

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Inequalities in the evolution of the COVID-19 pandemic: An ecological study of inequalities in mortality in the first wave and the effects of the first national lockdown in England
AUTHORS	Welsh, Claire; Albani, Viviana; Matthews, Fiona; Bambra, Clare

VERSION 1 – REVIEW

REVIEWER	Desjardins, M.R. Johns Hopkins University Bloomberg School of Public Health, Epidemiology
REVIEW RETURNED	15-Dec-2021

GENERAL COMMENTS	<p>This study examines how inequalities impact COVID-19 mortality in England during their first wave in 2020. While the manuscript is generally well-written and the discussion provides nice information, there are a number of issues that need to be addressed before publication is warranted.</p> <p>First, this is not a true geographical analysis nor space-time analysis like the title and introduction explains. A simple linear model was employed, so we do not really know how space and/or space-time impacted the outcome that can be explored using spatial/spatiotemporal regression approaches (e.g., Spatial lag, GWR, CAR, Bayesian). Second, the model and variable selection is minimally discussed and justified. I would stay away from "geographical" inequalities if a true spatial analysis is not employed. Furthermore, I would like to see a table describing the variables selected. I believe a more rigorous modeling procedure needs to be completed, along with more elaboration on methods and uncertainty of model results. Overall, this paper provides nice findings and has merit, but needs some work before consideration.</p> <p>Minor comments are provided below:</p> <p>Line 57-"This study interrogates the evolution" sounds odd. I would replace "interrogates" with "examines".</p> <p>Line 65-"COVID-19 should be capitalized. Please check and revise throughout manuscript.</p> <p>Line 70- "modified" seems odd. Suggest replacing with "impacted".</p> <p>-There is minimal explanation of the methods in the abstract, please elaborate.</p> <p>-Lines 103-105 - This statement is not entirely true. There have been studies that have examined inequalities during the pandemic</p>
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	<p>using spatiotemporal approaches. Please review the literature and revise your introduction. For example:</p> <p>Martines, M. R., Ferreira, R. V., Toppa, R. H., Assunção, L. M., Desjardins, M. R., & Delmelle, E. M. (2021). Detecting space–time clusters of COVID-19 in Brazil: mortality, inequality, socioeconomic vulnerability, and the relative risk of the disease in Brazilian municipalities. <i>Journal of Geographical Systems</i>, 23(1), 7-36.</p> <p>Benita, F., & Gasca-Sanchez, F. (2021). The main factors influencing COVID-19 spread and deaths in Mexico: A comparison between phases I and II. <i>Applied Geography</i>, 134, 102523.</p> <p>Also, suggest using this review paper as reference:</p> <p>Franch-Pardo, I., Desjardins, M. R., Barea-Navarro, I., & Cerdà, A. (2021). A review of GIS methodologies to analyze the dynamics of COVID-19 in the second half of 2020. <i>Transactions in GIS</i>, 25(5), 2191-2239.</p> <p>Line 138: Again, "interrogation" seems odd and suggest "examination" or "study". Also, this is not the first study to examine inequalities during the pandemic. Please revise.</p> <p>-Methods: The modeling needs to be better explained. Was testing done to examine the possibility of multicollinearity? I would like to see a more rigorous description of model selection, justification, and variables included/excluded.</p> <p>-Can you address/discuss uncertainty due to lack of testing at the start of the pandemic? For example, cases and rates at the start can be a function of testing effort. Now that testing is widely available, would the results change significantly?</p>
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REVIEWER	Pilkington , Hugo Universite Paris 8, Geography
REVIEW RETURNED	22-Dec-2021

GENERAL COMMENTS	<p>This is a great paper that I thoroughly enjoyed reviewing. It shows clearly the link between area-based deprivation and the geographical differences (and the evolution of these) in Covid-19 related mortality over the intial phase of the pandemic in England. I have a few comments that may be helpful in clearing up some sections of the paper I found unclear, confusing or needing greater argumentation and I hope these help strengthen your paper.</p> <ol style="list-style-type: none"> 1) The objectives are worded in such a way that they feel like a "sell" for the paper. Please reword with something that clearly states what you actually did. 2) Please briefly describe the level of geography your are using (essentially average size and demographics of these smaller areas) 3) The index of multiple deprivation should be briefly described or an appropriate reference provided 4) Please provide at least a cursory review of the literature for the inclusion of covariates, or a precise explanation of what "empirical" knowledge you used because I had trouble understanding exactly where these covariates are coming from 5) What statistical tests did you use to test for the differences in speed of increase/decrease?
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	<p>6) Table 1 shows a linear model for cumulative death rates per 100 000 population, expressed as beta coefficients (I presume). Please specify that (or if) the coefficients are adjusted and that they are beta coefficients (if this is indeed the case)</p> <p>7) Figure 5 shows two maps of deciles of deprivation and cumulative death rates per 100 000 over the study area and period. The black and white shading of the deciles makes it very hard to distinguish between deciles 1 to 10 on the left-hand map. I suggest you find some visual technique for combining both variables into one map, I think it would convey a much stronger and quicker message - but it may also be quite confusing. It is, I feel, worth a try</p> <p>8) In the discussion, I fully adhere to the fact that there were (are) two phases of the epidemic gaining a foothold in an area: an initial introduction due to rather more advantaged groups of individuals (i.e. travelers, vacationers returning from ski resorts, etc.) and a second, more detrimental phase where the virus compounds the preexisting population and area vulnerabilities, producing in turn greater levels of mortality. You also identify that geographical inequalities in mortality are compounded, possibly, through a the mediating factors of vulnerability, susceptibility, exposure and transmission. So why not go ahead and add that we are most likely within a syndemic framework?</p> <p>9) Furthermore, although in this case I agree that mandatory measures (that you state have little room for individual agency) have probably reduced the burden of mortality in an acute public health emergency such as this one, other pathologies/conditions (such as HIV/Aids but also recent outbreaks of Ebola) have shown otherwise. So I would nuance this somewhat (i.e. that the scale of the emergency, the size of susceptible population, the speed of transmission, the hospital (under-)capacity, etc. left little room for non-mandatory measures)</p> <p>10) One final thought for the limitations section. Is it at all possible that more deprived areas were more intent in looking for Covid-related deaths early on in the epidemic and this might possibly explain why these were identified there earlier (you say that reporting of deaths was based on date of registration rather than date of death)?</p>
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REVIEWER	Chowell, Gerardo Georgia State University, Population Health Sciences
REVIEW RETURNED	06-Jan-2022

GENERAL COMMENTS	<p>Review comments 'Geographical inequalities in the evolution of the COVID-19 pandemic: An ecological study of inequalities in mortality in the first wave and the effects of the first national lockdown in England'. The study aims to examine how the geographical inequalities in COVID-19 mortality rates evolved in England and the impact of the first national lockdown. This is an important study that adds knowledge base on geographical inequalities in COVID-19 mortality. Some comments for improvement are:</p> <p>A. Abstract: Please replace terms like 'tended to be', 'somewhat' and 'sufficiently strong' by a more precise/quantitative words.</p> <ul style="list-style-type: none"> • Result- <ul style="list-style-type: none"> o Give the full-form of IMD that is used first time in abstract. o Line 82-sentence inside the bracket (could have begun) is not clear. • Conclusion <ul style="list-style-type: none"> o the first sentence of conclusion (significant difference) does not match with the result.
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	<p>B. Introduction: Line 98-99, please mention 'most deprived vs least deprived in terms of what?' and please add COVID-19 before mortality rates in line 99.</p> <p>C. Methods:</p> <ul style="list-style-type: none"> • Line 163-166: Is not the peak and peak rate same? From this sentence, it seems like the 'peak' is different from the 'peak rate'. • First and third paragraph of method section is confusing when it comes to explaining weekly vs monthly age standardized mortality rate per authority and per IMD decile. I would suggest rewriting and avoiding unnecessary details to make it more easily understandable. • Line 154: Please include a little explanation about IMD and who publishes that data for local authority level in England. • Line 182: Please include which other metrics were included in the linear model. <p>D. Results:</p> <ul style="list-style-type: none"> • From figure 1, it looks like only authorities in 1st, 4th, and 6th, and 10th IMD rank decile recorded COVID-19 deaths in week 11 with highest proportion of reports from 1st decile. Does this mean there was no deaths reported from authorities in other six deciles? It could be mentioned in text in results as well. It would be interesting to view this in a authority level or IMD decile level map of England for week 11. • Line 216: Remove the after two. • It looks like to me that the information provided in line 217-218 is of IMD decile level rather than individual local authority. Please check. If it is of authority as mentioned in the text, can those four authorities be named, or their any other details provided? <p>E. Discussion:</p> <ul style="list-style-type: none"> • Line 328; add 's' in les • In paragraph starting from line 324 and 343, authors may also want to include a few sentences on the status of access to health system, treatment/care for different population groups in England, which is directly related to COVID-19 deaths despite the differential rate of exposure and infection in different population groups. • Have authors reviewed the most relevant recent COVID-19 studies ? See e.g: https://www.sciencedirect.com/science/article/pii/S1201971221008146#!
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. M.R. Desjardins, Johns Hopkins University Bloomberg School of Public Health

First, this is not a true geographical analysis nor space-time analysis like the title and introduction explains. A simple linear model was employed, so we do not really know how space and/or space-time impacted the outcome that can be explored using spatial/spatiotemporal regression approaches (e.g., Spatial lag, GWR, CAR, Bayesian). Second, the model and variable selection is minimally discussed and justified. I would stay away from "geographical" inequalities if a true spatial analysis is not employed. Furthermore, I would like to see a table describing the variables selected. I believe a more rigorous modeling procedure needs to be completed, along with more elaboration on methods and uncertainty of model results. Overall, this paper provides nice findings and has merit, but needs some work before consideration.

Thank you for these comments. We did not, as you say, employ spatiotemporal modelling techniques in this study. Therefore, we have altered the title to "Inequalities in the evolution of the COVID-19 pandemic: An ecological study of inequalities in mortality in the first wave and the effects of the first national lockdown in England", and we have removed all mention of geographical inequalities and

instead refer to ecological inequalities. Our unit of interest in this study was census geographies, which is why we initially referred to geographical inequalities, but we recognise how this could be misleading.

The regressions used were not employed to understand the significant risk factors acting on mortality rates, but rather to understand the role of IMD-related inequality, thus only simple models were used where the dependent variable was the epidemic curve characteristic per geographical area (e.g. the total cumulative mortality rate, or the speed of increase) and the main independent predictor was IMD. To better understand the driver of the cumulative mortality, we also used predictors that were other features of the same epidemic curve, e.g. the starting date when each area began recording deaths. These predictors were chosen based on empirical knowledge (if you start accumulating deaths earlier and accumulate them faster you're likely to end up with greater cumulative mortality, but which is more important – the early 'start' or the faster rise?). We have amended the methods to reflect this more clearly:

The purpose of these simple models was to understand the relative contribution of deprivation (measured by IMD) and relevant epidemic dynamics (e.g. date of first recorded COVID-19 deaths) to the metric of interest, therefore no model selection was employed, and covariate inclusion was based on empirical knowledge.

Line 57-"This study interrogates the evolution" sounds odd. I would replace "interrogates" with "examines".

Thank you, we have made this change.

Line 65-"COVID-19 should be capitalized. Please check and revise throughout manuscript. Thank you, we have made this change, and checked throughout the manuscript.

Line 70- "modified" seems odd. Suggest replacing with "impacted".
Thank you, we have made this change.

-There is minimal explanation of the methods in the abstract, please elaborate.

Thank you, the prescribed structure of the abstract leaves little room for methods, however we have added to the abstract in the 'Primary Outcome Measure' section:

Age-standardised COVID-19 mortality rates by local authority and decile of index of multiple deprivation, regressed on Index of Multiple Deprivation (IMD) and relevant epidemic dynamics.

-Lines 103-105 - This statement is not entirely true. There have been studies that have examined inequalities during the pandemic using spatiotemporal approaches. Please review the literature and revise your introduction. For example:

Martines, M. R., Ferreira, R. V., Toppa, R. H., Assunção, L. M., Desjardins, M. R., & Delmelle, E. M. (2021). Detecting space–time clusters of COVID-19 in Brazil: mortality, inequality, socioeconomic vulnerability, and the relative risk of the disease in Brazilian municipalities. *Journal of Geographical Systems*, 23(1), 7-36.

Benita, F., & Gasca-Sanchez, F. (2021). The main factors influencing COVID-19 spread and deaths in Mexico: A comparison between phases I and II. *Applied Geography*, 134, 102523.

Thank you, we have added these citations and amended the text to refer only to a paucity of evidence in this space for England.

Also, suggest using this review paper as reference:

Franch-Pardo, I., Desjardins, M. R., Barea-Navarro, I., & Cerdà, A. (2021). A review of GIS methodologies to analyze the dynamics of COVID-19 in the second half of 2020. *Transactions in GIS*, 25(5), 2191-2239.

Thank you, we have made this change.

Line 138: Again, "interrogation" seems odd and suggest "examination" or "study". Also, this is not the first study to examine inequalities during the pandemic. Please revise.

Thank you, we have amended the text to refer only to England.

-Methods: The modelling needs to be better explained. Was testing done to examine the possibility of multicollinearity? I would like to see a more rigorous description of model selection, justification, and variables included/excluded.

Thank you, we have amended the methods to read: The purpose of these simple models was to understand the relative contribution of deprivation (measured by IMD) and relevant epidemic dynamics (e.g. date of first recorded COVID-19 deaths) to the metric of interest, therefore no model selection was employed, and covariate inclusion was based on empirical knowledge. Any covariates falling above the threshold of statistical significance (Wald p-value >0.05 in multivariable models) would be removed from the model (however no covariates needed to be removed in this way).

Our approach was simple by design, and descriptive rather than truly interrogative of the causative effects on COVID-19 mortality. As the above passage describes, our main independent variable was IMD, beyond that we took into account only those limited epidemic dynamics that could have impacted on the metric of interest (e.g. the date of first recorded COVID-19 deaths which would have had an effect on the total cumulative mortality at a specific time). For this reason, we did not feel that an exhaustive formal model building process was appropriate.

-Can you address/discuss uncertainty due to lack of testing at the start of the pandemic? For example, cases and rates at the start can be a function of testing effort. Now that testing is widely available, would the results change significantly?

Thank you for this important point. We have added the following to the limitations section: Testing was limited to hospitalised patients in the earliest months of the pandemic in England. This may have introduced bias to our initial analyses since deaths from COVID-19 may not have been correctly attributed, had the person not received a positive test prior to death. However, we were unable to retrospectively account for this, and it would have applied to a small number of deaths in the earliest time period. Given consistency of trends across areas that began recording deaths at different stages of the national pandemic, we do not believe that this would have introduced serious bias.

Reviewer: 2

Prof. Hugo Pilkington , Universite Paris 8

1) The objectives are worded in such a way that they feel like a "sell" for the paper. Please reword with something that clearly states what you actually did.

Thank you, we have toned it down a bit and these now read: To examine how geographical ecological inequalities in COVID-19 mortality rates evolved in England, and whether the first national lockdown modified impacted them. This analysis aimed to provides evidence for important lessons to inform public health planning to reduce inequalities in any future pandemics.

2) Please briefly describe the level of geography your are using (essentially average size and demographics of these smaller areas)

Thank you, we've added the following to the methods section: (local authorities are local government organisations covering variable population sizes from just over 2000 to more than 1.5 million residents)

3) The index of multiple deprivation should be briefly described or an appropriate reference provided
Thank you, we've added the following to the methods section with an appropriate reference: (IMD, a relative measure of deprivation across multiple dimensions at small local area level)

4) Please provide at least a cursory review of the literature for the inclusion of covariates, or a precise explanation of what "empirical" knowledge you used because I had trouble understanding exactly where these covariates are coming from

Thank you, based on this and other reviewer comments, we have clarified our approach in the methods like this: The purpose of these simple models was to understand the relative contribution of deprivation (measured by IMD) and relevant epidemic dynamics (e.g. date of first recorded COVID-19 deaths) to the metric of interest, therefore no model selection was employed, and covariate inclusion was based on empirical knowledge. Any covariates falling above the threshold of statistical significance (Wald p-value >0.05 in multivariable models) would be removed from the model (however no covariates needed to be removed in this way).

5) What statistical tests did you use to test for the differences in speed of increase/decrease?

Thank you, we have clarified this point in the methods: Differences between mean speed of increase or decrease per IMD decile were assessed by non-overlap of 95% confidence intervals.

6) Table 1 shows a linear model for cumulative death rates per 100 000 population, expressed as beta coefficients (I presume). Please specify that (or if) the coefficients are adjusted and that they are beta coefficients (if this is indeed the case)

Thank you, we've changed the column heading as suggested.

7) Figure 5 shows two maps of deciles of deprivation and cumulative death rates per 100 000 over the study area and period. The black and white shading of the deciles makes it very hard to distinguish between deciles 1 to 10 on the left-hand map. I suggest you find some visual technique for combining both variables into one map, I think it would convey a much stronger and quicker message - but it may also be quite confusing. It is, I feel, worth a try

Thank you, and we agree it would be great to combine these maps into one large visually appealing and informative one, however in all our attempts to do this, inner-city areas were even harder to distinguish due to their comparatively small size. Using colours gave rise to issues for colour-blind readers, and any form of stippling/patterning also made smaller areas hard to distinguish within their boundaries. Unfortunately, we were unable to come up with a better way of presenting the two maps as one, without compromising on the readability, therefore have opted to keep the figure as-is. We would be happy to make any changes, however, if agreed with the editor that they would improve the maps' useability.

8) In the discussion, I fully adhere to the fact that there were (are) two phases of the epidemic gaining a foothold in an area: an initial introduction due to rather more advantaged groups of individuals (i.e. travelers, vacationers returning from ski resorts, etc.) and a second, more detrimental phase where the virus compounds the preexisting population and area vulnerabilities, producing in turn greater levels of mortality. You also identify that geographical inequalities in mortality are compounded, possibly, through a the mediating factors of vulnerability, susceptibility, exposure and transmission. So why not go ahead and add that we are most likely within a syndemic framework?

Thank you for this important point. We have added to the discussion on this as so: This relative risk of mortality increases for more deprived areas once transmission is established in 'phase 2' of the pandemic – due to population vulnerabilities including poverty, overcrowding and pre-existing chronic conditions(6) (6) (a so-called 'syndemic' pandemic(6)).

9) Furthermore, although in this case I agree that mandatory measures (that you state have little room for individual agency) have probably reduced the burden of mortality in an acute public health emergency such as this one, other pathologies/conditions (such as HIV/Aids but also recent outbreaks of Ebola) have shown otherwise. So I would nuance this somewhat (i.e. that the scale of the emergency, the size of susceptible population, the speed of transmission, the hospital (under-)capacity, etc. left little room for non-mandatory measures)

Thank you, we have added this point to the discussion.

10) One final thought for the limitations section. Is it at all possible that more deprived areas were more intent in looking for Covid-related deaths early on in the epidemic and this might possibly explain why these were identified there earlier (you say that reporting of deaths was based on date of registration rather than date of death)?

We are unable to say whether this was the case, however it seems unlikely given the national death reporting structures in place. Also, use of the COVID-19 diagnosis on deaths certificates was likely to begin almost simultaneously as the ICD-10 code was established, thus its early diagnosis and attribution would be unlikely to have been biased.

Reviewer: 3

Prof. Gerardo Chowell, Georgia State University

A. Abstract: Please replace terms like 'tended to be', 'somewhat' and 'sufficiently strong' by a more precise/quantitative words.

Thank you, we have made these changes and altered the wording in the Results section of the abstract to be more forthright.

- Result-

- o Give the full-form of IMD that is used first time in abstract.

Thank you, we have made this change.

- o Line 82-sentence inside the bracket (could have begun) is not clear.

Thank you, we have made these changes: By 2020-04-06 (week 15, the earliest time when the March 23rd lockdown could have begun affecting death rates) the cumulative death rate in local authorities in the two most deprived deciles of Index of Multiple Deprivation (IMD) was 54% higher than the rate in the two least deprived deciles.

- Conclusion

- o the first sentence of conclusion (significant difference) does not match with the result.

Thank you, however we respectfully disagree, as our results found significant differences in the epidemic metrics related to IMD.

B. Introduction: Line 98-99, please mention 'most deprived vs least deprived in terms of what?' and please add COVID-19 before mortality rates in line 99.

Thank you, we have made this change.

C. Methods:

- Line 163-166: Is not the peak and peak rate same? From this sentence, it seems like the 'peak' is different from the 'peak rate'.

As the methods reads, we are calculating the speed of increase from 25% of the peak, up to the peak itself. We realise that this may be confusing to read, particularly as this sentence is split over multiple lines in the draft, therefore, we have altered the wording of this passage for clarity: The speed of increase was defined as the change in mortality rate between 25% of the peak death rate and the

peak rate itself, divided by the number of weeks between them, and similarly the speed of descent was calculated using the peak death rate and subsequent reduction to 50% of this peak rate (25 and 50% selected to include time window when epidemic peaks were visibly most stable)

- First and third paragraph of method section is confusing when it comes to explaining weekly vs monthly age standardized mortality rate per authority and per IMD decile. I would suggest rewriting and avoiding unnecessary details to make it more easily understandable.

We have now moved this part of the methods into a supplement to make the methods section of the manuscript more streamlined. We realise that the methods of obtaining age-standardised rates per local authority and per IMD decile are confusingly similar, however we felt it important to describe these in full for the sake of transparency and reproducibility for readers.

- Line 154: Please include a little explanation about IMD and who publishes that data for local authority level in England.

Thank you, we have added this to the methods section with the following and a reference for IMD: The level of deprivation of each local authority was determined by the rank of average rank of the Index of Multiple Deprivation (IMD, a relative measure of deprivation across multiple dimensions at small local area level(25)).

- Line 182: Please include which other metrics were included in the linear model.

Thank you, we have altered the methods for clarity, which now reads: The purpose of these simple models was to understand the relative contribution of deprivation (measured by IMD) and relevant epidemic dynamics (e.g. date of first recorded COVID-19 deaths) to the metric of interest, therefore no model selection was employed, and covariate inclusion was based on empirical knowledge. Any covariates found to fall below the threshold of statistical significance (Wald p-value >0.05 in multivariable models) would be removed from the model (however no covariates needed to be removed in this way).

D. Results:

- From figure 1, it looks like only authorities in 1st, 4th, and 6th, and 10th IMD rank decile recorded COVID-19 deaths in week 11 with highest proportion of reports from 1st decile. Does this mean there was no deaths reported from authorities in other six deciles? It could be mentioned in text in results as well. It would be interesting to view this in a authority level or IMD decile level map of England for week 11.

Yes, you are reading Figure 12 correctly, the earliest weeks of COVID-19 death reporting in England were dominated by a small number of local authorities. The local authorities that began recording in week 11 were Oldham, Wokingham, Wolverhampton, Dudley, and Milton Keynes. Other than the 2 that are near Birmingham, and the fact that they are all in central/Southern England, there is little that links them. We considered adding a map of these authorities but it was felt to be rather uninformative, as in the context of the whole country, these non-clustered areas were hard to pinpoint or to draw any conclusions from. For these reasons, we have opted to avoid drawing attention to them via an extra plot. We would be happy to reverse this decision should the editor be in favour.

- Line 216: Remove the after two.

Thank you, we have made this change.

- It looks like to me that the information provided in line 217-218 is of IMD decile level rather than individual local authority. Please check. If it is of authority as mentioned in the text, can those four authorities be named, or their any other details provided?

Thank you for spotting this error, indeed it should be IMD deciles not local authorities. We have changed the text to reflect this.

E. Discussion:

- Line 328; add 's' in les

Thank you, we have made this change.

- In paragraph starting from line 324 and 343, authors may also want to include a few sentences on the status of access to health system, treatment/care for different population groups in England, which is directly related to COVID-19 deaths despite the differential rate of exposure and infection in different population groups.

Thank you, we have added to the text which now reads: These communities may also face barriers to health system access and differences in treatment or care (34)

- Have authors reviewed the most relevant recent COVID-19 studies ? See e.g:

<https://eur03.safelinks.protection.outlook.com/?url=https%3A%2F%2Fwww.sciencedirect.com%2Fscience%2Farticle%2Fpii%2FS1201971221008146%23&data=04%7C01%7Cclaire.welsh%40newcastle.ac.uk%7C1e03d2f7d7614b55a92d08d9f55fc71d%7C9c5012c9b61644c2a91766814fbe3e87%7C1%7C0%7C637810613981522530%7CUnknown%7CTWFpbGZsb3d8eyJWIjoiMC4wLjAwMDAiLCJQIjoiV2luMzliLCJBTiI6Iklk1haWwiLCJXVCi6Mn0%3D%7C3000&sdata=eAr7sPMkF8XPvoCEkJwZmtwZAXZYdXGQpZN9Nr0aqQg%3D&reserved=0!>

Thank you, we have added this reference. The literature was up to date with when we submitted the paper for consideration.

VERSION 2 – REVIEW

REVIEWER	Desjardins, M.R. Johns Hopkins University Bloomberg School of Public Health, Epidemiology
REVIEW RETURNED	14-Mar-2022

GENERAL COMMENTS	I commend the authors for their hard work on the revised manuscript submission and believe it is suitable for publication after addressing my comments and concerns. Nicely done!
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REVIEWER	Pilkington , Hugo Universite Paris 8, Geography
REVIEW RETURNED	27-Mar-2022

GENERAL COMMENTS	Since this is the revised version of the paper, my review will be brief. I am happy the authors found my remarks helpful and have amended their manuscript accordingly. I feel the points I raised have been covered and that no further revisions are needed.
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