowledge this an important limitation.

The study fails to follow the second Panel's recommendation that: "An important part of accounting for net future costs is to also account for the non-healthcare consumption costs during the added years of life." (Basu, p. 213)

The time frame of the analysis is somewhat unclear. As I understand it, the study considers a tax increase in the current year that increases smoking cessation in the current year, which yields a flow of life years gained, averted treatment

costs, and reductions in catastrophic healthcare expenditures in the current year and in the future years. The analysis of tax revenues apparently only considers the current year. Standard guidelines for CEA and CBA recommend that future consequences – including the health gains as well as healthcare expenditures – should be discounted to their present value. The study does not discount future health gains or expenditures.

In sum, the paper's method does not appear to share the conceptual foundations of CEA, and it fails to follow well-established guidelines for CEA studies. For these reasons, calling the method "extended" CEA seems misleading. It probably makes sense to use another term. The paper simply conducts a policy analysis of SOME of the consequences of the tax increase it considers. Whatever the analysis is called, an important drawback is that is seems to lack a conceptual framework to guide which consequences it considers and which consequences it ignores.

Evidence base

The paper fails to acknowledge key gaps and uncertainty in the evidence base of estimates and assumptions about the price elasticity of smoking.

The study begins with an estimate that the price elasticity of cigarette demand is -0.4. This is the median from a very wide range of estimates from a meta-analysis; in that meta-analysis the standard deviation of estimates is 0.43 and the estimates range from -3.12 to +1.41. The meta-analysis is from 2003, so the studies included are mainly from the 1980s and 1990s and some date back to the 1950s. Most of the estimates are for the U.S. and Europe. These limitations raise serious doubts about the generalizability of this evidence to low- and middle-income countries (LMICs) in 2017. Another concern is that insights from modern applied econometrics cast doubt on whether many of these older studies credibly identified the causal effect of higher cigarette prices on smoking. A counter-example that addresses many of these concerns is the study by Lance, Akin and Dow (Journal of Health Economics 2004). This study exploits a credible source of variation in cigarette prices and finds that cigarette smoking in China and Russia is almost perfectly inelastic. More generally, the recent NCI monograph (#21, The Economics of Tobacco and Tobacco Control, 2016) notes that price elasticity estimates in studies of LMICs "vary considerably" and "many … found very little impact of price on smoking prevalence." (p. 137)

The study next assumes that half of the -0.4 price elasticity reflects smoking cessation. The basis for this assumption is not clear. I am aware of a few estimates (from U.S. studies) that suggest that about half of the price-elasticity of cigarette demand reflects the elasticity of smoking participation. However, smoking participation reflects both initiation and cessation. The study needs an estimate of the elasticity of smoking cessation to link with its estimates of life years gained and other outcomes. If some of the effect of the tax increase operates through reduced smoking initiation, the lack of discounting noted above becomes even more important, because the health gains from reducing current initiation occur decades in the future.

The study next assumes that there is strong gradient where the price-elasticity is larger (in absolute value) for lower-income groups. The Gallet and List meta-analysis is cited as supporting an income gradient in price elasticity estimates, but this is not correct – the meta-analysis does not consider this question. The recent NCI Monograph (2016) discusses a number of studies that do not find an income gradient in price-elasticity estimates and calls the evidence from LMICs on this question "mixed" (p. 576)

Another major limitation is that the study implicitly assumes that in response to the substantial tax increases there will be no smuggling or any other form of tax avoidance and evasion. A recent monograph by the Institute of Medicine/ National Research Council cites an estimate that 12% of global cigarette consumption is illicit and reflects smuggling, tax avoidance, or tax evasions. It is very likely that the large tax increases needed to increase cigarette prices by 50% would increase the size of the illicit cigarette market and blunt the effects of the tax increase on life years gained and treatment costs averted.

Given the substantial uncertainties in the evidence base, the study should follow standard practice and conduct and report more sensitivity analyses. The analyses should show how the results vary: when the average price-elasticity is substantially smaller (in absolute value) than -0.4; and when there is a weaker or non-existent income gradient in price elasticities.

Style and other comments

The first sentence of the paper is dramatic but wrong. In high-income countries, many smokers quit – for example in the U.S. there are more former smokers than there are current smokers.

The last sentence of the second paragraph claims "high excise taxes are underused in nearly all LMICs." In terms of scientific content, the term "underused" is meaningless: there is no scientific way to determine the "correct" level of excise taxes. In terms of policy analysis, the claim is premature. It should be the point of a careful policy analysis to determine whether or not higher taxes might be a desirable public policy. Instead, this paper seems to assume that we already know that higher taxes are desirable, which makes the point of the paper appear to be advocacy.
In general, unless the paper is intended as an editorial, it should be written more objectively and strive to avoid advocacy.

Additional Questions:
Please enter your name: Don Kenkel

Job Title: Professor

Institution: Cornell University

Reimbursement for attending a symposium?: No

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A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

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