

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association between Participation in the Government Subsidy Program for Domestic Travel and Symptoms Indicative of COVID-19 Infection in Japan: Cross-sectional Study
AUTHORS	Miyawaki, Atsushi; Tabuchi, Takahiro; Tomata, Yasutake; Tsugawa, Yusuke

VERSION 1 – REVIEW

REVIEWER	Paul Henery University of Glasgow, MRC/CSO Social and Public Health Sciences Unit
REVIEW RETURNED	15-Feb-2021

GENERAL COMMENTS	<p>This is a pertinent study addressing one of the government-subsidised economic stimulus programs implemented in the wake of the first wave of the COVID-19 pandemic. The findings are highly relevant in the current climate, and carry implications for policy both in Japan and worldwide. I am happy to recommend this manuscript for publication if the following comments are addressed.</p> <p>The authors describe in detail the random sampling method for the JACSIS survey. However, it was not stated how individuals were targeted. How did the research agency find individuals matching the criteria: for example, via a national administrative database, or via individuals electing to join the agency? The latter would present further problems with selection bias which should also be addressed in the limitations.</p> <p>The algorithm for excluding individuals based on inconsistent responses sounds novel and I would be interested in it being described more thoroughly, either in-text or via an appendix. This is also important for follow-up research which replicates the study design.</p> <p>The only excluded participants appear to be individuals identified with the algorithm as mentioned above. This would presumably leave a sizeable number of participants who have missing data for some variables. How did the authors deal with the problem of missing data?</p> <p>The description of the exposure variable (participation in the subsidy program for domestic travel) would benefit from being more explicit. From my understanding this would encompass anyone who has participated in at least one incidence of travel or accommodation subsidised by the program, but I may be wrong, and other readers may also be unsure.</p>
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	<p>The authors include household income based on data in 2019. Given the impact of the pandemic on employment status, and disproportionately in certain sectors, this variable may not be entirely indicative of income in 2020. Did the authors have access to a more recent household income variable? If not, this should be acknowledged in the limitations. If the JACSIS survey contains a question regarding subsidy from Japan's furlough scheme, this may alleviate any potential problems if included.</p> <p>The authors adjust for prefecture-level fixed-effects. Did they consider running separate analyses for each prefecture, to ascertain whether the relationship between participation and COVID symptoms vary regionally? This would potentially be very interesting.</p> <p>JACSIS is clearly a high-quality survey, and the authors have addressed most of the common concerns regarding survey data; however, a study using administrative data would invariably be of higher quality and present a reduced risk of bias. In particular, I am concerned at potential response bias; those participating in the subsidy program may underplay COVID-19 symptoms whether unconsciously or not. This is not present in the limitations, and should be addressed.</p> <p>Further to my comment above, an administrative data-based study would allow authors to use objective COVID-19 outcomes, such as positive serology data or primary care records. Is it possible to link JACSIS to such data? Are the authors planning to conduct further study in this area using administrative data if and when available? This could potentially be mentioned as future research in the discussion.</p> <p>The "secondary analysis" section consists exclusively of sensitivity analyses, and should be labelled as such.</p> <p>Two sensitivity analyses adjust for further variables not included in the main model (fear of COVID-19 infection, and individuals who had not traveled in the month preceding the survey). There is no clear rationale given as to why these were not included in the main analyses. If they simply were not associated with the outcome, I would state this in the methods; there is no need to bring them up again in the results.</p> <p>The Japanese government has now suspended the subsidy program; the discussion should be updated to reflect this.</p> <p>I have concerns with the authors' conclusion that individuals with low-risk of infection should continue to be encouraged to engage in economic activities that risk contact with others. It has been documented that low-risk individuals will still spread the infection to high-risk relatives or friends outside of public areas, especially when asymptomatic. I would suggest that all individuals be encouraged to stay at home as much as possible, with economic stimulus programs taking the form of relief directly to affected sectors.</p>
REVIEWER	Chris Zielinski University of Winchester, Health and Wellbeing
REVIEW RETURNED	05-Mar-2021
GENERAL COMMENTS	This study covers one of the non-pharmaceutical interventions (NPI) attempted by the Government of Japan to mitigate the economic

	<p>impact of COVID-19 on the Japanese population: a subsidy scheme incentivizing people to undertake domestic travel. Although it may seem that this scheme – applied only in Japan, according to the authors – is unlikely to have any relevance to those in other countries, in fact the study presents a methodology that could be applied to other NPIs). Consequently, it is suggested that the authors might add a section on “Possibilities of generalizing this approach to other NPIs”.</p> <p>The study is well-written and presented with full recognition of the limitations and well-known fallacies that often recur in such work.</p> <p>A few specific observations:</p> <p>Page 8 line 24: “Rakuten Insight Inc, which had approximately 2.2 million qualified individuals” – the word “had” needs expansion. Rakuten didn’t have 2.2 million employees. Also “qualified” how?</p> <p>Page 8 line 52 – perhaps I missed it, but it would be interesting to see the algorithm developed to exclude “individuals showing unnatural or inconsistent responses”</p> <p>Page 9 Outcome variables: as the authors later acknowledge, they are counting “COVID-19-like symptoms”, and not necessarily COVID-19 symptoms.</p> <p>Page 9 Adjustment variables: I am not sure if frequency of travel was included in this analysis – of those who travelled under the subsidy scheme, some people presumably travelled many times, while others only travelled once. What were the rates for the frequent travellers vis-à-vis one-time travellers?</p> <p>Page 17 lines 19-27: How can you identify “those with a higher risk of contracting COVID-19”? Those who are older certainly have a higher risk of death, as do socially disadvantaged populations. This paragraph should be reconsidered. This also arises in the last sentence of the conclusion.</p> <p>Page 18 lines 1-8: Yes, this is indeed the main strength – but this could be generalized, since the study does present a methodology that could be used to analyse other NPIs as well. This paragraph could be expanded to suggest this.</p> <p>Page 18 limitations: I would add the inability to distinguish single journey travellers from multiple journey travellers. Also, since smell and taste disorders are such good COVID-19 proxies for the analysis, why not only use these?</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Mr. Paul Henery, University of Glasgow

Comments to the Author:

This is a pertinent study addressing one of the government-subsidised economic stimulus programs implemented in the wake of the first wave of the COVID-19 pandemic. The findings are highly relevant in the current climate, and carry implications for policy both in Japan and worldwide. I am happy to recommend this manuscript for publication if the following comments are addressed.

1. The authors describe in detail the random sampling method for the JACSIS survey. However, it was not stated how individuals were targeted. How did the research agency find individuals matching the criteria: for example, via a national administrative database, or via individuals electing to join the agency? The latter would present further problems with selection bias which should also be addressed in the limitations.

The research agency (Rakuten Insight, Inc.) has a pool of approximately 2.2 million recruited individuals (registered individuals) to be used for a variety of internet surveys. For the purpose of this study, we collaborated with this company to reach out to registered individuals in a way that could be analyzed as a nationally-representative sample. We clarified this point in the Methods section as follows (page 8, paragraph 2):

"Rakuten Insight, Inc. is a research agency with a survey panel of approximately 2.2 million registered individuals in 2019. For the purpose of this study, we collaborated with this company to reach out to registered individuals in a way that could be analyzed as a nationally-representative sample [15]."

As the reviewer pointed out, individuals included in the survey were self-selected, and therefore, there are potential concerns of the sample selection bias. For instance, this population would have more digital literacy than the general population because enrollment into this agency needs Internet access. To address this issue, we applied the inverse probability weighting (IPW) method and minimized the socio-economic and health-related characteristics between the analytic sample and general population (derived from Comprehensive Survey of Living Conditions = nationwide representative government survey), as described in the main text. Having said that, to address the reviewer's concern, we discussed the potential issue of the sample selection bias in the limitation subsection of the revised manuscript (page 20, paragraph 1).

"Finally, we used the weighted analyses to address the issue that the participants were recruited from the survey panel of registered individuals in the internet research agency (to minimizing the difference in demographics, SES, and health-related characteristics between respondents of the current internet survey and the nationally representative sample). However, it is still possible that individuals included in our analyses differed from the general population in unmeasurable ways, and therefore, our findings may not be generalizable to other populations such as the population with limited access to and literacy about the internet."

2. The algorithm for excluding individuals based on inconsistent responses sounds novel and I would be interested in it being described more thoroughly, either in-text or via an appendix. This is also important for follow-up research which replicates the study design.

Thank you for giving us an opportunity to describe our method. As suggested, in our revised manuscript, we explained this approach in more detail (in Method A1 of the online appendix).

"Method A1. Management of data quality

To validate data quality, we excluded respondents showing unnatural or inconsistent responses.

(A) We excluded those who answered incorrectly for the survey item

Please choose the second from the bottom of the following options.

A

B

C

D

E

***The correct answer is D.**

(B) We excluded those participants who answered "almost every day" or "several times per week" (as opposed to "once a week," "once a month," or "never") to all nine questions asking about the use of the following substances:

(1) alcohol, (2) sleeping pills/anti-anxiety drugs, (3) prescribed narcotics for cancer pain, (4) prescribed narcotics for non-cancer pain, (5) non-prescribed narcotics, (6) inhalation of organic solvents such as paint thinner or toluene, (7) illegal herbs/magic mushrooms, (8) cannabis (marijuana), and (9) methamphetamine/cocaine/heroin.

(C) We excluded those participants who answered "currently have this condition and receiving treatment" or "currently have this condition but not receiving treatment" (as opposed to "never in the past" or "not now, but existed in the past") to all 16 questions asking about the presence of the following chronic conditions:

(1) hypertension, (2) diabetes, (3) asthma, (4) bronchitis/pneumonia, (5) atopic dermatitis, (6) periodontal disease, (7) caries, (8) otitis media, (9) angina pectoris, (10) myocardial infarction, (11) stroke, (12) chronic obstructive pulmonary disease, (13) cancer/malignant tumor, (14) chronic pain, (15) depression, and (16) mental disorder other than depression."

3. The only excluded participants appear to be individuals identified with the algorithm as mentioned above. This would presumably leave a sizeable number of participants who have missing data for some variables. How did the authors deal with the problem of missing data?

The current survey is designed so that it could not be completed if any item was not responded. In such cases, the participants were regarded as not having consented to participate in the survey and were not counted as respondents. Therefore, we did not observe missing data for any variables. We clarified this point in the Methods section.

(page 8, paragraph 2)

"They also had the option not to respond or to discontinue at any point in the survey; in such cases, they were regarded as not having consented to participate in the survey and were not counted as respondents."

(page 9, paragraph 1)

"Although there was no missing value due to the survey design described above (if any item was not responded, the survey could not be completed), there was still a possibility of unnatural or inconsistent responses."

4. The description of the exposure variable (participation in the subsidy program for domestic travel) would benefit from being more explicit. From my understanding this would encompass anyone who has participated in at least one incidence of travel or accommodation subsidised by the program, but I may be wrong, and other readers may also be unsure.

We apologize if this was not clear enough in our original submission. The reviewer's understanding is correct. We clarified this in our revised manuscript as follows (page 9, paragraph 2):

"The primary exposure variable was participating at least once in travel or accommodation funded by the subsidy program for domestic travel, which was announced on July 10, 2020, and implemented on July 22, 2020."

5. The authors include household income based on data in 2019. Given the impact of the pandemic on employment status, and disproportionately in certain sectors, this variable may not be entirely indicative of income in 2020. Did the authors have access to a more recent household income variable? If not, this should be acknowledged in the limitations. If the JACSIS survey contains a question regarding subsidy from Japan's furlough scheme, this may alleviate any potential problems if included.

Unfortunately, we did not have access to the income data for 2020. However, our survey collected the information on the receipt of lay-off or unemployment benefits after April 2020, which we believe is a good proxy for the income in 2020. In our revised manuscript, we additionally adjusted this variable for all analyses (we also kept the income level in 2019 in our models). We found that our findings did not change by adding this variable to our model, supporting the robustness of our findings.

6. The authors adjust for prefecture-level fixed-effects. Did they consider running separate analyses for each prefecture, to ascertain whether the relationship between participation and COVID symptoms vary regionally? This would potentially be very interesting.

The limited number of respondents per prefecture, unfortunately, precluded us from running separate analyses for each of 47 prefectures. Instead, to address the reviewer's comment, we ran separate analyses for five regions (47 prefectures were clustered into five widely-accepted geographical regions), and found no meaningful variation across these regions. We present the result of this analysis in our revised manuscript:

(page 14, paragraph 1)

"Finally, we ran separate analyses for five regions to ascertain whether the relationship between the subsidy program participation and COVID-19-like symptoms varied regionally."

(page 16, paragraph 1)

"There were no consistent regional variations in the relationships between the subsidy program participation and COVID-19-like symptoms (Table A7)."

Table A7. Association between Participation in the Subsidy Program for Domestic Travel and Incidence of COVID-19-Like Symptoms, Stratified by Region

	Region 1 (n=3,750)	Region 2 (n=5,589)	Region 3 (n=5,390)	Region 4 (n=3,884)	Region 5 (n=6,869)
Total conformed cases of COVID-19 as of September 1, 2020 (/ million)	169.0	790.2	339.1	663.0	394.3
High Fever					
Adjusted odds ratios (95%CI)	5.20 (1.45, 18.6)	1.19 (0.72, 1.96)	1.58 (0.73, 3.43)	2.42 (1.24, 4.72)	1.50 (0.52, 4.30)
Adjusted P value	0.04	0.49	0.24	0.048	0.45
Sore Throat					
Adjusted odds ratios (95%CI)	1.45 (0.90, 2.32)	2.23 (1.60, 3.12)	1.56 (1.08, 2.24)	1.65 (1.13, 2.40)	1.04 (0.71, 1.52)
Adjusted P value	0.13	<0.001	0.09	0.03	0.84
Cough					
Adjusted odds ratios (95%CI)	1.13 (0.72, 1.77)	2.00 (1.44, 2.77)	1.05 (0.69, 1.62)	1.27 (0.88, 1.84)	1.11 (0.77, 1.59)

Adjusted P value	0.59	<0.001	0.81	0.21	0.59
Headache					
Adjusted odds ratios (95%CI)	1.62 (1.11, 2.38)	1.42 (1.10, 1.82)	1.44 (1.05, 1.97)	0.97 (0.73, 1.30)	1.00 (0.76, 1.32)
Adjusted P value	0.052	0.02	0.10	0.86	0.98
Smell and Taste Disorder					
Adjusted odds ratios (95%CI)	0.57 (0.17, 1.93)	1.04 (0.47, 2.28)	0.40 (0.16, 1.03)	2.83 (1.30, 6.13)	3.98 (1.49, 10.6)
Adjusted P value	0.37	0.92	0.06	0.04	0.03

Division 1: Seven prefectures in Hokkaido and Tohoku District (northern region in Japan). Division 2: seven prefectures in Kanto District (Tokyo metropolitan area). Division 3: nine prefectures in Tokai and Hokuriku District (central region). Division 4: seven prefectures in Kinki District (mid-west region). Division 5: 17 prefectures in Chugoku, Shikoku, Kyusyu, and Okinawa District (southwest region).

7. JACSIS is clearly a high-quality survey, and the authors have addressed most of the common concerns regarding survey data; however, a study using administrative data would invariably be of higher quality and present a reduced risk of bias. In particular, I am concerned at potential response bias; those participating in the subsidy program may underplay COVID-19 symptoms whether unconsciously or not. This is not present in the limitations, and should be addressed.

We agree with the reviewer that those participating in the subsidy program may underplay COVID-19 symptoms. We discussed this point in the limitation subsection (page 20, paragraph 1).

"Conversely, it is also possible that those participating in the subsidy program may under-report COVID-19-like symptoms. However, if this is the case, this would bias our estimates towards the null, and the true difference in COVID-19-like symptoms between the participants and non-participants of the subsidy program would be larger than what we have estimated."

8. Further to my comment above, an administrative data-based study would allow authors to use objective COVID-19 outcomes, such as positive serology data or primary care records. Is it possible to link JACSIS to such data? Are the authors planning to conduct further study in this area using administrative data if and when available? This could potentially be mentioned as future research in the discussion.

As much as we would like to do, unfortunately, it is impossible to link the current survey data with external databases. However, prospective studies that investigate the association

between the participation in the subsidy program for domestic travel and COVID-19 incidence (identified by PCR test or administrative data) warrant. We discussed this point in the Discussion section.

"Nevertheless, further prospective studies that investigate the association between the participation in the subsidy program for domestic travel and COVID-19 incidence (identified by PCR test or administrative data) warrant."

9. The "secondary analysis" section consists exclusively of sensitivity analyses, and should be labelled as such.

Thank you for bringing this to our attention. We relabeled as such.

10. Two sensitivity analyses adjust for further variables not included in the main model (fear of COVID-19 infection, and individuals who had not traveled in the month preceding the survey). There is no clear rationale given as to why these were not included in the main analyses. If they simply were not associated with the outcome, I would state this in the methods; there is no need to bring them up again in the results.

We agree to the reviewer that the rationale was unclear as to why we did not include fear of COVID-19 infection in the main model. We believe that fear of COVID-19 infection, as a proxy of risk preference for COVID-19 infection, would be associated with the outcomes. Therefore, as suggested by the reviewer, we included fear of COVID-19 infection in Model 2 in the main text. As for the individuals who had not traveled in the month preceding the survey, we did not include this factor in the statistical model; rather, we excluded these individuals to focus on the individuals who have made at least one trip. Hence, this sensitivity analysis after excluding those who did not travel is left intact in the appendix.

(page 10, paragraph 1)

"Adjustment variables

We adjusted for the respondents' demographics [22], socio-economic status (SES) [23], health-related characteristics [22], use of preventive measures (see below for details), perceived fear against the COVID-19 infection, ..."

(page 11, paragraph 2)

"The perceived fear against the COVID-19 infection was adjusted for to test whether the difference in the risk preference between participants and non-participants could explain the differences in the incidence of the COVID-19-like symptoms. It was measured on a five-point scale of "not afraid at all (0% if I were to rate the level of fear between 0% and 100%)," "not afraid (25%)," "neutral (50%)," "somewhat afraid (75%)," and "very afraid (100%)" to the question "Are you afraid of the COVID-19 infection?" "

11. *The Japanese government has now suspended the subsidy program; the discussion should be updated to reflect this.*

Thank you for pointing this out. We have updated the information on this travel subsidy program (page 7, paragraph 2).

"As a result, the Japanese government has suspended this subsidy program since December 28, 2020, but is considering resuming it (as of March 2021) [14]."

12. I have concerns with the authors' conclusion that individuals with low-risk of infection should continue to be encouraged to engage in economic activities that risk contact with others. It has been documented that low-risk individuals will still spread the infection to high-risk relatives or friends outside of public areas, especially when asymptomatic. I would suggest that all individuals be encouraged to stay at home as much as possible, with economic stimulus programs taking the form of relief directly to affected sectors.

Thank you for your great suggestion. We completely agree with the reviewer that economic stimulus policies should take the form of the relief directly to affected sectors (e.g., travel industries), rather than incentivizing individuals to engage in activities that lead to a higher risk of the COVID-19 infection. We revised the discussion and conclusion as such.

(page 18, paragraph 1)

"A better policy may be to directly provide financial assistance to affected sectors (e.g., travel industries) and encourage all individuals to stay at home until vaccinated."

(page 21, paragraph 2)

"In the midst of an economic recession due to the COVID-19 pandemic, economic stimulus policies should take the form of directly subsidizing financial loss of affected sectors or incentivizing economic activities that do not involve increase physical interactions, rather than incentivizing individuals to travel more or use restaurants."

Reviewer: 2

Dr. Chris Zielinski, University of Winchester

Comments to the Author:

This study covers one of the non-pharmaceutical interventions (NPI) attempted by the Government of Japan to mitigate the economic impact of COVID-19 on the Japanese population: a subsidy scheme incentivizing people to undertake domestic travel. Although it may seem that this scheme – applied only in Japan, according to the authors – is unlikely to have any relevance to those in other countries, in fact the study presents a methodology that could be applied to other NPIs). Consequently, it is suggested that the authors might add a section on "Possibilities of generalizing this approach to other

NPIs".

As far as we understand, NPIs are often used to describe interventions that aim to control outbreaks without using medications and vaccinations. On the contrary, the subsidy program for domestic travel introduced in Japan (and a similar program to incentivize the use of restaurants in the UK) is a form of economic stimulus that financially incentivizes people to engage in economic activities. Therefore, we did not add a section regarding the possibilities of generalizing this approach to other NPIs. However, if editors and reviewers have specific suggestions about how this should be incorporated in our manuscript, we are willing to revise as suggested.

The study is well-written and presented with full recognition of the limitations and well-known fallacies that often recur in such work.

A few specific observations:

1. Page 8 line 24: "Rakuten Insight Inc, which had approximately 2.2 million qualified individuals" – the word "had" needs expansion. Rakuten didn't have 2.2 million employees. Also "qualified" how?

We apologize if this was unclear in our original submission. Rakuten Insight Inc. is an internet survey company that is part of Rakuten group. However, Rakuten Insight Inc. is different from Rakuten Inc., and the participants of our survey were not employees of Rakuten Insight Inc. or Rakuten Inc. Instead, the participants of our survey were the general public population who were recruited by Rakuten Insight Inc.

The Rakuten Insight Inc. had a survey panel of approximately 2.2 million registered individuals, from which we recruited participants of our survey. We revised our manuscript to clarify this point:

(page 8, paragraph 2)

"Rakuten Insight, Inc. is a research agency with a survey panel of approximately 2.2 million registered individuals in 2019. For the purpose of this study, we collaborated with this company to reach out to registered individuals in a way that could be analyzed as a nationally-representative sample [15]."

(page 8, paragraph 2).

"the registered individuals are assured through annual updates of demographic information and the exclusion of individuals with concerns about incorrect information."

2. Page 8 line 52 – perhaps I missed it, but it would be interesting to see the algorithm developed to exclude "individuals showing unnatural or inconsistent responses"

Thank you. As the other reviewer pointed out, we described the exclusion algorithm above. Please see our response to comment #2 of reviewer 1 (page 2-3 of this response letter).

3. Page 9 Outcome variables: as the authors later acknowledge, they are counting “COVID-19-like symptoms”, and not necessarily COVID-19 symptoms.

We acknowledge that although the outcomes used in this study are indicative of the COVID-19 infection, they are not necessarily definitive “COVID-19 symptoms.” Even though self-reported COVID-19-like symptoms have been reported as a useful measure to monitor the spread of COVID-19 infections, it is likely that some individuals who reported five COVID-19-like symptoms had illnesses that were not COVID-19, such as influenza and the common cold. We discussed this point in the limitation subsection of our revised manuscript.

(page 19, paragraph 1)

"Third, it is likely that some individuals who reported five COVID-19-like symptoms had illnesses that were not COVID-19, as we were unable to collect data on confirmed diagnoses of COVID-19 infection (e.g., diagnoses using the PCR test)."

4. Page 9 Adjustment variables: I am not sure if frequency of travel was included in this analysis – of those who travelled under the subsidy scheme, some people presumably travelled many times, while others only travelled once. What were the rates for the frequent travellers vis-à-vis one-time travellers?

Unfortunately, the information on how many times the respondents travelled was unavailable in this survey. We discussed this point in the limitation subsection.

(page 20, paragraph 1)

"Fifth, the information on how many times the respondents traveled was unavailable, and we could not distinguish one-time travelers from frequent travelers."

5. Page 17 lines 19-27: How can you identify “those with a higher risk of contracting COVID-19”? Those who are older certainly have a higher risk of death, as do socially disadvantaged populations. This paragraph should be reconsidered. This also arises in the last sentence of the conclusion.

Based on comment #12 from reviewer #1, we revised the corresponding part as follows:

(page 18, paragraph 1)

"A better policy may be to directly provide financial assistance to affected sectors (e.g., travel industries) and encourage all individuals to stay at home until vaccinated."

(page 21, paragraph 2)

"In the midst of an economic recession due to the COVID-19 pandemic, economic stimulus policies should take the form of directly subsidizing financial loss of affected sectors or incentivizing economic activities that do not involve increase physical interactions, rather than incentivizing individuals to travel more or use restaurants."

As a result, we believe that the expression the reviewer pointed out no longer exists in our revised manuscript.

6. Page 18 lines 1-8: Yes, this is indeed the main strength – but this could be generalized, since the study does present a methodology that could be used to analyse other NPIs as well. This paragraph could be expanded to suggest this.

Thank you for your suggestion. As stated above, NPIs are often used to describe interventions that aim to control outbreaks without using medications and vaccinations. On the contrary, the subsidy program for domestic travel is a form of economic stimulus that financially incentivizes people to engage in economic activities. Because of this difference in the policy context, we did not add a section regarding the possibilities of generalizing the approach used in the current study to other NPIs. However, if editors and reviewers have specific suggestions about how this should be incorporated into our manuscript, we are willing to revise as suggested.

8. Page 18 limitations: I would add the inability to distinguish single journey travellers from multiple journey travellers. Also, since smell and taste disorders are such good COVID-19 proxies for the analysis, why not only use these?

This is an excellent point. As stated above (the response to comment #4), we discussed the inability to distinguish single journey travelers from multiple journey travelers in the limitation subsection.

(page 20, paragraph 1)

"Fifth, the information on how many times the respondents traveled was unavailable, and we could not distinguish one-time travelers from frequent travelers."

Other symptoms than smell and taste disorders were used as outcomes because they have relatively high sensitivity or specificity. We described as such in the Methods section.

(page 9, paragraph 3)

"These symptoms are reported to have high sensitivity (50% for high fever and 70% for cough) or specificity (70% for sore throat, 80% for headache, and 90% or higher for smell and taste disorder) [19]."

VERSION 2 – REVIEW

REVIEWER	Paul Henery University of Glasgow, MRC/CSO Social and Public Health Sciences Unit
REVIEW RETURNED	29-Mar-2021

GENERAL COMMENTS	Thank you for addressing my queries. I am satisfied that all of my recommended changes have been resolved, and am happy to recommend the manuscript for publication.
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REVIEWER	Chris Zielinski University of Winchester, Health and Wellbeing
REVIEW RETURNED	30-Mar-2021

GENERAL COMMENTS	Thanks to the authors for taking my suggestions on board, along with those of the other reviewer. No further comments on my part.
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