

## PEER REVIEW HISTORY

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## ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Impact of an e-cigarette tax on cigarette and e-cigarette use in a middle-income country: a study from Indonesia using a pre-post design
<b>AUTHORS</b>	Kowitt, Sarah; Anshari, Dien; Orlan, Elizabeth N.; Kim, Kyung Su; Ranney, Leah; Goldstein, A; Byron, M.

## VERSION 1 – REVIEW

<b>REVIEWER</b>	Christian, Jay University of Kentucky
<b>REVIEW RETURNED</b>	10-Sep-2021

<b>GENERAL COMMENTS</b>	<p>This is an interesting and well written paper that contributes important information and analysis to the literature on e-cigarettes, especially in a global context. Some comments that might help to improve the paper follow:</p> <p><b>Background</b> This section is very well written overall and clearly identifies research topic and hypotheses. One minor comment: there is some redundant numbering of hypotheses at the end.</p> <p><b>Methods</b> Again, well written section. There are, however, some details missing regarding modeling strategy for multivariable regressions. Authors only state that models were adjusted "for demographics", but do not specify further. From the tables, it appears that the authors controlled for income, education, and urban/rural residence. These are really socioeconomic, rather than demographic, variables. Furthermore, there is no real modeling strategy mentioned. How/why were these specific variables chosen to include in model? Is age really best as a continuous variable? Often age ranges can be more realistic because behavioral patterns aren't necessarily linear with regard to age.</p> <p><b>Results</b> Well organized text, but tables are somewhat inadequate since they do not show counts, only percentages. Please consider amending all tables to include counts, in the manner of Supplementary Tables A &amp; B. This is important because the number of women in the sample is very small compared to men. The authors do not even list in the text how many women and men were in the sample, relying instead on percentages and forcing the reader to do the math if they want to know. This lack of transparency is kind of a "red flag". In fact, due to the lack of women, a males-only analysis might be warranted. It would also be nice to know how many women are in each education and</p>
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	<p>income category, as it is possible that some of the cross-tabulated cell counts are rather small.</p> <p>Discussion The relative paucity of women in the sample should be listed as a limitation, and the authors might want to acknowledge that this research may not be generalizable to women. Similarly, the authors mention that their sample was recruited from 31 of 34 Indonesian provinces, but do not provide any other contextual information to help the reader assess the representativeness of the sample.</p>
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<b>REVIEWER</b>	Kramer, Elisabeth The University of Sydney, Sydney Southeast Asia Centre
<b>REVIEW RETURNED</b>	30-Oct-2021

<b>GENERAL COMMENTS</b>	<p>This article is clear, concise and addresses a burgeoning issue in Indonesia (indeed, around the world), as the country decides how to best regulate e-cigarettes. It shed light on the relationship between e-cigarette policy and smokers' decisions to use cigarettes, either to replace, or in conjunction with, traditional cigarettes.</p> <p>As a preliminary study conducted over a relatively short period of time, the authors have presented sufficient data to support their analysis. The discussion is clearly conveyed and justified by the results. The authors have clearly acknowledged the limitations of the paper and draw reasonable conclusions. As I am not a statistician, I would recommend the journal ensure that the statistical analysis in the paper by cross-checked and verified by someone with the relevant background and skills.</p> <p>Overall, I think this paper offers a novel insight into e-cigarette use and smokers' behaviour in response to tax changes and recommend it for publication, based on the authors' consideration of some questions (outlined below).</p> <p>I do have some suggestions to improve the article. In terms of revisions, there are 2 main points that I would like the authors to consider. First, I strongly suggest the article clarify the mechanisms for participant consent to participate in the study, whether any incentives were offered, what mechanisms were in place for people to withdraw and whether any personal identifying information was collected. In short, some additional details n the nature of recruitment and processes applied to ensure the ethical collection of data. While this can be addressed quite simply with some additional information added on page 6, I think it is important to note so readers can be satisfied that they understand the means through which participants were recruited and their expectations of the study when they agreed to participate.</p> <p>The other key point for consideration relates to page 10-11 of the Discussion section. From line 52, the authors outline some interesting information related to the application of the 57% tax rate and the finding that this tax is largely not passed on to consumers. While this is very interesting and certainly a key finding of the study, it does not relate to any of the stated aims of the study as outlined in the introduction. The authors could consider either incorporating an additional "goal" in the introduction that would then justify the inclusion of this section of</p>
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	<p>the discussion or removing this insight and repurposing it for a separate article.</p> <p>I have some additional very minor points:          -Typo is sentence line 10, page 5: should read “use of e-cigarettes”          -It would be good to note in the introduction the name and number of the regulation that cover the 57% tax rate for e-cigarettes. This will help reader who wish to know more about the nature of the regulation and the application of the tax system.</p>
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<b>REVIEWER</b>	Bigwanto, Muhammad Prof Dr Hamka University of Muhammadiyah
<b>REVIEW RETURNED</b>	31-Oct-2021

<b>GENERAL COMMENTS</b>	<p><b>Title:</b> The use of 'a middle-income country' in the title seems to be OK but could mislead the readers, especially when you do not mention Indonesia in the title. I suggest you mention Indonesia at the end of the title or change 'a middle-income country' to 'in Indonesia'.</p> <p><b>Abstract:</b> Please add the information on how you selected or recruited participants.</p> <p><b>Methods:</b>          1) Please explain why was this sample selected in light of the research goals.          2) What is the platform used to collect the data from the participants? the goggle forms? qualtrics? please mention.          3) Was the informed consent solicited, how? please explain.          4) Is there any relevant reliability and validity data from previous research or from this of the study that supports the use of these measures? please describe.          5) Since this is an online survey, are there any procedural checks were completed to avert potential sources of bias?          6) What is the definition of e-cigarettes in this study? since most Indonesian also consider HTP as e-cigarettes, this definition needs to be confirmed in the manuscript. For example, the term of e-cigarettes refers to ENDS from WHO.          7) Please kindly mention the details of the ethical clearance, the approval number, and the year?          8) Does the research employ any staff to do recruitment of the participants and send an online survey link? if yes, are they well trained? please explain clearly.</p> <p><b>Discussion:</b>          1) about the information from tax officials, how many of them and when was this being obtained? by interview? Please be specific.          2) to robust the discussion part, you can add the data on dual-use (cigarettes and e-cigarettes) in Indonesia.</p> <p><b>Reference:</b>          1) Ref No 1. It is published in 2019, or 2020? not clear.          2) Ref No 3. please check the double comma in the end of the reference (typo).          3) There are at least 2 journal articles that were published more than 10 years, please consider finding the latest one, if any.</p>
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<b>REVIEWER</b>	Astuti, Putu The University of Sydney, School of Public Health, The University of Sydney
<b>REVIEW RETURNED</b>	13-Nov-2021

<b>GENERAL COMMENTS</b>	<p>The manuscript discussed an interesting and important topic in this growing uptake of e-cigarette use worldwide including in Indonesia. Overall, it is clear and well written. I have several comments and clarification to the authors.</p> <ol style="list-style-type: none"> <li>1. In the introduction of the abstract, the authors describe that e-cig tax may increase cigarette smoking. It is seemed unlinked, and I think not all readers understand about the fact that many of e-cig users may also use cigarette (dual user). I think it is better if the authors could provide this link Similarly, on the Introduction, although the authors provide the description of the substitution nature of the two products, providing the magnitude of dual users (if possible, in low income/Indonesian setting) will emphasize it.</li> <li>2. What was the rational of the time lag between wave 1 and 2 which was pretty short? In the article summary the authors mentioned the study observed behaviour over two-three months periods, but from time of data collection (method section), it is around one month lag between the two waves. How this short period effect the ability to answer the hypothesis of changing e-cig and cigarette use?</li> <li>3. Based on the inclusion and exclusion criteria, the study is basically involving dual smokers and e-cigarette users who quit within one year, right? Then, the study is not reflecting the impact of e-cig tax on novice cigarette smokers. I am not sure if the authors have discussed this. In Indonesia, the use of e-cig is also high among young people age 10-18 years which also warrant future investigation.</li> <li>4. On the measure regarding "cigarette frequency", the authors measured the quantity of cigarette use instead of cigarette use frequency, right? I think it will be clearer if the authors also assert that they measured number of cigarettes use per week at the beginning of the description before explanation of the questions asked to daily user.</li> <li>5. Regarding the urbanicity variable, what question was asked to the participants? It is better if the authors could provide the criteria for categorizing this variable.</li> <li>6. The participants of the study were 96.4% males and the authors asserted that this is aligned with the high prevalence of tobacco use in Indonesia; this is true for cigarette smoking but as described in the introduction proportion of e-cigarette use among adult male and female are almost similar. Please clarify this claim.</li> <li>7. In the result section, there is no description on the age of the participants (it is available on the table 1). From the table, we can see that the mean age is 23.8 which fall into younger adult categories, I suggest providing information on age range, and does the study reflect the older age group category?</li> <li>8. I found there a sign of correlation between age, education and income. More than 50% participants complete high school and around a third falls into low-income category (some are not earning at all), I am wondering if certain proportion of the participants are still studying as university students which seem to fit this fact. Does the income question include pocket money? I think this can be discussed to avoid misinterpretation.</li> </ol>
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	<p>9. It is great that the participants of the study come from 31 provinces of Indonesia. I think the authors should provide additional information about the distribution of the participants based on the geographic area to get sense on the representativeness of the response. Although we know that e-cigarette use is much higher in bigger city and urban area.</p> <p>10. When I read the result on certain proportion of participants recalled the tax stamps on Wave 1, I was questioning their response until I see the explanation on the discussion section. I think the authors can described this information either in the Introduction or Methods (setting &amp; context) to provide background knowledge about this fact.</p> <p>11. I am wondering if the authors asked about the awareness of the participants on the time of tax increase come into effects since this will affect their price minimisation technique.</p> <p>12. For the age variable, did the authors try to fit it in as a categorical variable in the multivariate analysis? I commented about the age range of participants above which may provide more understanding about the study participants. And fitting age as categorical variable may also provide additional information on the association between age and the outcome.</p> <p>13. Based on the finding of this study, what is practical recommendation on both products?</p> <p>Writing and interpretation:</p> <p>1. Page 10, line 25-27; the second part of the sentence can be dropped because it is redundant to the first description</p> <p>2. Page 11, line 8, the "relative to no change" should be dropped since the comparison has been mentioned earlier in this sentence.</p> <p>3. Page 11, line 9-10 regarding cigarette frequency. This is connected to my comment above regarding the use of term frequency to measure quantity. The authors should also provide the measurement unit when interpreting the regression coefficient.</p>
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### VERSION 1 – AUTHOR RESPONSE

Reviewer: 1, Dr. Jay Christian, University of Kentucky	
<p>1. This is an interesting and well written paper that contributes important information and analysis to the literature on e-cigarettes, especially in a global context. Some comments that might help to improve the paper follow:</p>	<p>Thank you for reviewing our paper and providing feedback.</p>
<p>1. Background: This section is very well written overall and</p>	<p>Thank you! We removed the redundant numbering of hypotheses at the end of the introduction (pages 4-5).</p>

<p>clearly identifies research topic and hypotheses. One minor comment: there is some redundant numbering of hypotheses at the end.</p>	
<p>1. Methods” Again, well written section. There are, however, some details missing regarding modeling strategy for multivariable regressions. Authors only state that models were adjusted "for demographics", but do not specify further. From the tables, it appears that the authors controlled for income, education, and urban/rural residence. These are really socioeconomic, rather than demographic, variables. Furthermore , there is no real modeling strategy mentioned. How/why were these specific variables chosen to include in model? Is age really best as a continuous variable? Often age ranges can be more realistic because behavioral patterns aren't necessarily linear with regard to age.</p>	<p>We amended our description of the multivariable regressions and now note that our models control for demographic and socioeconomic variables (page 8). We chose to control for these demographic and socioeconomic variables because it is possible that these variables could have affected change in e-cigarette use and cigarette use between Waves 1 and 2. Additionally, given your suggestion and a suggestion from reviewer 4, we categorized age as two groups (young adults 18-24 years old vs. adults 25+) (pages 9, 19, 21).</p>
<p>1. Results: Well organized text, but tables are somewhat inadequate since they do not show counts, only percentages. Please consider amending all tables to</p>	<p>Thank you for this comment. We amended Tables 1 and 2 to provide counts of all cells. We also acknowledge in the limitations section that our sample included few women and results may not be generalizable to women (page 14). We agree that a males-only analysis might be warranted, but we would like to preserve as much of the sample as possible and women were eligible for this study and completed the surveys, so we believe it makes sense to still include them in the analyses.</p>

<p>include counts, in the manner of Supplementary Tables A &amp; B. This is important because the number of women in the sample is very small compared to men. The authors do not even list in the text how many women and men were in the sample, relying instead on percentages and forcing the reader to do the math if they want to know. This lack of transparency is kind of a "red flag". In fact, due to the lack of women, a males-only analysis might be warranted. It would also be nice to know how many women are in each education and income category, as it is possible that some of the cross-tabulated cell counts are rather small.</p>	<p>With regards to cross-tabs of sex by education: 2 women had completed secondary school or less, 20 women had completed high school, 14 women had completed college / university, and 1 woman had completed a graduate degree. For sex by income, 11 women had an income of IDR 0-1.5 million, 15 women had an income of IDR 1.5-3.5 million, 9 women had an income of IDR 3.5-7 million, and 2 women had an income of more than IDR 7 million. Although these cell size counts are small, we did not analyze interactions for sex by education or sex by income, so we do not believe that this is a major concern.</p>
<p>1. Discussion: The relative paucity of women in the sample should be listed as a limitation, and the authors might want to acknowledge that this research may not be generalizable to women. Similarly, the authors mention that their sample was recruited from 31 of 34 Indonesian provinces, but do not provide any other contextual information to help the reader assess the representativeness of the sample.</p>	<p>We added a limitation that there were very few women in our sample and results may not be generalizable to women (page 14).</p> <p>We have also added a sentence comparing the sample to the general population: "While the income levels of our sample were approximately in line with the national average of 2.9 million rupiah per month [28] and most participants were from urban areas which is also in line with national averages [29], our sample skewed younger and toward a higher than average education level, perhaps due to our recruitment via social media." (page 14)</p>
<p>Reviewer: 2, Dr. Elisabeth Kramer, The University of Sydney</p>	



<p>1. Comments to the Author: This article is clear, concise and addresses a burgeoning issue in Indonesia (indeed, around the world), as the country decides how to best regulate e-cigarettes. It shed light on the relationship between e-cigarette policy and smokers' decisions to use cigarettes, either to replace, or in conjunction with, traditional cigarettes. As a preliminary study conducted over a relatively short period of time, the authors have presented sufficient data to support their analysis. The discussion is clearly conveyed and justified by the results. The authors have clearly acknowledged the limitations of the paper and draw reasonable conclusions. As I am not a statistician, I would recommend the journal ensure that the statistical analysis in the paper by cross-checked and verified by someone with the relevant background and skills. Overall, I think this paper offers a novel insight into e-cigarette use and smokers' behaviour in response to tax changes and recommend it for publication, based on</p>	<p>Thank you for reviewing our paper and providing feedback.</p>
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<p>the authors' consideration of some questions (outlined below).</p>	
<p>1. I do have some suggestions to improve the article. In terms of revisions, there are 2 main points that I would like the authors to consider. First, I strongly suggest the article clarify the mechanisms for participant consent to participate in the study, whether any incentives were offered, what mechanisms were in place for people to withdraw and whether any personal identifying information was collected. In short, some additional details on the nature of recruitment and processes applied to ensure the ethical collection of data. While this can be addressed quite simply with some additional information added on page 6, I think it is important to note so readers can be satisfied that they understand the means through which participants were recruited and their expectations of the study when they agreed to participate.</p>	<p>Thank you for this comment. In the methods we now provide information about how participant consent was obtained, participants ability to withdraw from the study at any point, the incentives for taking the survey, and what personal identifying information was collected (page 5).</p>

<p>1. The other key point for consideration relates to page 10-11 of the Discussion section. From line 52, the authors outline some interesting information related to the application of the 57% tax rate and the finding that this tax is largely not passed on to consumers. While this is very interesting and certainly a key finding of the study, it does not relate to any of the stated aims of the study as outlined in the introduction. The authors could consider either incorporating an additional “goal” in the introduction that would then justify the inclusion of this section of the discussion or removing this insight and repurposing it for a separate article.</p>	<p>Thank you for bringing this up. We appreciate your concern and would like to explain our reasoning for including this information in the discussion. We had three explicit goals going into the study: 1) examine if the e-liquid tax is associated with changes in e-cigarette use, 2) examine if the e-liquid tax is associated with changes in cigarette use, and 3) examine if changes in e-cigarette use are associated with changes in cigarette use. Our results section aligns with these three stated goals. We did not expect that there would be a large discrepancy between the intended and actual tax rates, so interviewing tax officials was not one of our original research aims. Yet, when we saw some tax officials at a table at a vaping convention, we took the opportunity to ask some questions about the basic implementation plan and impact on pricing. We also followed up on these discussions with two additional private meetings with tax officials. We provide these additional details on page 11. We do not have sufficient data for a separate article but we did think a brief mention was relevant and useful to readers seeking basic context.</p>
<p>1. I have some additional very minor points:</p> <ul style="list-style-type: none"> <li>- Typo is sentence line 10, page 5: should read “use of e-cigarettes”</li> <li>- It would be good to note in the introduction the name and number of the regulation that cover the 57% tax rate for e-cigarettes. This will help reader who wish to know more about the nature of the regulation and the application of the tax system.</li> </ul>	<p>Thank you. We have fixed the typo and explained that this regulation was the “Finance Ministerial Regulation No. 146/010/2017.” We now provide this information in the methods section (page 5).</p>
<p>Reviewer: 3, Dr. Muhammad Bigwanto, Prof Dr Hamka University of Muhammadiyah</p>	
<p>1. Title: The use of 'a middle-income country' in the title seems to be OK but could mislead the readers, especially</p>	<p>Thank you—we added Indonesia to the title (page 1).</p>

when you do not mention Indonesia in the title. I suggest you mention Indonesia at the end of the title or change 'a middle-income country' to 'in Indonesia'.	
1. Abstract: Please add the information on how you selected or recruited participants.	We added information for how we recruited participants to the methods in the abstract (page 2).
1. Methods: 1) Please explain why was this sample selected in light of the research goals.	We added the methods why we selected this sample in light of our research goals (page 5).
1. Methods: 2) What is the platform used to collect the data from the participants? the goggle forms? qualtrics? please mention.	We added the survey collection tool we used (Qualtrics) to the methods section (page 5).
1. Methods: 3) Was the informed consent solicited, how? please explain.	We added more details to the methods on how informed consent was solicited (page 5).
1. Methods: 4) Is there any relevant reliability and validity data from previous research or from this of the study that supports the use of these measures? please describe.	One of our implicit goals with this study was to make sure other researchers could reproduce our results. As such, we selected measures for most of our outcomes from previously used population-based surveys including the International Tobacco Control Policy Evaluation Project (ITC), National Health and Nutrition Examination Survey (NHANES), and Global Adult Tobacco Survey (GATS). We would love to include reliability and validity data supporting these measures from previous research, but unfortunately, we are limited by space concerns. We did, however, cite all of our measures so that readers can look up validity and reliability data on their own (pages 4-8). There are a few items that we created for the purposes of this study (e.g., recall of tax stamp) since these items have not been assessed in previous population-based surveys. We also checked with our Indonesian contacts to make sure our questions made sense as written.
1. Methods: 5) Since this is an online survey, are there any procedural checks were completed to avert potential sources of bias?	We checked to make sure there were no duplicate responses from the same participants. We also included three attention checks in the survey, but after the survey was completed, Qualtrics published new data that suggests that removing respondents that fail attention checks is likely to introduce a demographic bias. In light of this information, we chose not to analyze data on our attention checks. For more

	information, please see <a href="https://www.qualtrics.com/blog/using-attention-checks-in-your-surveys-may-harm-data-quality/">https://www.qualtrics.com/blog/using-attention-checks-in-your-surveys-may-harm-data-quality/</a>
1. Methods: 6) What is the definition of e-cigarettes in this study? since most Indonesian also consider HTP as e-cigarettes, this definition needs to be confirmed in the manuscript. For example, the term of e-cigarettes refers to ENDS from WHO.	<p>We included additional details for how we defined e-cigarettes. Specifically, before assessing e-cigarette use, we told participants: “The next few questions are about e-cigarettes, also called vapes. E-cigarettes are battery-powered and produce vapor instead of smoke. There are many different types of e-cigarettes.” We also included a photo of eight different types of commonly used e-cigarette devices (pages 5-6).</p> <p>At the time of data collection in 2018, we (including our Indonesian research colleagues) do not think there was substantial use of HTPs. News reports concur (<a href="https://www.thejakartapost.com/news/2019/05/13/hm-sampoerna-to-launch-iqos-smoking-device-in-indonesia.html">https://www.thejakartapost.com/news/2019/05/13/hm-sampoerna-to-launch-iqos-smoking-device-in-indonesia.html</a>). In our visits to numerous vape shops in 2018 and in our attending a 2018 Jakarta vaping convention we did not see HTPs for sale.</p>
1. Methods: 7) Please kindly mention the details of the ethical clearance, the approval number, and the year?	We now provide the approval number and year of approval in the methods (page 6).
1. Methods: 8) Does the research employ any staff to do recruitment of the participants and send an online survey link? if yes, are they well trained? please explain clearly.	Recruitment was run by Dr. Byron and Dr. Anshari, both of whom have doctoral degrees in public health and extensive research experience. We now mention this in the methods (page 5). The recruiting method was automated and direct – i.e., clicking on the Facebook ad sent participants to a screener and if they met the qualifications, they immediately completed the survey. The follow-up survey was similarly managed. University of Indonesia public health student research assistants helped with distribution of the incentive payments (because that had to be done individually).
1. Discussion: 1) about the information from tax officials, how many of them and when was this being obtained? by interview? Please be specific.	We spoke with four tax officials (two at a public event in fall of 2018 prior to the law implementation and two in private meetings). We now mention in the discussion (page 11).
1. Discussion: 2) to robust the discussion part, you can add the data on dual-use (cigarettes and e-cigarettes) in Indonesia.	<p>Thank you—this is an important point. We now note in the introduction that recent data suggests a high prevalence of dual use (cigarettes and e-cigarettes) in Indonesia (page 4).</p> <p><a href="https://protc.id/wp-content/uploads/2021/07/Siaran-Pers-Beban-Ganda-Pengguna-Rokok-Elektronik-Konvensional-1.pdf">https://protc.id/wp-content/uploads/2021/07/Siaran-Pers-Beban-Ganda-Pengguna-Rokok-Elektronik-Konvensional-1.pdf</a></p> <p>We felt that this information fit better in <a href="#">the</a> introduction as opposed to the discussion, but we would be happy to amend the article and include this information in the discussion at a later point if necessary.</p>

<p>1. References:</p> <ul style="list-style-type: none"> <li>- Ref No 1. It is published in 2019, or 2020? not clear.</li> <li>- 2) Ref No 3. please check the double comma in the end of the reference (typo).</li> <li>- 3) There are at least 2 journal articles that were published more than 10 years, please consider finding the latest one, if any.</li> </ul>	<p>Thank you for catching this—we fixed the two typos in the references.</p> <p>We also removed one of the journal articles that was published more than 10 years ago. There are two remaining journal articles that were published more than 10 years ago that we would like to keep. One refers to the methods of the ITC survey, which we used to select some of our measures (20. Thompson ME, Fong GT, Hammond D, et al. Methods of the International Tobacco Control (ITC) four country survey. <i>Tob. Control</i> 2006;15(suppl 3):iii12-iii18).</p> <p>The other journal article supports our statement that taxes are often absorbed in middle-income countries, rather than passed on and is from Indonesia. Because this article is Indonesia-specific, it is hard to replace with a newer citation. (Barber S, Ahsan A. The tobacco excise system in Indonesia: hindering effective tobacco control for health. <i>J. Public Health Policy</i> 2009;30(2):208-25).</p>
<p>Reviewer: 4, Dr. Putu Astuti, The University of Sydney</p>	
<p>1. Comments to the Author: The manuscript discussed an interesting and important topic in this growing uptake of e-cigarette use worldwide including in Indonesia. Overall, it is clear and well written. I have several comments and clarification to the authors.</p>	<p>Thank you for reviewing our paper and for providing your thoughtful comments.</p>
<p>1. In the introduction of the abstract, the authors describe that e-cig tax may increase cigarette smoking. It is seemed unlinked, and I think not all readers understand about the fact that many of e-cig users may also use cigarette (dual user). I think it is better if the authors could provide this link. Similarly, on the Introduction, although the authors provide the description of the substitution nature of the two products, providing the magnitude of dual</p>	<p>We have added in the introduction that: “recent research suggests that the prevalence of dual use in Indonesia is high (i.e., the majority of people who use e-cigarettes also smoke cigarettes).” (page 4)</p> <p><a href="https://protc.id/wp-content/uploads/2021/07/Siaran-Pers-Beban-Ganda-Pengguna-Rokok-Elektronik-Konvensional-1.pdf">https://protc.id/wp-content/uploads/2021/07/Siaran-Pers-Beban-Ganda-Pengguna-Rokok-Elektronik-Konvensional-1.pdf</a></p> <p>We also now mention in the abstract: “Economic research suggests that this tax could unintentionally increase cigarette smoking among dual users” (page 2).</p>

<p>users (if possible, in low income/Indonesian setting) will emphasize it.</p>	
<p>1. What was the rationale of the time lag between wave 1 and 2 which was pretty short? In the article summary the authors mentioned the study observed behaviour over two-three months periods, but from time of data collection (method section), it is around one month lag between the two waves. How this short period effect the ability to answer the hypothesis of changing e-cig and cigarette use?</p>	<p>We agree that there was a short amount of time between the wave 1 and wave 2 surveys. Ideally, we would have liked to have additional follow-up surveys to understand long-term changes from the law, but we were constrained by the limited timeframe of our research funding and by our goal of having a high follow-up rate in the second survey. We acknowledge the short timeframe in the limitations section (page 14).</p>
<p>1. Based on the inclusion and exclusion criteria, the study is basically involving dual smokers and e-cigarette users who quit within one year, right? Then, the study is not reflecting the impact of e-cig tax on novice cigarette smokers. I am not sure if the authors have discussed this. In Indonesia, the use of e-cig is also high among young people age 10-18 years which also warrant future investigation.</p>	<p>We required all participants at Wave 1 to currently use e-cigarettes (defined as using in the past month) and currently use cigarettes (defined as using in the past month) OR have recently quit (defined as within the past year). So people were either current dual users or recently quit. You are correct that we excluded people who experimented with e-cigarettes / cigarettes and people who were below the age of 18. We now note this in the limitations section (page 14).</p>
<p>1. On the measure regarding “cigarette frequency”, the authors measured the quantity of cigarette use instead of cigarette use frequency, right?</p>	<p>This is correct and we now refer to cigarette use quantity (number of cigarettes used per week) rather than cigarette use frequency throughout the manuscript.</p>

<p>1. I think it will be clearer if the authors also assert that they measured number of cigarettes use per week at the beginning of the description before explanation of the questions asked to daily user.</p>	<p>Thank you--we made this change (pages 7-8).</p>
<p>1. Regarding the urbanicity variable, what question was asked to the participants? It is better if the authors could provide the criteria for categorizing this variable.</p>	<p>We provided the exact item for our urbanicity variable (page 8).</p>
<p>1. The participants of the study were 96.4% males and the authors asserted that this is aligned with the high prevalence of tobacco use in Indonesia; this is true for cigarette smoking but as described in the introduction proportion of e-cigarette use among adult male and female are almost similar. Please clarify this claim.</p>	<p>We made this clarification in the results section of the manuscript (page 9).</p>
<p>1. In the result section, there is no description on the age of the participants (it is available on the table 1). From the table, we can see that the mean age is 23.8 which fall into younger adult categories, I suggest providing information on age range, and does the study reflect the older age group category?</p>	<p>Thank you for this suggestion. In the results section, we now provide more data on age (page 9), and in table 1, we provided the age range in a footnote (18-55 years).</p>
<p>1. I found there a sign of correlation between</p>	<p>We did collect data on participants' jobs and 33.2% reported being a student (n=345). The exact wording for our income</p>



<p>age, education and income. More than 50% participants complete high school and around a third falls into low-income category (some are not earning at all), I am wondering if certain proportion of the participants are still studying as university students which seem to fit this fact. Does the income question include pocket money? I think this can be discussed to avoid misinterpretation.</p>	<p>question was “How much is your average monthly income?” It is possible that for some people this was interpreted as including pocket money.</p>
<p>1. It is great that the participants of the study come from 32 provinces of Indonesia. I think the authors should provide additional information about the distribution of the participants based on the geographic area to get sense on the representativeness of the response. Although we know that e-cigarette use is much higher in bigger city and urban area.</p>	<p>We appreciate the comment. While it would perhaps be useful to discuss the geographic distribution of the participants in the 32 provinces as compared to the actual population distribution in the 34 provinces, we think that may be beyond what fits this paper. We accept that this is a convenience sample, but the widespread use of Facebook across Indonesia, combined with our knowledge that the survey’s urbanicity rate was aligned with the national average, and that we reached almost all of the provinces do provide some helpful information.</p> <p>Please see this article about Facebook use in Indonesia for more context: <a href="https://www.thejakartapost.com/life/2018/03/04/indonesia-fourth-highest-number-of-facebook-users-in-the-world.html">https://www.thejakartapost.com/life/2018/03/04/indonesia-fourth-highest-number-of-facebook-users-in-the-world.html</a></p>
<p>1. When I read the result on certain proportion of participants recalled the tax stamps on Wave 1, I was questioning their response until I see the explanation on the discussion section. I think the authors can described this information either in the Introduction or Methods (setting &amp; context) to provide</p>	<p>This is important, thank you. We added this information to the methods section under “recall of tax stamp.” (page 7)</p>

background knowledge about this fact.	
1. I am wondering if the authors asked about the awareness of the participants on the time of tax increase come into effects since this will affect their price minimisation technique.	This is a great point. We asked at Wave 2: "In the last year, have you heard any news about changes in the taxes or price of e-cigarette liquid?" We did not want to ask this question at Wave 1 because we did not want to prime people to change their behaviors and use price minimization techniques.
1. For the age variable, did the authors try to fit it in as a categorical variable in the multivariate analysis? I commented about the age range of participants above which may provide more understanding about the study participants. And fitting age as categorical variable may also provide additional information on the association between age and the outcome.	Thank you for this suggestion. We amended our analyses to categorize age as two groups (young adults 18-24 years old vs. adults 25+) (page 8).
1. Based on the finding of this study, what is practical recommendation on both products?	In the conclusion we state that, "These findings suggest that cigarettes and e-cigarettes may be substitutes for one another and that care should be taken when proposing tobacco taxes so as not to unintentionally increase use of the most harmful forms of tobacco use." (page 14) We believe this is the extent of what can be said based on our study.
1. Writing and interpretation: <ul style="list-style-type: none"> <li>• Page 10, line 25-27; the second part of the sentence can be dropped because it is redundant to the first description</li> <li>• Page 11, line 8, the "relative to no change" should be dropped since the comparison has been mentioned earlier in this sentence.</li> </ul>	Thank you—we made the suggested changes, including providing the measurement unit when interpreting regression coefficients from Table 3.

<ul style="list-style-type: none"> <li>Page 11, line 9-10 regarding cigarette frequency. This is connected to my comment above regarding the use of term frequency to measure quantity. The authors should also provide the measurement unit when interpreting the regression coefficient.</li> </ul>	
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### VERSION 2 – REVIEW

<b>REVIEWER</b>	Christian, Jay University of Kentucky
<b>REVIEW RETURNED</b>	07-Jan-2022

<b>GENERAL COMMENTS</b>	Well done with revisions.
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<b>REVIEWER</b>	Bigwanto, Muhammad Prof Dr Hamka University of Muhammadiyah
<b>REVIEW RETURNED</b>	10-Jan-2022

<b>GENERAL COMMENTS</b>	<p>Good work from the authors. All stressed suggestions for improvement in the previous manuscript have been carried out in the revised version.</p> <p>The title of the manuscript is now clearer, the abstract is accurate, balanced, and complete. Methods described sufficiency to allow the study to be repeated. Research ethics such as participant consent, ethics approval are addressed appropriately.</p> <p>Only one more thing that needs to be changed in the conclusion. Since the study is not comparing the harmful effect of these two products, the last statement in the conclusion is not relevant and not appropriate to this study. Therefore, the sentence: 'to unintentionally increase the use of the most harmful forms of tobacco use' should be changed to 'to unintentionally increase the use of cigarettes'.</p> <p>Thank you, my best wishes to all authors.</p>
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<b>REVIEWER</b>	Astuti, Putu The University of Sydney, School of Public Health, The University of Sydney
<b>REVIEW RETURNED</b>	17-Jan-2022

<b>GENERAL COMMENTS</b>	Dear authors,
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	Thank you for revising the manuscript based on all reviewers comments. It is become more clearer, and I have no further comments.
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### VERSION 2 – AUTHOR RESPONSE

Reviewer's comment: Only one more thing that needs to be changed in the conclusion. Since the study is not comparing the harmful effect of these two products, the last statement in the conclusion is not relevant and not appropriate to this study. Therefore, the sentence: 'to unintentionally increase the use of the most harmful forms of tobacco use' should be changed to 'to unintentionally increase the use of cigarettes'.

Authors' Response: Thank you for reviewing our manuscript again. We have made the change you suggested.