

## PEER REVIEW HISTORY

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### ARTICLE DETAILS

<b>TITLE (PROVISIONAL)</b>	Secondhand smoke (SHS) exposure before and after the implementation of the Tobacco Free Cities (TFC) initiative in five Chinese cities: a pooled cross-sectional study
<b>AUTHORS</b>	Duan, Zongshuan; Wang, Yu; Huang, Jidong; Redmon, Pamela; Eriksen, Michael

### VERSION 1 – REVIEW

<b>REVIEWER</b>	Phong K Thai and Long K Tran QAEHS, Queensland Alliance for Environmental Health Science, The University of Queensland, Brisbane, Australia
<b>REVIEW RETURNED</b>	25-Sep-2020

<b>GENERAL COMMENTS</b>	<p>This is a well-written manuscript that provides valuable information about the state of exposure to second hand smoke and impact of tobacco control campaigns in China. I enjoyed reading the work and found it generally well written. There are some issues that I would encourage the authors to address</p> <ol style="list-style-type: none"><li>1. Abstract<ul style="list-style-type: none"><li>• The SHS exposure in the article was in the past 30 days. It is the key information that I thought should be mentioned in the abstract as it would influence the uncertainty of recall bias as mentioned in the limitation section.</li></ul></li><li>2. Strengths and limitations of this study<ul style="list-style-type: none"><li>• The limitation of self-report was presented in the discussion but not here. So please add the limitation from this point</li></ul></li><li>3. Introduction<ul style="list-style-type: none"><li>• The information on the main study was presented quite too much, e.g. paragraph 2 of page 6. This could be summarized and could be transferred to the method section</li><li>• The worldwide information on this topic could be added instead in the introduction</li><li>• The intervention applied in this project is fully comprehensive or partial? Limitation of the tobacco control law in China? (this point could be used as a reason to implement the project)</li></ul></li><li>4. Method section<ul style="list-style-type: none"><li>• Line 36 page 7: please make sure that the term “SHS exposure” in the study is the SHS exposure in the past 30 days? <input type="checkbox"/> it should be consistent in the whole document</li><li>• The definition of SHS in relation to current smokers: were current smokers considered exposed to SHS when they smoked themselves? Were they exposed in the work place at the smoking places? Did you clean for logical in the data?</li></ul></li></ol>
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	<ul style="list-style-type: none"> <li>• The category of “factory, business, agriculture, and service industry employee,” is too broad as the probability of exposure to SHS could be very different in factory workers than in service industry employees.</li> <li>• Please report more detail on the missing information in line 27, page 8. Should the listwise technique be better in this case because the total n was massive and it is cluster sampling (you reported the 95%CI)</li> <li>• Please provide the total number of participants of the whole project and then the valid n has been used for analysis</li> </ul> <p>5. Results section</p> <ul style="list-style-type: none"> <li>• This should be more interesting if you could present an overall picture of SHS exposure: % exposure at only 1 venue? 2 venues? All 3 venues</li> <li>• Please consider the current smoking status? People should be exposed when they smoked.</li> <li>• Could you consider to run the model stratify by gender and smoking status? This should make the information more valuable</li> <li>• The categorized of occupation should be re-consider because this factor could influence the SHS exposure in home/workplace</li> <li>• Provide the n of table 4?</li> <li>• In Table 4, maybe explain why the age group of &gt;65 and the occupation group of “not in the labor force” were used as reference.</li> </ul> <p>6. Discussion section</p> <ul style="list-style-type: none"> <li>• Page 14, line 42, is there any explanation why the education level would influence the exposure in restaurants?</li> <li>• Again similar question about why current smokers have higher exposure to SHS</li> <li>• This part should be improved by discussing with other countries/studies about the SHS situation and the declining trend over the years. Table S4 compared to only the China nationwide survey</li> <li>• The discussion on the effects of intervention should be added. Comparing with other intervention?</li> </ul>
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<b>REVIEWER</b>	Genevieve Sansone University of Toronto, Canada
<b>REVIEW RETURNED</b>	27-Sep-2020

<b>GENERAL COMMENTS</b>	<p>Overall comments:</p> <p>This is a well-written, well-organized paper examining the impact of city-level smoke-free initiatives in five cities in China – where the toll of smoking and SHS is the greatest in the world. This paper adds to the existing literature by using pre-post data to evaluate the effectiveness of an intervention; including analyses by subgroups to examine differences in SHS exposure according to education, income, sex, and occupation. In particular, the findings on gender were interesting and point to the need for further research on tobacco control interventions to reduce smoking among men, who are more likely to expose other members of their household to their smoking at home. The findings on occupation type were also interesting and important for examining the impact of any workplace smoking bans.</p> <p>Specific comments:</p> <p>Introduction - Paragraph 3, lines 32-35 – please provide the year for the data cited on workplace and home exposure in the text</p>
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	<p>When naming the five cities selected for the TFC initiative, the authors use the wording “including” before naming the cities. This implies there were more cities that participated in the TFC initiative, besides the five listed here. Is this the case? If it was only implemented in the five cities that were included in this study, please delete the word “including” from the Introduction (paragraph 3, line 40) and Abstract (under “Setting”)</p> <p>Introduction – Paragraph 5 describes what is known about SHS exposure in China from previous studies. The authors state that there is a lack of representative, citywide data and only small randomized controlled trials have been done. I would suggest to include findings from Fong et al. (2015) and Sansone et al (2019), in which data from a large, representative sample of smokers in several large Chinese cities in the ITC Survey were used to evaluate trends in SHS exposure in public places over time. The 2019 study also expanded to include data from non-smokers and rural areas – which the authors mention is lacking from other studies. These references are already included in the citation list but should be mentioned in this paragraph as well.</p> <ul style="list-style-type: none"> <li>• Fong GT, Sansone G, Yan M, et al. Evaluation of smoke-free policies in seven cities in China, 2007–2012. <i>Tobacco Control</i> 2015;24:iv14-iv20.</li> <li>• Sansone G, Fong GT, Yan M, et al. Secondhand smoke exposure and support for smoke-free policies in cities and rural areas of China from 2009 to 2015: a population-based cohort study (the ITC China Survey). <i>BMJ Open</i> 2019;9:e031891. doi: 10.1136/bmjopen-2019-031891</li> </ul> <p>Results - The results show that smoking prevalence did not change in these cities from 2015 to 2018 despite the TFC initiative (23% at each time point). In addition, while SHS exposure rates decreased significantly from 2015 to 2018, SHS levels remain high overall. This could be highlighted or discussed in the Discussion section. For example, if SHS decreased but smoking did not, does this mean people may be smoking elsewhere? Is this a possible reason why SHS exposure in the home did not decrease as much in the five cities as it did nationally over this period, according to Suppl Table 4, and why it did not decrease among men or among smokers according to Table 3? Given that some critics of smoke-free policies covering public places argue that such policies will result in a displacement of smoking into the home, it may be important to mention these findings and provide any explanation for why the current findings appear to be not as strong for home SHS exposure.</p> <p>Discussion – paragraph 1, lines 30-31 – this is a good point to make comparing trends in SHS exposure in the cities in this study with previous results reported from the nationwide survey. However, the current wording is unclear that this comparison occurs over the same time period (as is clearly shown in Supplemental Table 4). Please add “over the same time period” to the phrase “Compared with the overall levels of SHS exposure reported in the nationwide surveys,”.</p> <p>Methods - I am interested in knowing more about differences across the five cities that participated. It appears from Supplemental Table 1 that Xi’an is the only city that implemented a city-wide smoke-free law, whereas other cities were only sector-</p>
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	<p>wide. This could provide an opportunity to compare results in Xi'an with the other cities to see if the impact on SHS exposure was greater in Xi'an. The authors should clarify: a) that the results presented are among all cities combined only, thus results represent an average across the cities; and b) why there is no examination of differences across cities (or clarify if this was indeed done).</p>
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## VERSION 1 – AUTHOR RESPONSE

### Reviewer 1:

Reviewer Name: Phong K Thai and Long K Tran  
 Institution and Country: QAEHS, Queensland Alliance for Environmental Health Science, The University of Queensland, Brisbane, Australia

### Overall comments:

**This is a well-written manuscript that provides valuable information about the state of exposure to secondhand smoke and impact of tobacco control campaigns in China. I enjoyed reading the work and found it generally well written. There are some issues that I would encourage the authors to address.**

Response: We appreciate this reviewer's overall positive evaluation of our manuscript. Below we provided a point-by-point response to the comments raised by this reviewer. We hope this reviewer will find our responses satisfactory.

### Comment #1:

#### 1. Abstract

**• The SHS exposure in the article was in the past 30 days. It is the key information that I thought should be mentioned in the abstract as it would influence the uncertainty of recall bias as mentioned in the limitation section.**

Response to Comment #1: Per this comment, our study outcomes have been clarified as "Self-reported past-30-day (P30D) SHS exposure in indoor workplaces, restaurants, and homes" in the Abstract. We also clarified the description of the outcome variables throughout the rest of our revised manuscript.

### Comment #2:

#### 2. Strengths and limitations of this study

**• The limitation of self-report was presented in the discussion but not here. So please add the limitation from this point**

Response to Comment #2: We appreciate this comment. The limitation of self-reported outcome measures has now been added to the "Strengths and limitations of this study" section. The revised sentence is now written as "Limitations of this study included self-reported smoking status and SHS exposure, which may be subject to recall bias and social desirability bias, and the inability to estimate the SHS exposure changes before and after the implementation of the TFC initiative at the individual level using pooled cross-sectional surveys."

### Comment #3:

#### 3. Introduction

**• The information on the main study was presented quite too much, e.g. paragraph 2 of page 6. This could be summarized and could be transferred to the method section**

Response to Comment #3: Per this comment, we shortened the description of the TFC initiative in the Introduction section and moved the description of the main study to the Methods section.

### Comment #4:

**• The worldwide information on this topic could be added instead in the introduction**

Response to Comment #4: Per this comment, the comparison on SHS exposure in the indoor workplaces, restaurants, and home between China and other countries were added to the first paragraph of the Introduction. The revised paragraph is now written as, "Secondhand smoke (SHS)

exposure is a major preventable cause of diseases for infants, children, and nonsmoking adults.<sup>1-3</sup> It has been well documented that there is no risk-free level of exposure to SHS.<sup>2</sup> As the largest tobacco consumption country in the world, China has more than 300 million smokers, exposing an estimated 740 million people to harmful environmental tobacco smoke.<sup>4</sup> According to the findings from a cross-country comparison of past-30-day (P30D) SHS exposure among adults in 14 countries (Bangladesh, Brazil, China, Egypt, India, Mexico, the Philippines, Poland, Russia, Thailand, Turkey, Ukraine, Uruguay and Vietnam), China had the highest rates of P30D SHS exposure in indoor workplaces (63.3%) and restaurants (88.5%) in 2010.<sup>5,6</sup> In addition, the rate of P30D exposure to SHS at home in China (67.3%) was the second highest among these 14 countries,<sup>5</sup> resulting in more than 164 million children being exposed to SHS at home in 2010.<sup>7</sup>

**Comment #5:**

**• The intervention applied in this project is fully comprehensive or partial? Limitation of the tobacco control law in China? (this point could be used as a reason to implement the project)**

Response to Comment #5: The intervention in our study, the Tobacco Free Cities (TFC) initiative, did not include all components of a comprehensive tobacco control program as recommended by the Framework Convention on Tobacco Control (FCTC) of World Health Organization (WHO). As described in our manuscript, our TFC initiative included adoption of city- or business-wide smoke-free policies, communication strategies to increase knowledge on harms of tobacco use, and provision of smoking cessation services. It did not address other key components of a comprehensive tobacco control program, such as efforts to increase tobacco tax and efforts to adopt warning labels, as these are under the jurisdiction of the Chinese Central Government. In addition, the city- or business-wide smoke-free policies in our study often focused on certain types of venues in public indoor places. Not all of these policies covered all public indoor places. Our results show that even these “partial” interventions had the potential to reduce exposure to SHS. This suggests that comprehensive interventions as recommended by WHO FCTC guidelines would have even larger impact on reducing exposure to SHS in China.

**Comment #6:**

**4. Method section**

**• Line 36 page 7: please make sure that the term “SHS exposure” in the study is the SHS exposure in the past 30 days? □ it should be consistent in the whole document**

Response to Comment #6: Per this comment, the description of our study outcomes has been revised to “past-30-day (P30D) SHS exposure in indoor workplaces, restaurants, and homes” throughout the revised manuscript.

**Comment #7:**

**• The definition of SHS in relation to current smokers: were current smokers considered exposed to SHS when they smoked themselves? Were they exposed in the workplace at the smoking places? Did you clean for logical in the data?**

Response to Comment #7: The question regarding SHS exposure at the workplace in the past 30 days was asked to participants who reported worked in indoor areas. The question was “During the past 30 days, did anyone (including yourself) smoke in indoor areas where you work?” Therefore, smokers who smoked in indoor workplace areas were considered as exposing themselves to SHS in our study. Per this comment, the description of outcomes was revised to clarify that smokers who smoked in indoor venues were considered as exposing themselves to SHS.

**Comment #8:**

**• The category of ““factory, business, agriculture, and service industry employee,” is too broad as the probability of exposure to SHS could be very different in factory workers than in service industry employees.**

Response to Comment #8: We appreciate this insightful comment. We agree with the reviewer that the likelihood of exposure to SHS could vary across these occupational categories. Unfortunately, due to the limitation of TQS survey instrument, which grouped “factory, business, and service industry employees” as one single category, we were unable to estimate whether the SHS exposure was different between factory workers and service industry employees.

The TQS survey grouped “Government employee, teacher, healthcare provider” into one single category because most schools and hospitals in China are government-owned. On the contrary, those work in “factory, business, agriculture, and service industry” are generally considered to be working in the private sector in China. The rationale for this grouping method was because existing

smoke-free policies in China were generally implemented in government buildings, schools, and hospitals. This grouping method was consistent with the method used in other national level surveys in China, such as the 2015 and 2018 China Adult Tobacco Survey (CATS). We clarified the rationale of this grouping method in the revised Methods section.

**Comment #9:**

**• Please report more detail on the missing information in line 27, page 8. Should the listwise technique better in this case because the total n was massive and it is cluster sampling (you reported the 95%CI)**

Response to Comment #9: We appreciate this comment. The reasons that we used pairwise deletion rather than listwise deletion in our analysis were: first, pairwise deletion enabled us to make more use of the data available. Unlike listwise deletion that removes subjects that have missing values on any key variable, pairwise deletion only excludes the specific missing values from the analysis rather than the entire subject. Second, for each venue, the proportion of participants with any missing value for the key variables was less than 5%. Therefore, it is reasonable to use pairwise deletion to handle missing values, assuming that missing was at random (MAR) after controlling for other variables in this study. Listwise deletion assumes values missing completely at random (MCAR), which is much stronger than the MAR assumption.<sup>8</sup> Per this comment, we provided details on proportion of missing values for key variables in the Methods section.

**Comment #10:**

**• Please provide the total number of participants of the whole project and then the valid n has been used for analysis**

Response to Comment #10: Per this comment, the total number of participants (10,184 in 2015 and 10,233 in 2018) was added to the “Study design and survey participants” in the Methods section. The valid n has been used for analysis was also added in corresponding tables and reported in the Results section. There were 4,710 and 5,011 respondents who usually worked indoors and reported P30D SHS exposure in indoor workplaces in 2015 and 2018, respectively. There were 6,576 and 6,878 respondents who had visited any indoor areas of restaurant in the past 30 days and reported SHS exposure in 2015 and 2018, respectively. There were 9,943 and 10,086 respondents who reported P30D SHS exposure at home in 2015 and 2018, respectively. As shown in Table 4, the number of observations used for multivariate logistic regression for each venue was 9,587, 13,239, and 19,721 for indoor workplaces, indoor areas of restaurants, and homes, respectively. The Supplementary Table 2 was also updated with the total number of participants in each year per this comment.

**Comment #11:**

**5. Results section**

**• This should be more interesting if you could present an overall picture of SHS exposure: % exposure at only 1 venue? 2 venues? All 3 venues**

Response to Comment #11: We appreciate this comment. In our study, only participants visited each venue in the past 30 days were asked to report their P30D SHS exposure at each venue, as such, the sample used to estimate SHS exposure at one specific venue was different the sample of a different venue. For example, although all participants were asked about SHS exposure at home, only people working in the indoor workplace were asked about P30D SHS exposure in the workplace. Similarly, only people who visited restaurants in the past 30 days were asked about P30D SHS exposure in the restaurants. Consequently, it may not be appropriate to report the overall SHS exposure based on % exposure at only 1 venue, 2 venues, and all 3 venues.

**Comment #12:**

**• Please consider the current smoking status? People should be exposed when they smoked.**

Response to Comment #12: We agree that smokers were exposed to secondhand smoke when they smoke. In our TQS questionnaire, respondents were asked if they had noticed anyone (including themselves) smoking inside of a venue that they visited in the past 30 days. As such, smokers who smoked were counted as being exposed to SHS in our study. The definitions of our outcome variables were revised to clarify this point per this comment. In addition, exposure to SHS among smokers and nonsmokers at each venue were reported. Current smoking status was controlled for in our regression analysis.

**Comment #13:**

**• Could you consider to run the model stratify by gender and smoking status? This should make the information more valuable**

Response to Comment #13: This is a great suggestion. We agree that the general association may mask important subgroup differences, such as those defined by gender and smoking status. Per this comment, subgroup analyses were conducted to examine these differences. Results from subgroup analysis are presented in the supplementary tables 4 to 9. We did not find significant differences in SHS exposure changes between men and women, nor did we find significant differences between current smokers and current non-smokers. These results were discussed briefly in the Results section in our revised manuscript.

**Comment #14:**

**• The categorized of occupation should be re-consider because this factor could influence the SHS exposure in home/workplace**

Response to Comment #14: We agree that respondents with different occupations may have different rates of SHS exposure at home. As we explained in the response to Comment #8, “factory, business, and service industry employees” were grouped as one single category in the TQS questionnaire. Due to the limitation of the survey instrument, we were unable to estimate whether the SHS exposure was different between factory workers and service industry employees. We clarified the rationale of this grouping method in the revised Methods section.

**Comment #15:**

**• Provide the n of table 4?**

Response to Comment #15: Per this comment, the n was added to the column heading of Table 4.

**Comment #16:**

**• In Table 4, maybe explain why the age group of >65 and the occupation group of “not in the labor force” were used as reference.**

Response to Comment #16: We appreciate this comment. We chose the reference groups primarily based on our research question and interests. We found that, in general, the estimated prevalence of SHS exposure was the lowest for respondents in the age group of >65 in the workplaces (41.66% in 2015 and 25.39% in 2018), restaurants (59.40% in 2015 and 59.36% in 2018), and homes (33.94% in 2015 and 28.79% in 2018), except for that of SHS exposure in restaurants in 2018. Therefore, the senior group was chosen as the reference group with which other age groups (young and middle-aged groups) were compared. Similarly, those not in the workforce were the group that was least likely to be exposed to SHS, so, they were used as the reference group with which other groups (currently employed) were compared.

**Comment #17:**

**6. Discussion section**

**• Page 14, line 42, is there any explanation why the education level would influence the exposure in restaurants?**

Response to Comment #17: We appreciate this comment. Given that education level was positively correlated with socioeconomic status (SES), respondents with higher SES may be more likely to dine in the restaurants that were more likely to have smoke-free air policies, or the policies were better implemented/enforced. An explanation was added to the revised Discussion section per this comment.

**Comment #18:**

**• Again similar question about why current smokers have higher exposure to SHS**

Response to Comment #18: We appreciate this comment. Smokers may be more likely to have peers who smoke, therefore exposing themselves to the SHS produced by their smoking peers. In addition, smokers would be exposed to SHS if they themselves smoked in the venues in the past 30 days. An explanation was added to the revised Discussion section per this comment.

**Comment #19:**

**• This part should be improved by discussing with other countries/studies about the SHS situation and the declining trend over the years. Table S4 compared to only the China nationwide survey**

Response to Comment #19: We appreciate this comment. Per this comment, we summarized the data on SHS exposure in a few selected countries (please see the attached table below). Generally,

Asian countries in this table showed a decline in exposure to SHS over time.<sup>9</sup> For example, in Vietnam, the SHS exposure declined in the indoor workplaces (from 55.9% to 42.6%), restaurants (from 84.9% to 80.7%), and at homes (from 73.1% to 59.9%) between 2010 and 2015. In Philippines, SHS exposure decreased in the indoor workplaces (from 32.6% to 21.5%), restaurants (from 33.6% to 21.9%), and at homes (from 54.4% to 34.7%) from 2009 to 2015. Although these data provided important context for cross-country comparison of SHS exposure, we do not believe it is appropriate to make such comparisons. The reasons against such comparison included: first, each of these countries have adopted tobacco control policies that differ substantially both in terms of the scope and the timing, therefore, comparing the trend of SHS exposure across these countries would not have revealed whether and to what extent the change in SHS exposure was due to specific policies. Second, although located in Asia, many of these countries have different culture and social norms towards smoking, which may influence SHS exposure, as such, cross-country comparison would not be appropriate without considering these differences. Per this comment, we added the following sentence to summarize SHS exposure in other Asian countries without drawing conclusions from cross-country comparisons. “This declining trend in SHS exposure in workplaces, restaurants, and homes in China was similar to those observed in other Asian countries that have adopted smoke-free policies over the past decade.”<sup>9</sup>

Country	Year	Workplace	Restaurant	Home
Bangladesh	2009, 2017	62.2, 42.7	79.7, 49.7	50.9, 39.0
India	2009, 2016	29.9, 32.7		52.3, 38.1
Thailand	2009, 2011	27.2, 30.5		
Philippines	2009, 2015	32.6, 21.5	33.6, 21.9	54.4, 34.7
Vietnam	2010, 2015	55.9, 42.6	84.9, 80.7	73.1, 59.9
Romania	2011, 2018	34.2, 10.1	86.6, 7.5	34.2, 10.1
Russia	2009, 2016	34.9, 21.9	78.6, 19.9	34.7, 23.1
Turkey	2008, 2012, 2016	37.3, 15.6, 10.6	55.9, 12.9, 12.7	56.3, 38.3, 26.7
Ukraine	2010, 2017	24.6, 10.5	59.1, 21.2	14.2, 7.3
Mexico	2009, 2015		29.6, 24.6	17.3, 12.6
Uruguay	2009, 2017	16.5, 11.1	4.4, 2.8	29.2, 20.0

**Comment #20:**

**• The discussion on the effects of intervention should be added. Comparing with other intervention?**

Response to Comment #20: We appreciate this comment. Our study findings were based on pooled cross-sectional data, as such, we could not estimate the causal effect of the TFC initiative on exposure to SHS. Consequently, we do not believe it is appropriate to discuss the effects of intervention and compare with other interventions.

**Reviewer: 2**

Reviewer Name: Genevieve Sansone  
 Institution and Country: University of Toronto, Canada

**Overall comments:**

**This is a well-written, well-organized paper examining the impact of city-level smoke-free initiatives in five cities in China – where the toll of smoking and SHS is the greatest in the**



world. This paper adds to the existing literature by using pre-post data to evaluate the effectiveness of an intervention; including analyses by subgroups to examine differences in SHS exposure according to education, income, sex, and occupation. In particular, the findings on gender were interesting and point to the need for further research on tobacco control interventions to reduce smoking among men, who are more likely to expose other members of their household to their smoking at home. The findings on occupation type were also interesting and important for examining the impact of any workplace smoking bans.

Response: We appreciate the overall positive evaluation of our manuscript by Dr. Sansone. We are deeply grateful for her constructive comments/suggestions, which we have incorporated into our revised manuscript. We believe our revised manuscript has been significantly improved because of her comments/suggestions. Below we provided a point-by-point response to her comments/suggestions. We hope Dr. Sansone and the journal editors will find our responses satisfactory.

Specific comments:

**Comment #1:**

**Introduction - Paragraph 3, lines 32-35 – please provide the year for the data cited on workplace and home exposure in the text**

Response to Comment #1: Per this comment, the year (2015, China Adult Tobacco Survey) was added for the data on the secondhand smoke exposure at workplace and home.

**Comment #2:**

**When naming the five cities selected for the TFC initiative, the authors use the wording “including” before naming the cities. This implies there were more cities that participated in the TFC initiative, besides the five listed here. Is this the case? If it was only implemented in the five cities that were included in this study, please delete the word “including” from the Introduction (paragraph 3, line 40) and Abstract (under “Setting”)**

Response to Comment #2: We appreciate this comment. Chengdu, Chongqing, Wuhan, Xiamen, and Xi'an were the only five cities participated in the TFC initiative from 2015-2018. We deleted the word “including” in the Introduction section and in Abstract per this comment.

**Comment #3:**

**Introduction – Paragraph 5 describes what is known about SHS exposure in China from previous studies. The authors state that there is a lack of representative, citywide data and only small randomized controlled trials have been done. I would suggest to include findings from Fong et al. (2015) and Sansone et al (2019), in which data from a large, representative sample of smokers in several large Chinese cities in the ITC Survey were used to evaluate trends in SHS exposure in public places over time. The 2019 study also expanded to include data from non-smokers and rural areas – which the authors mention is lacking from other studies. These references are already included in the citation list but should be mentioned in this paragraph as well.**

• Fong GT, Sansone G, Yan M, et al. Evaluation of smoke-free policies in seven cities in China, 2007–2012. *Tobacco Control* 2015;24:iv14-iv20.

• Sansone G, Fong GT, Yan M, et al. Secondhand smoke exposure and support for smoke-free policies in cities and rural areas of China from 2009 to 2015: a population-based cohort study (the ITC China Survey). *BMJ Open* 2019;9:e031891. doi: 10.1136/bmjopen-2019-031891

Response to Comment #3: We appreciate this suggestion. The findings from these two studies are now highlighted and cited in this paragraph. The revised paragraph is now written as:

“This study aims to conduct a preliminary assessment of the effectiveness of the TFC initiative in reducing SHS exposure in indoor areas. **Studies using the International Tobacco Control (ITC) data indicated that partial smoke-free air policies had minimal impact on reducing SHS exposure in indoor workplaces, restaurants, and bars in China.**<sup>10 11</sup> Although a few small randomized controlled trials had demonstrated the effectiveness of tobacco control intervention programs in reducing SHS exposure in China,<sup>12 13</sup> the evidence on the effectiveness of citywide tobacco control activities in reducing SHS exposure in China is scarce. In addition, because smoking behavior and its determinants may differ considerably between urban and rural areas in China, the overall SHS exposure status at the national level may mask the differences across regions and population subgroups.<sup>14 15</sup> Importantly, due in part to a lack of representative, citywide data on SHS exposure in China, very little is known about SHS exposure in large cities, where the population

is more concentrated than small cities and rural areas, and SHS exposure may be more pronounced. **One study using the ITC data from seven cities and five rural areas of China found that from 2009 to 2015, there were no significant differences in SHS exposure between smokers and non-smokers except that SHS exposure among smokers was higher than among non-smokers in rural workplaces.<sup>11</sup>**

**Comment #4:**

**Results - The results show that smoking prevalence did not change in these cities from 2015 to 2018 despite the TFC initiative (23% at each time point). In addition, while SHS exposure rates decreased significantly from 2015 to 2018, SHS levels remain high overall. This could be highlighted or discussed in the Discussion section. For example, if SHS decreased but smoking did not, does this mean people may be smoking elsewhere? Is this a possible reason why SHS exposure in the home did not decrease as much in the five cities as it did nationally over this period, according to Suppl Table 4, and why it did not decrease among men or among smokers according to Table 3? Given that some critics of smoke-free policies covering public places argue that such policies will result in a displacement of smoking into the home, it may be important to mention these findings and provide any explanation for why the current findings appear to be not as strong for home SHS exposure.**

Response to Comment #4: We appreciate this very insightful comment. Indeed, we also noticed that the decrease in SHS exposure at home in our five study cities (39.8% to 34.7%) was less than that at the national level (57.1% to 44.9%) between 2015 and 2018, and that smoking prevalence did not change in these cities from 2015 to 2018 despite the TFC initiative. As this reviewer pointed out, our results were consistent with the hypothesis of substitution of smoking in public indoor places with home smoking because of the implementation of smoke-free policies at public indoor places. However, to test this hypothesis, individual level longitudinal data are needed to examine whether prohibiting smoking in public indoor places had the unintended consequences of increasing smoking at home. All we can say at this point is that our results could not refute this hypothesis. We added a few sentences to discuss this point in the revised Discussion section. "It is notable that the decrease in SHS exposure at home in our five study cities (from 39.8% to 34.7%) was less than that at the national level (from 57.1% to 44.9%) between 2015 and 2018, and that smoking prevalence did not change in these five cities from 2015 to 2018 despite the TFC initiative. Although these results were consistent with the hypothesis of substitution of smoking in public indoor places with home smoking because of the implementation of smoke-free policies at public indoor places, individual level longitudinal data are needed to examine whether prohibiting smoking in public indoor places had the unintended consequences of increasing smoking at home."

**Comment #5:**

**Discussion – paragraph 1, lines 30-31 – this is a good point to make comparing trends in SHS exposure in the cities in this study with previous results reported from the nationwide survey. However, the current wording is unclear that this comparison occurs over the same time period (as is clearly shown in Supplemental Table 4). Please add “over the same time period” to the phrase “Compared with the overall levels of SHS exposure reported in the nationwide surveys.”**

Response to Comment #5: This sentence has been revised per this comment. The revised sentence is now written as "In addition, compared with the overall levels of P30D SHS exposure reported in the nationwide surveys over the same time period,<sup>16 17</sup> the decline of P30D SHS exposure in indoor workplaces and indoor areas of restaurants was significantly larger in these five TFC cities (see Table 5), indicating the potential effectiveness of the TFC initiative in reducing SHS exposure in public indoor areas in large Chinese cities."

**Comment #6:**

**Methods - I am interested in knowing more about differences across the five cities that participated. It appears from Supplemental Table 1 that Xi'an is the only city that implemented a city-wide smoke-free law, whereas other cities were only sector-wide. This could provide an opportunity to compare results in Xi'an with the other cities to see if the impact on SHS exposure was greater in Xi'an. The authors should clarify: a) that the results presented are among all cities combined only, thus results represent an average across the cities; and b) why there is no examination of differences across cities (or clarify if this was indeed done).**

Response to Comment #6: We appreciate this comment. Indeed, the results presented in Table 4 showed the combined average decrease in SHS exposure among all five study cities at each venue. The overall association may mask important differences across these five cities. Per this comment, separate analyses for each city were conducted to examine the potential differences in SHS exposure across these cities. The results from city specific analysis were presented in supplementary tables 10 to 12. These results show that there were no significant differences across cities in terms of SHS exposure changes between 2015 and 2018. As this reviewer correctly pointed out, Xi'an is the only city that have citywide smoke-free air policies. However, the city-wide smoke free laws covering indoor public places in Xi'an became effective only in August 2018, after our second-wave data collection had already completed (July 2018). Therefore, we were not able to examine the effect of city-wide smoke free laws in Xi'an.

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## VERSION 2 – REVIEW

<b>REVIEWER</b>	Phong K Thai & Long K Tran QAEHS, Queensland Alliance for Environmental Health Sciences, The University of Queensland, Brisbane, Australia
<b>REVIEW RETURNED</b>	24-Nov-2020

<b>GENERAL COMMENTS</b>	<p>The manuscript is well revised and we are satisfied with the revision. One point could be considered to amend in the main text. The regression model stratified by current-smoking status showed very interesting results. In the workplace and home environment, the SHS decreased in the current smoking group greater than in the current non-smoking group over time. The contrast pattern was found in the restaurant's environment. These findings somehow help to explain while the smoking prevalence did not decrease between two campaigns but the SHS statistically decreased (comment of reviewer 2). The decrease could be explained by the decline in SHS of the non-smoking group. This fact also emphasised the effect of the intervention in raising community awareness on SHS. The smoker may smoke in the "right" place more and the non-smoking people had an attitude of protecting themselves.</p>
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<b>REVIEWER</b>	Genevieve Sansone University of Toronto, Canada
<b>REVIEW RETURNED</b>	20-Nov-2020

<b>GENERAL COMMENTS</b>	<p>The authors have done a thorough job of responding to each of the reviewer's comments and updating the manuscript accordingly. I appreciate the authors' responses and efforts to address each of the points that I had made in the previous review, including the additional analyses that were conducted and the addition of the ITC survey findings.</p> <p>I believe each of my previous points have been sufficiently addressed in the revision, and I do not have any new comments.</p> <p>My only suggestion is with respect to the authors "Response to Comment 6" for Reviewer 2. The authors explained that Xi'an implemented their city-wide smoke-free law AFTER the Wave 2 survey was conducted, thus they could not evaluate in this study whether the stronger law in Xi'an had a greater impact compared to the TFC initiatives in the other four cities. Given the fact that I had raised this question about Xi'an in the first draft and required explanation, I think the authors could note this important point somewhere in the manuscript to be clear to readers - perhaps as a footnote to Supplementary Table 1, or in the Methods section where the timeline of the survey in relation to the TFC initiative is described.</p>
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## VERSION 2 – AUTHOR RESPONSE

### REVIEWER COMMENTS

**Reviewer: 1**

**Comments to the Author**

The manuscript is well revised and we are satisfied with the revision. One point could be considered to amend in the main text. The regression model stratified by current-smoking status showed very interesting results. In the workplace and home environment, the SHS decreased in the current smoking group greater than in the current non-smoking group over time. The contrast pattern was found in the restaurant's environment. These findings somehow help to explain while the smoking prevalence did not decrease between two campaigns but the SHS statistically decreased (comment of reviewer 2). The decrease could be explained by the decline in SHS of the non-smoking group. This fact also emphasised the effect of the intervention in raising community awareness on SHS. The smoker may smoke in the "right" place more and the non-smoking people had an attitude of protecting themselves.

**Response to Reviewer 1:**

We appreciate the careful review of our revision by this reviewer and are glad to hear that this reviewer was satisfied with our revision.

As this reviewer correctly pointed out, in the workplace and home environment, the magnitude of decrease in the SHS exposure (based on the point estimates) in the current smoking group was larger than in the current non-smoking group over time. However, this difference was NOT statistically significant. Similarly, the difference in the magnitude of the point estimates between the current smoking group and current non-smoking group in SHS exposure in restaurants was again NOT statistically significant. As such, we don't believe these results would help explain why the smoking prevalence did not decrease between two campaigns but the SHS exposure decreased significantly. Nevertheless, our main results are consistent with the hypothesis that smokers may have smoked more at homes due to restrictions at workplaces and restaurants. We specifically discussed this point in our revised Discussion section (at the end of paragraph #1).

**Reviewer: 2**

**Comments to the Author**

The authors have done a thorough job of responding to each of the reviewer's comments and updating the manuscript accordingly. I appreciate the authors' responses and efforts to address each of the points that I had made in the previous review, including the additional analyses that were conducted and the addition of the ITC survey findings.

I believe each of my previous points have been sufficiently addressed in the revision, and I do not have any new comments.

My only suggestion is with respect to the authors "Response to Comment 6" for Reviewer 2. The authors explained that Xi'an implemented their city-wide smoke-free law AFTER the Wave 2 survey was conducted, thus they could not evaluate in this study whether the stronger law in Xi'an had a greater impact compared to the TFC initiatives in the other four cities. Given the fact that I had raised this question about Xi'an in the first draft and required explanation, I think the authors could note this important point somewhere in the manuscript to be clear to readers - perhaps as a footnote to Supplementary Table 1, or in the Methods section where the timeline of the survey in relation to the TFC initiative is described .

Response to Reviewer 2: We are very grateful to this reviewer for her constructive comments/suggestions, which we believe have helped significantly improve our manuscript. Per this comment, we add a note about the timing of Xi'an citywide smoke-free law to the Supplemental Table 1.