

DEPARTMENT OF SOCIAL POLICY AND INTERVENTION

Barnett House, 32 Wellington Square, Oxford, OX1 2ER  
Tel: +44 (0)1865 280339

Email: [david.humphreys@spi.ox.ac.uk](mailto:david.humphreys@spi.ox.ac.uk)



Dr Emma Rourke  
The BMJ,  
BMA House,  
Tavistock Square,  
London WC1H 9JP,  
UK

27<sup>th</sup> September,  
2019

Dear Dr. Rourke and colleagues,

We are resubmitting this manuscript (BMJ-2019-050778) entitled “Media coverage of the violence epidemic in England and Wales: are we adding fuel to the fire?” to the BMJ for consideration as an *Analysis* article. We are extremely grateful to the three reviewers (Professors Hardelid, Harron and Cromwell) for their thoughtful and constructive comments as well their suggestions for how to improve and extend our analysis.

The reviewers comments could be organised in three themes. First, reviewers suggested interesting extensions to our analysis which were unfortunately beyond the scope of the article and often beyond the possibilities of what can be done with the data in the format it is available to the public. These comments have been extremely helpful in our thinking about future research and how we can investigate some of these matters in forthcoming research.

Second, the reviewers asked us to provide further clarity and discussion of the limitations of the mortality and hospital data. We’ve addressed this in three ways: (a) we have provided an extended summary of the data (with limitations) in supplementary file freely available on the Open Science Framework; (b) we have improved the clarity of the data description in the text with improvements to Box 1 and more signposts to supplementary material; and (c) for full transparency, we will upload all our data and programming code to Github to enable readers to reproduce the graphs.

Third and finally, reviewers asked us to make a number of technical changes to the way we present the data or to remove inconsistencies in naming conventions of our variables. We have addressed all of these aspects. We attach an itemised response to each of the reviewers’ points (below), explaining what changes were made or the reasons why we felt the suggested changes were not possible or necessary.

The editors will also notice that one of the authors, Professor Manuel Eisner, has withdrawn from the latest version of the manuscript. Professor Eisner felt that his contribution to the two most recent versions of the paper no longer fulfilled the authorship criteria and felt it best to withdraw. We now acknowledge Professor Eisner’s contribution to earlier drafts in the acknowledgements section.

We look forward to hearing from you.

Thank you for your consideration,



Dr. David K. Humphreys  
Lead and corresponding author  
Associate Professor,  
Department of Social Policy and Intervention  
University of Oxford, UK

Reviewer(s)' Comments to Author:

**Reviewer: 1**

Recommendation:

Comments:

This analysis paper discusses the data sources available to examine trends in violence and specifically knife crime in England and Wales - specifically, that recorded crime data are too poor to assess trends and whether media reporting may even contribute to increased violence. As a 'data nerd' I found the discussion of quality of crime data fascinating and I think this could be expanded as this was the strongest part of the paper. I have a few specific comments which I hope will improve the paper:

- 1) My main concern is regarding your conclusions drawn from the available hospital data on knife crime, which for all of England do show an increase in admissions, albeit from a historically low level in 2014. You show data for all of England when you discuss trends but in the conclusion you mention that homicides (rates?) have increased in London. On which data you base this conclusion? I think you should either present regional trends in hospital and mortality data (which would be very interesting indeed) in which case you can make inferences about London, or state that these data are not available (if this is the case). In which case I would have thought publication by NHS D and ONS of these data should be prioritised – otherwise interventions to reduce violence cannot be targeted or assessed for their effectiveness.

*Author response*

We agree that it would be desirable to provide a regional breakdown of these data, but

sadly the publicly available HES injury data are not available at this level of aggregation. We make reference to London in response to a previous editorial comment asking us to reflect more specifically on the situation in London, given the growing concern in that particular community. We have now removed the reference to London in the conclusion to avoid any confusion.

- 2) Ideally the mortality and admission rates should also be age and sex adjusted, a point which you don't mention (but I think you should). Can you present both crude and adjusted rates?

#### *Author response*

We report the total number of injuries and deaths, which encompasses all ages and sexes. Therefore, the total population is the most accurate denominator for calculating rates as it represents the true population at risk. Although the figures may disproportionately represent young adult males, the administrative data we are using are not consistently broken down by age and gender so we would be unable to adjust for age and gender using our current data sources. We agree with the reviewer that it would be of future interest to investigate whether changes in the population's age structure might partially explain some of the trends, but given the need to use alternative data sources and the relative brevity of *Analysis* articles, we opted not to investigate this here.

- 3) In relation to point 1) above, mortality may be a poor indicator of incidence of violence, since death rates may be decreasing due to improved treatment for severe injuries. A further challenge in England & Wales with recording of violent deaths is that there are substantial delay between occurrence and registration when deaths are referred to coroners  
(See <https://www.ons.gov.uk/peoplepopulationandcommunity/birthsdeathsandmarriages/deaths/methodologies/impactofregistrationdelaysonmortalitystatistics2016>). Since ONS publish mortality data by date of registration, the number of \*registered\* homicides in a particular year may reflect deaths occurring several years previously. In addition, changing hospital thresholds over time may explain changes in admission rates, although very severe injuries that require intensive care and/or an operation would still require an admission. I think all these points would be worth mentioning when you discuss the quality of these data sources.

#### *Author response*

The delays in reporting the occurrence and registration of deaths with mortality data is something we gave considerable thought to prior to submitting this manuscript. As stated here—and also in response reviewer 2 (comment #3)—the problem with mortality data is that the coding of homicides is likely to be delayed and potentially less accurate over short periods of time, usually until investigations have been resolved. Police data on homicide operate using alternative *subtractive* logic, in which cases counted as initially recorded and later revised down as the result of pending investigations are concluded.

To acknowledge this (and to limit the bias towards undercounting), we applied an approach laid out in previous studies (Rooney & Griffiths, 2004, 2013); in which we

include all deaths caused by external injury from assault (ICD codes X85-Y09) as well as all deaths caused by external injury from undetermined intent where the verdict is pending (ICD 10 code: U50.9 (before 2007 the ICD code was Y33.9)). The latter is a special purpose ICD code for accelerated registrations and represents probable homicide pending further investigations and thus helps to counteract registration delays in the mortality data.

We realise that this is a limitation of homicide data and have worked to further clarify this (concisely) in the body of the paper (pg 4). In addition, we have responded to reviewer comments by providing a description of our approach for addressing registration delays in mortality data in the methods section in the supplementary material, and a more extensive overview of the strengths and limitations of the data in supplementary Table 1. To further clarify that homicide from mortality data can be defined differently to reflect the underestimation homicides from pending cases, we have created graphical representations of the trends under different definitions (supplementary figures 1 & 2).

4) I found the section on recorded crime extremely useful and I think it could be expanded. For example, I would have liked to see plotted not just the ratio of police recorded violence to CSEW reported incidents but the actual rates or recorded and reported crime. Perhaps this can be added to Figure 2? I think it would also be very important to report how much of the increase in recorded violent crime since 2012 can be attributed to different types of crime, eg is most of the increase due to better reporting of domestic violence and stalking/harassment, or has there also been increases in recorded knife crime? Could you present the ratio of recorded: reported violent crime broken down by type?

#### *Author response*

These are very interesting ideas and something we plan to examine in a subsequent (and lengthier) article. However, it is beyond the scope of this paper to look at the ratio of crime and reported crime, or to further disaggregate the data by different crime types. As with previous comments, there are limitations to how much we can disaggregate these publicly available data. The central point of Figure 2 is to demonstrate that there have been changes to the recording police-recorded violence. We think that the current graph demonstrates this point adequately and would argue that adding further dimensions to the graph may dilute its purpose.

5) In box 1, you mention that HES includes admissions, outpatient appointments and A&E attendances – but I would flag that it is only the admissions that are well coded using ICD-10. HES A&E data is currently being replaced by the new Emergency Care Dataset (ECDS) which should contain better data on A&E attendances, including reason for attendance, going forward: <https://digital.nhs.uk/data-and-information/data-collections-and-data-sets/data-sets/emergency-care-data-set-ecds> you may want to mention this.

#### *Author response*

We are glad that the reviewer has mentioned this. As mentioned above, we have made

significant improvements to Box 1 to help clarify aspects of the data.

6) I definitely agree that the reporting of the data quality for violent crime should improve, however based on the available data (and bearing in mind that you don't present subnational data, or data on whether reported knife crime has increased based on CSEW – see point 4) I think you play down the increase in knife crime related injury admissions too much. For a rare but serious event such as this, a continuous increase since 2014 is concerning, and something that should continue to be monitored and may need to be acted on by policy makers.

#### *Author response*

The point of this paper is not to make light of any increases in knife crime, but to question whether they represent a national epidemic on the basis of the data available for a longer period of time. Using data on serious injuries related to knife injuries, we show the magnitude of this increase, when interpreted at the national level, is small in magnitude. However, in light of reviewer comments, we felt it necessary to clarify that even if this is not a national epidemic, as the media states, if the small increases in knife related injury were restricted to only a small number of areas (or to one city), then this would indeed represent a concerning localised problem with knife-related injury. We felt that this point was important in balancing the recent concerns in urban areas like London with our own concerns (and the subject of the paper) with how the changes to rates in violence has been portrayed by the media. To reflect this, we added an extra paragraph on page 5.

7) An alternative view to the one you present is that it is positive that the media does give attention to a very serious type of crime that tends to cluster strongly among vulnerable teenagers from non-white ethnic groups living in poor urban areas. An alternative might be that they ignored it until it affected a white, middle class community. Although your comment about data quality and historical context still stands.

#### *Author response*

We hope that the previous comment and the subsequent changes related to it help to address this comment.

8) Can you make clearer whether you are actually calling for a reporting guideline for violent crime similar to the WHO for suicide reporting?

#### *Author response*

We are not making a formal call for a WHO (or other) form of reporting guideline at this stage. In this manuscript we are highlighting the presence of a well-established relationship between media reporting and violence at the population level (i.e. self-directed violence, suicide). This relationship is of sufficient concern that the WHO

released specific reporting guidelines for suicide. We hope that this article will help to generate attention for the similar issue of media reporting on interpersonal violence.

Currently, there is limited debate on the possible harms of media reporting on interpersonal violence. We think it is an important issue to highlight given the substantiated evidence of harm from related violent behaviours, such as suicide and mass shootings, and our evidence showing drastic increases in media reporting on interpersonal violence. Further discussion and more research is needed to evaluate the harms vs benefits on the current approach to media reporting on interpersonal violence. Our reference to the issue is the first step in initiating such discussions.

In summary, we find it important to draw attention to the potential need for a reporting guideline for interpersonal violence – similar to the WHO’s guideline for suicide – but it would be premature to call for a specific reporting guideline at this stage.

Additional Questions:

Please enter your name: Pia Hardelid

Job Title: Associate professor

Institution: UCL Great Ormond Street Institute of Child Health

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

## Reviewer: 2

Recommendation:

Comments:

1. This is an important study that highlights the need to look at multiple sources of data when interpreting trends that could be affected by changes in data quality or systems over time. This triangulation of data sources has also been effective in other settings, e.g. child maltreatment (see, e.g. Roehrkasse et al. Administrative data and long-term trends in child maltreatment: the prospects and pitfalls. *The Lancet Public Health*, Volume 4, Issue 3, e121 - e122).

### *Author response*

We thank the reviewer for highlighting the cited commentary, which was written in response to an article we published earlier this year in the *Lancet Public Health*:

Degli Esposti, M., Humphreys, D. K., Jenkins, B. M., Gasparrini, A., Pooley, S., Eisner, M., & Bowes, L. (2019). Long-term trends in child maltreatment in England and Wales, 1858–2016: an observational, time-series analysis. *The Lancet Public Health*, 4(3), e148-e158.

This and the extensive work on temporal trends in violence from members of our team



have informed our approach to triangulating data and has motivated us to study and understand inconsistencies if and when they occur.

2. I have a few suggestions, the main one being that there should be a little more recognition of the potential pitfalls in using administrative hospital / mortality data to look at trends (where differences in recording over time can also occur, e.g. in response to changing guidelines e.g. QOF). In addition to the critique of the police data, the limitations of the hospital and mortality data used in this study should also be discussed.

#### *Author response*

We have taken this (and similar) comments onboard in the revisions. As we describe throughout these responses, we've added extra detail in the body of the text, additional information to Box 1 and have extended the supplementary files to include further discussion of the limitations of each data source. The supplementary material also covers relevant recoding changes that may affect the consistency of the data over time. We've added more explicit directions to the reader to better signpost where this further information is provided.

3. Box 1 states that there is typically a delay between death and registration of 5 days. In fact, only around 40% of deaths are registered within 5 days, and around 4% of deaths are registered the year after the death occurred. Only 7.1% of deaths to external causes of morbidity and mortality were registered within five days of the death in 2016 (<https://www.ons.gov.uk/peoplepopulationandcommunity/birthsdeathsandmarriages/deaths/methodologies/impactofregistrationdelaysonmortalitystatistics2016>). This should be discussed, in light of the reliability of the mortality data and the difference late registration might make to results.

#### *Author response*

In addition to our response to the same question above (to reviewer 1) we have removed the delay estimate from Box 1. We have described these delays concisely in the body of the paper and in more depth in the supplementary materials, as well as our approach for addressing these registration delays.

4. Was the HES data restricted to Admitted Patient Care? From Figure 1a, this seems to be the case, but this is not clear in Box 1 which mentioned appointments and attendances. I would question whether it is appropriate to include elective admissions in these estimates. It might be helpful to say that diagnoses aren't historically recorded with accuracy in HES A&E data.

#### *Author response*

We thank the reviewer for noticing this. We agree that the information presented in Box 1 could be slightly confusing for readers. We've updated this information to clarify the issue and to make other concerns about the data more transparent.

We did originally restrict the HES data to finished consultant episodes of Admitted Patient Care, but the reviewer makes the very helpful observation that elective admissions may be inappropriate for our estimates. We agree, so have now restricted

our estimates to emergency admissions for HES Admitted Patient Care. We have also provided a more detailed description of the HES Admitted Patient Care data in the supplementary material, including different measures and corresponding definitions within this dataset. For full clarity we have also plotted the different trends for finished consultant episodes, admissions, and emergency admissions (supplementary figure 3). The trends for all three measures are very similar.

We also wanted to take opportunity to clarify that we do not use HES A&E data. These are a newer form of 'experimental' statistics that have only collected since 2007/08 and, as the reviewer correctly points out, these diagnoses were not historically recorded with accuracy. The A&E data we refer to in the paper come from Cardiff University's ongoing comprehensive surveillance work on violent injury (National Violence Surveillance Network). We hope that we have now made this clearer throughout the manuscript.

5. Please be specific about what you mean by 'rate', e.g. in Figure 1a. Could there have been double counting of events if someone was re-admitted more than once following the same event? Further to the above comment, the text states that "HES suggest 0.086 per 1000 population are admitted to hospital each year". Again this implies that you are counting people rather than admissions, was this the case? Please be explicit about the unit of analysis within HES.

*Author response*

We appreciate this comment. We have gone back through the manuscript to ensure that we have made it explicit that rates refer to admissions and not people admitted. We have also highlighted this limitation in Box 1 and in our supplementary material.

6. Is it possible to tell whether the increase in homicides by knife since 2014 was offset by reductions in homicides by other means?

*Author response*

We have examined this previously using HES A&E data and found no substantial evidence for a weapon substitution effect. Specifically, from 2014/15 to 2014/18 assault by sharp object increased by 38.7% and assault by a blunt object increased by 9.4%. Assault by bodily force showed a small decrease of 5.8%, while assault by other and unspecified means were stable over this period. We haven't explored this in the manuscript due to space constraints and because, as discussed in point 4 above, HES A&E data are not very reliable. However, we plan to examine this more closely and look at such effects across sources in subsequent work.

7. The text mentions that "stalking and harassment" is now included in the definition of violent crime. Crimes like this are less likely to result in death, hospital admission, or A&E. This is a limitation of the data presented and there should be some discussion of this – that a limitation of this analysis is that we cannot infer anything about trends in other forms of violent crime.

*Author response*

We agree with the reviewers point about the bias introduced by the inclusion of "stalking



and harassment” events as violent crime, which we cover in the manuscript:

“Furthermore, there have been changes in definition of violent crime, such as the inclusion of death by dangerous driving and the creation of a “stalking and harassment” sub-category of violence against the person. These changes make it difficult to disentangle whether any increases in police-recorded violence occur as a result of recording changes or from genuine increases in violence. The inconsistency of patterns between police and hospital data are suggestive of the former.”

We take the point that hospital admissions and A&E do not tell us about these forms of violence that may not result in admittance to hospital. To acknowledge the reviewer’s comments, we’ve made slight changes throughout the paper to ensure that it is clear that police-recorded violence is much more broadly defined than injury data (see page 5, para 1). However, with space limitations in mind, we don’t offer an extensive discussion of police data (beyond what we describe in the relevant section), as this was not the focus of the manuscript. We think it is therefore justifiable to focus the extended discussion of the data (in the supplementary files) on the injury and mortality data, especially given that the recent public and political debate focus on “serious violence”.

8. Figure 2 – be more precise in what the ratio represents (is this a ratio of rates or of numbers?) What exactly is the comparable subset that is being compared? It would be helpful to give some figures here, e.g. how many crimes in each source for at least some of the years.

We agree that this is unclear. We’ve tried to clarify this in the text, we’ve also provided specific links to the pages of the ONS report (page 6) where the methodology for constructing a comparable subset of the crime survey and violent crime is described at length. We hope this helps to clarify matters for interested readers.

Figure 3 axis states ‘hospital attendances’ but the figure caption describes this more specifically as A&E attendances. Please be consistent and state the data source.

#### *Author response*

We have changed the figure axis to make it consistent with the figure caption. We have also worked to make sure there are less inconsistencies throughout the manuscript.

Additional Questions:

Please enter your name: Katie Harron

Job Title: Associate Professor

Institution: UCL

Reviewer: 3

Recommendation:

Comments:

David Cromwell, London School of Hygiene & Tropical Medicine

This paper examines issues generated by the recent focus on rates of violence in the UK media. It addresses an important topic and challenges the perceptions given in the

media by comparing the figures on violent crimes based on police data with figures from other sources. They highlight that the trends in the police figures are not evident in the time-series produced from routine hospital data or from population surveys, both of which the authors argue might be more reliable, with just reason. Furthermore, the authors highlight that the unbalanced view of rates of violent offences in the media can lead to undesirable consequences both in terms of policy responses and in relation to the population's reaction. That the latter could lead to an increase in rates of violence makes this a relevant topic for the BMJ.

The article would be improved if various weaknesses could be addressed. The major issues of concern for me are:

1. The article contains a number of phrases that are like the journalist language being condemned. The article would benefit if these were rewritten to be more specific and neutral. For example, the first line of the Abstract reads "Media coverage of spiralling rates of violence in England and Wales has been ubiquitous...", followed by "motivating a search for drastic measures". Alternative statements might be: "National newspapers and television news programmes have reported a rise in rates of violence in England and Wales..." and "stimulating debate among politicians and policy commentators about measures that might reduce the risk of violence." There is also a tendency to use phrases that are broad generalisations (eg, last sentence of Abstract - "safety concerns", "intensify the problem") when more precise concepts would be more helpful.

#### *Author response*

We thank the reviewer for these suggestions. We think this is fair comment. We've reviewed the article and removed comments like "spiralling" and "ubiquitous", rephrasing those sections where necessary.

2. The article refers to the media without distinguishing between the different types of media (newspaper, television, web-based), the extent of their coverage (national, local) and their quality (tabloid, broad-sheet). It would be helpful to separate / contrast the responses to the changes in the police figures in the different sectors of the media. For instance, it would be interesting to see the media time-series in Figure 4 for different groups, if feasible.

#### *Author response*

We agree that it would be potentially revealing to disaggregate the analysis in this way. Figure 4 refers only to mentions of violence reported in the Guardian between 2002-2018, details of how we extracted these data from the Guardian's Application Programming Interface (API) are documented in the Open Science Framework ([https://osf.io/4kwj7/?view\\_only=9aae634f87db4dc7b948f0c3789629f5](https://osf.io/4kwj7/?view_only=9aae634f87db4dc7b948f0c3789629f5)), this includes our Python code. Sadly, the Guardian is one of the only media outlets, at present, with the functionality to allow users to extract data in this way. We have previously tried to use manual searches of online databases for different media outlets, but these are too insensitive and only pick up a small subset of articles, which can't reliably track changes

over time.

3. There needs to be more consistent referencing of sources for statements about media coverage throughout the article. For example, the last sentence of the introduction refers to recent increases in rates of stabbings but gives no reference.

#### *Author response*

We have removed the reference to stabbings in the introduction.

4. The article needs to be clearer in presenting the numerical figures from the different sources. Firstly, sufficient information should be given to enable the results to be reproduced (eg, how were the hospital data obtained, what codes were used to identify events, and how where the data analysed?).

The statement about recent HES data not being available (pg 4) is not true - data are released / published on a monthly basis by NHS Digital. Secondly, the graphs need to be redrawn so that the vertical axis is clearly anchored at 0. A well-known way of emphasising change is to remove 0 from the vertical axis. The general statement about the accuracy of HES at the bottom of page 4 needs to be more nuanced.

#### *Author response*

We sympathise with the reviewer's comments. There are three points that we need to address from this comment:

The first contains our description of the data. In an ordinary research article one would present this information in a detailed methods section. We have extended the information in Box 1 in order to give more information about the data sources, in line with comments from reviewers 1 and 2. We direct readers to supplementary material in which we provide further information about the data sources, as well as the specific routines for extracting data from the Guardian website API (OSF: [https://osf.io/4kwj7/?view\\_only=9aae634f87db4dc7b948f0c3789629f5](https://osf.io/4kwj7/?view_only=9aae634f87db4dc7b948f0c3789629f5)). We also, in the latest submission, have uploaded the data and the code to reproduce the plots on the Github platform.

The second point is specifically in reference to access to HES data not being available. The reviewer is correct to suggest that HES data for admitted patient care are available monthly from NHS Digital. But these data are too coarse for the purposes that we are using HES data on violent injuries as they only refer to total numbers of admissions, which cannot be collapsed by different types (e.g. violent injury) of admissions. The HES data on Admitted Patient Care by external causes that we are using in this paper are annually released. The most recent release was published on 19 September 2019 (<https://digital.nhs.uk/data-and-information/publications/statistical/hospital-admitted-patient-care-activity/2018-19>). As a result, we have now been able to include HES data for 2018/19 and have updated our estimates and graphs accordingly.

The third and final point refers to the anchoring of the axis at zero. We have changed the graphs to address this problem.

5. The article does not examine how the strengths / weaknesses of the different data

sources on the rates of violent offences might influence the results. For example, deaths from violence are likely to make the news but people who die before arriving at hospital will not be admitted and so will not be part of these statistics. The article should state how (un)likely is it that these differences explain the observed patterns.

#### *Author response*

As mentioned previously, we have extended Box 1 and provided additional commentary on the data and limitations of the data in a supplementary file.

6. The section describing the evidence is inconsistent when describing the setting in which studies were conducted. I think it is difficult to extrapolate from the US or other countries with different cultures / laws to the UK. The source of the evidence needs to be clearly stated with appropriate caveats.

#### *Author response*

The purpose of including these examples is not to extrapolate information about mass shootings from the US to the UK, but instead to demonstrate that there is evidence that media coverage of violent events may prompt behavioural changes in the population—in this example, self-protective behaviours—that could impact rates of interpersonal violence. Evidence of the pathway between mass media coverage and behaviours related to self-inflicted violence are relatively well established (see Hawton, 2002; *BMJ*). But few studies have considered the relationship between media coverage of interpersonal violent events and their effects on behaviours of the general public. The cited papers are a few of those that *have* studied this, finding effects that correspond to the concerns we have in this manuscript.

Nevertheless, we have added a caveat to the relevant section (page 7) to acknowledge that the evidence is currently limited to non-UK examples.

7. The conclusion needs to be revised to give a balanced account of the paper results and its implication. For example, its first line refers to fatalities which is odd – the data presented concerns all injuries. Also, why does the conclusion refer to “swift justice”? – the nature of justice is a new idea and is outside the scope of this article.

#### *Author response*

We have made edits to the conclusion to remove the phrasing that the reviewer has identified to be inappropriate.

#### Minor points

1. page 3: I would say “we examine the consistency of police-recorded trends in serious violence .. with other national sources of data on injuries rather than “using injury data from...”

#### *Author response*

We have changed this as suggested.

2. The section titled “Data reliability” is more accurately described as “Reliability of police-reported data on violent offences”

*Author response*

We have changed this as suggested.

3. The end paragraph of the section on data reliability seems inconsistent with the rest of the content in this section, and the article more generally. I feel it needs to be reviewed / moved.

*Author response*

We have reviewed this section but it is unclear from the reviewers comments what is felt to be inconsistent in this section.

4. Page 6: I would re-write the phrase “may have in skewing the public debate” to something more neutral “may have on the public debate”. Also, in the following sentences, “adverse effects” and “respond disproportionately” are also vague statements that require revision.

*Author response*

We have changed this as suggested.

5. Page 8: I don’t think it is accurate / clear to write “During the recent crisis,” – crisis in what sense?

*Author response*

We agree. We have changed this to “Over recent months...”

6. The summary points need to be revised after the changes to the abstract / main article have been made.

*Author response*

We have made changes to the summary points.

Additional Questions:

Please enter your name: David Cromwell

Job Title: Professor of Health Services Research

Institution: London School of Hygiene & Tropical Medicine

Reimbursement for attending a symposium?: No

A fee for speaking?: No

A fee for organising education?: No

Funds for research?: No

Funds for a member of staff?: No

Fees for consulting?: No

Have you in the past five years been employed by an organisation that may

in any way gain or lose financially from the publication of this paper?: No  
Do you hold any stocks or shares in an organisation that may in any way  
gain or lose financially from the publication of this paper?: No