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Dr. Fiona Godlee
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Dr. Fiona

Thank you very much for reviewing our manuscript. We also greatly appreciate the reviewers for their comments and suggestions. We have revised the manuscript accordingly.

Please find attached a point-by-point response to reviewer's concerns. We hope that you find our responses satisfactory and that the manuscript is now acceptable for publication.

Sincerely yours,

Valerie Ulep (in behalf of other co-authors)

We attempted to address all the comments and suggestions of reviewers. However, we have identified three major comments that needed thorough attention and discussion in the paper.

R1: Concerns about the elasticity estimates
Two of the reviewers expressed concern about the validity of elasticity estimates used in our model. According to them, we may have overestimated demand elasticity, and generally assumed that there is income gradient (i.e. poor are more responsive to price change than the rich). The reviewers thus believe that we may have overstated the health and financial gains of higher cigarette prices.

There are hundreds of demand elasticity studies of tobacco from different time periods and settings published. The quality of these studies varies depending on the available data and empirical strategy. We acknowledged the heterogeneity of elasticity estimates in the literature. However, scientifically most robust method is to take into account all studies, with some consideration for study quality.

If we assess available empirical studies, price elasticity estimates range from -1.12 (i.e. studies in Myanmar) to -0.02 (i.e. studies in China). However, based on the review conducted by the International Agency for Research on Cancer (IARC), most elasticity estimates fall within the range of -0.2 to -0.6 (average of -0.4) (see IARC page 97), which is the point estimate we used. Most empirical studies are largely coming from the United States and high-income other countries. However, these studies imply that tobacco products in LMICs is at least as sensitive to price, and often more sensitive to price, than it is in high income countries. Further, the 2016 National Cancer Institute (NCI) Monograph on Tobacco Control (Series 21) also corroborates this assumption stating that while estimates for high-income countries are clustered around –0.4, price elasticity estimates for LMICs are more variable and somewhat greater in absolute terms (further from zero), with estimates clustered around –0.5. If this is case, the -0.4 elasticity estimates may be conservative.

The poor being price responsive has strong theoretical foundation (i.e. rational addiction models). This is also supported consistently by empirical evidence in HICs and is well documented in IARC and the NCI Monograph. In LMICs, there are settings where results are less conclusive perhaps due to limitation of data and problems in measurement (e.g. socio-economic status is not well defined or used inconsistently), but it is incorrect to argue that income gradient as assumed in the study neither has theoretical nor empirical basis. We expanded the methods and discussion (page 4) to further address the issue.

R2: Cost effectiveness analysis vs. Extended cost-effectiveness
One of the reviewers discussed the fundamental differences between the traditional cost effectiveness (CEA) and our methodology - "extended cost effectiveness (ECEA)" at length. We concur with the reviewer's observations. Nonetheless, we highlight the policy and methodological contributions of our paper. Our study models the distributional impact of increasing cigarette prices on health and poverty, which to our knowledge, has not been studied and explored extensively in LMICs. We are explicit about the limitations of our model, and its underlying assumptions. Our study should spark debate, and encourage methodologists and economists to further refine the method, and to address its caveats, which we and the reviewer acknowledge. We have removed statements or descriptions that refer to the traditional cost-effectiveness model. We also strengthened the limitation section (page 14) to reinforce the need for future research work.

R3: Extending the model
Reviewers have suggested refining the model to incorporate the following: a.) intensive margin (i.e. benefits of reduced smoking); b.) a scenario where the tax incidence is borne by both the consumer and producer; c:) illegal activities

R3a. Reduced smoking among non-quitters
We acknowledged this caveat in the limitation section. Our study only looks at full cessation, and not the benefits accrued from reduced smoking. Epidemiologic data on the health gains of reduced consumption remain controversial. Evidence that reduced smoking decreases the harm from smoking is suggestive, but not conclusive. We focus on cessation, which is more conservative than considering both cessation an lower amount.

R3b. Tax incidence that is borne by both the consumer and producer
Our parameters suggest the demand elasticity is not highly inelastic. Thus, the consumer will not solely bear the taxes, and it may partly be shared by the producer. In reality, given the oligopolistic nature of the cigarette industry, tax hikes may in fact lead to even higher consumer prices, as shown by Jeffrey Harris (http://web.mit.edu/jeffrey/harris/Harris_ACS_Report_May_1998.pdf).

R3c. Illegal activities
Two of the reviewers suggested incorporating the effects of tax non-compliance, smuggling and illegal activities in the model. However the totality of the evidence suggests that higher taxes still work in the presence of smuggling. We have added more references and a new box on common misconceptions about smuggling.

Reviewer 3 (Michael Darden)
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.

R4: We disagree. There is strong evidence that tobacco and alcohol (or possibly other addictive substances such as cannabis) are complementary goods. A decrease in the consumption of tobacco, could also lead to decrease in the consumption of alcohol not increase (see cross-price elasticity studies of tobacco and alcohol such as Decker and Schwartz, 2000, Cameron and Williams, 2002). If this is the case, the health effects of decrease tobacco consumption would actually understate our estimates not overstate as the reviewer suggests. Hence, there is a bigger health benefit of tobacco taxation (see discussion section).

Furthermore, studies suggest, at least in LMICs, that tobacco consuming households had lower consumption of certain health improving items such as food and education, and this is stronger among mothers who are smoking. In developing countries, where most people are constrained by money, it is reasonable to expect that the expenditure on tobacco will have higher opportunity cost in terms of reduced expenditure on basic goods such as food, education and energy over and above the direct health consequences of tobacco consumption (John, 2008).

At a bare minimum, it would be inconceivable that the mix of goods and services bought with funds freed up from tobacco would not be better for health than the one consumer product that kills at least half of its consumers when used as directed.

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.

R5: A 50% increase in tobacco prices in many LMICs is very realistic especially for countries where prices are low to begin with. Countries like the Philippines and Thailand, for example, increased the prices of cigarette prices by more than 100%. More than 100 empirical studies (including LMICs) demonstrate that tobacco taxes are powerful tool for reducing tobacco consumption, and provides continuing source of revenue. We have added additional references that suggest an increase is practicable.

In some instances, consumption of smuggled goods will go up with higher tobacco prices. Hence, we might have overestimated the health and financial gains. In HICs, the share of smuggled cigarette on total consumption remains fairly low. In LMICs, the share varies by each country (range: 3-15%). We argue however that increasing the price of cigarette will not significantly reduce the health and financial gains because of smuggling. Legal and smuggled cigarettes are not perfect substitute, as there is transaction cost in consuming illegal products. There is also ample evidence that even in the face of moderate smuggling, marginal prices on both official and black markets go up as do revenue, which shows that increases in official and black market go up as do revenues. We have added Box 1 with these key messages and additional references.

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.

R6: We think that the reviewer has misread our paper and not understood the landmark study of Doll et al followed. In that study the British doctors were quite homogenous (most likely similar socio-economic status, educational attainment, cognitive ability) with little differences between smokers and non-smokers in moderate alcohol use. Hence, additional adjustment made no material impact on the smoker: non-smoker mortality ratios. All the studies included in our study for mortality impact and benefits of cessation are done by competent epidemiologists and do take confounding with alcohol, obesity, education etc into account.

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).

R7: See Reply 1. Economists and epidemiologists should use the totality of the evidence, and not cherry-pick some studies.

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

R8: We agree to some extent and have already addressed this issue in limitations in the study. We did not consider the benefits within the utility maximization framework (or welfarism). However, it is not only legitimate but important to consider health gains and equity from a societal point of view, and even more so from a household point of view, when confronted with dilemmas in public policies. There is an extensive literature on extra-welfarism or the consideration of health gains and not mere utility gains (see Wagstaff and Culyer, 2008; Hurley, 2010). Furthermore, the effect of increased taxes on poor smokers who do not quit is likely to be outweighed by benefits to those who do and, importantly, to their households – who, among other negative effects of smoking, face economic disaster from the loss (beyond OOP medical expenses) in income of the illness and death of breadwinners/wage earners.

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?

R9: The few smokers in LMICs who quit do so up until about age 50 or so. The use of the spline methods takes into account the smaller absolute benefits of cessation at younger ages.

Reviewer 4 (Naoki Kondo)

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.

R10: We agree, and have changed altered the tone of the paper.

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.

R11: We have estimates per country as the goal was to asses a large price increase in diverse settings in terms of UHC, economic circumstances and smoking behaviour. We have now added on page 4 the justification for selecting these 13 countries. However, it is still make sense to discuss the impact by aggregate because we are almost covering about half of global smokers (500 million out of ~1 billion).

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.

R12: Tobacco taxes in most LMICs are relatively low and much below the recommended WHO- FCTC and World Bank-recommended levels. To meet these global standards, large tax increases are imperative. A 50% increase in tobacco prices in many LMICs is very realistic especially for countries where prices are low to begin with. Countries like the Philippines and Thailand, for example, increased the prices of cigarette prices by more than 100%. Our sensitivity analysis includes two other alternatives i.e.25% and 100% price increase. We have already revised our discussion on page 12 to address this issue.

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

R13: In line with the standard formatting guidelines of the journal, we have the detailed explanation of concepts and methods in the appendix.

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.

R14: We have included the names of surveys in Colombia, Philippines and Chile which are now cited in the Appendix.

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").

R15: Our study is a simulation to show the effect of price increases, controlling for age and income. The point here is to look at the effect of increasing tobacco taxes, holding slower-changing things like social norms and

influences constant. We initially, said "quitting is a function of price elasticity, age and income" at a purely mechanical level because these are the three parameters needed to estimate the number of quitters. [Quit=F (Elasticity, Age, Income)]. We have revised the methods for clarity.

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

R16: Yes, this is considered. Treatment cost by country and illness are considered and their sources are documented in the appendix.

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

R17: It is beyond the scope of the study to examine the political feasibility of the price increase. This is a simulation. It may or may not be politically feasible to do all at once, but it would be highly desirable. And e.g. Ukraine is raising (real) prices by more than 50% over a 2year period.

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?

R18: Using assets and education as proxies in some cases is justifiable, given the lack of other data, because of the high correlation between education and income. This is the best thing we can do given the lack of income variable in GATS. Asset index is a strong predictor of income. In theory, there are differences of the two. For example, income is responsive to shocks, assets are more stable. To test this, we examined the GATS Mexico data which collected both income and asset index. The prevalence of smoking by income quintile and education quintile is approximately the same.

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

R19: We revised the discussion section to better reflect our findings.

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.

R20: We respectfully disagree. The model is likely conservative, and we are learning more and more about the behavioural economics related to nicotine addiction. Hence, the impact of large price increase on poverty as well as consumer behavior is likely to be large.

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

R21: We discuss why countries like Armenia are not included in the table in the footnotes.

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

R22:. Thank you. Corrected.

Reviewers 5: Gordon C. McCord

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:
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?

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.

R23: We have already discussed the issue of compliance as raised by the first reviewer (see R2).

On the second point, our parameters suggest the demand elasticity is not highly inelastic. Thus, the consumer will not solely bear the taxes, and it may partly be shared by the producer. However, our estimates are looking at market demand side, i.e. changes in consumption due to a price increase faced by the consumer at the retail store level. To achieve a 50% increase in market price, the tax increase will be higher than 50%. We have strengthened the method section and results section for clarity about the pass through.

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?

R24: Unlike in setting like US and other HICs, there is intra-country variation of elasticity estimates from LMICs. Elasticity estimates vary depending on the statistical method and dataset used hence the quality of elasticity estimates is quite mixed. To address this problem, we decided to highlight pooled estimates from systematic reviews. This also yields more comparable estimates across the 13 countries. Moreover, we show in the sensitivity analyses that there is little difference using local PEs. The main issue remains price responsiveness of the poor versus rich smoker.

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.

R25: Thanks for the comment. We partly agree with the comment and have revised the manuscript to be clearer about the way we have calculated people averting poverty. For our poverty analysis, we only consider individuals who are above the poverty line. Further the estimate of number of smokers averting poverty is calculated by counting the individuals who would have fell below the poverty line without a price increase otherwise but are prevented due to quitting by saving the OOPs.

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.

R26: While we agree that basic income is usually used to determine poverty lines, in many LMICs expenditure is also used to determine standard of living index as well as national poverty lines. This is in part as self-reported income is less reliable than household expenditure, and due to lack of linkage with tax records. The net disposable income available for non-medical consumption is altered with higher medical expenses. Therefore, if there is a large medical cost incurred, it may push the individual into poverty. Indeed, WHO has carefully examined the mechanisms that lead to catastrophic health expenditure.

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.

R27: We agree with the comment and have revised the results and discussion sections to reflect the one-time benefit.

Page 11: "… practicable tax hikes could avert OOP treatment costs in an amount that would de facto avoid about 2.4% of income poverty." Following my comment above, this sentence seems to be wrong both technically and confusing one-time income shocks with changes in poverty rates.

R28: Thank you for your suggestion. We have changed the wording to emphasize the reduction in OOP expenditures.

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.

R30: See Reply 3
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.

R31: See Reply 3

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

R32: Thanks, see Reply 3 and Box 1.

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.).

R33: We included a statement of understated benefits because of negative externalities averted.

Smaller comments:
Page 5: Typo "We excluded the marginal health benefits ACCRUED"
R34: Thank you! Revised.

Page 5: Typo "…due to of fewer cigarettes…"
R35: Thank you! Revised.

Reviewer 6: Don Kenkel

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).

R36: See Reply 2.

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.

R37: We are purposely limiting the benefits we are considering for three reasons: showing how important the health and poverty-reduction benefits we cover are; being conservative in claiming benefits; and lack of data.

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.

R38: We already removed any reference to the standard CEA framework. See reply 2.

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).

R39: In the conventional analysis of distributional consequences, the poor indeed pay a higher percentage of their incomes in incremental taxes than do the rich. But the whole purpose of the study is to go far beyond that conventional analysis and, by quantifying on a subset of benefits

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.

R40: We have acknowledged why we are only looking at healthcare cost at current period. As discussed in the method and limitation, we used a static model, and we did not apply discount rates to reflect the future cost and benefits.

Conceptually, the comment is still debatable. If a person avoids illness or death, he/she then gets some additional life, with a set of benefits, costs and risk. Basu would be right if having your life saved from stopping smoking would result in some incremental health costs as compared to non-smokers. Is it necessary to say that the study does not deal with how people spend their incomes, including for health care, in their extra years of life.

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.

R41: Same point as Reply 40

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)

R42: Same point as Reply 40.

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.

R43: We have acknowledged in the method and limitation section about the static nature of the model.

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)

R44: See reply 1.

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. ??

R45: We have already addressed this issue of variation in elasticity estimates. See Reply 1

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)

R46: We have already addressed this issue. We also corrected our reference.

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.

R47: We have already addressed the issue of smuggling. See Reply 3 and box 1.

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.

R49: The US is home to about 4% of the world's smokers. We focus on LMICs where cessation remains uncommon.

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.

R549: We have now cited WHO and World Bank recommendations on optimal taxes. We disagree with the assertion that this is an editorial. The analyses and assumptions are fair, reasonable and scientifically robust, including holding up to various sensitivity analyses. We have altered the tone to ensure it is even more conservative.