

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Trends in Healthcare Utilisation during COVID-19: A Longitudinal Study from the UK
AUTHORS	Howarth, Ana; Munro, Morag; Theodorou, Alf; Mills, Peter R.

VERSION 1 – REVIEW

REVIEWER	Becker, Nora Michigan Medicine
REVIEW RETURNED	23-Feb-2021

GENERAL COMMENTS	<p>Overall I think this is a really interesting paper. While the analysis is descriptive, it provides timely and suggestive data about how COVID-19 has impacted utilization of health services in the UK. I do have some clarifying questions and suggestions for the authors to improve the manuscript, mostly having to do with their descriptions of the data, and how to interpret their results. I also have concerns about the generalizability of the data and would like more detail on the population from which the data is drawn.</p> <ol style="list-style-type: none">1. The primary outcome the authors report in Table 1 is the frequency of claims per 1,000 enrollees. Is the frequency of claims the same as the number of medical encounters? I have only worked with U.S. insurance claims, however in those claims a given medical encounter may contain multiple claim lines per encounter. The frequency of claims, therefore, would not be an accurate representation of the number of medical encounters. The authors should describe what is captured when they report claims frequency in more detail and explain how to interpret this number for an international audience. I found the secondary outcome percent of claimants with a claim in a given month (as described in Table 2) to be more comprehensible.2. Does the data represent the entire universe of private health claims in the U.K.? The methods state that data was obtained from a “global insurance provider” operating in the U.K. Who is included in this population?3. Membership in the data fluctuates by month. How did it change during 2020? Did people appear to lose their private insurance? Were there dramatic shifts in the distribution of the population by age, gender, income? What information is available about the enrollees in the data? If enrollees disenroll in private insurance could they still obtain care in the NHS? Is it possible that reductions in private care caused spillover effects, i.e., relative increases in NHS services as a result of individuals disenrolling?4. Relatedly, the authors provide only descriptive statistics. A regression analysis examining changes in healthcare utilization could potentially adjust for changes in the underlying distribution of the enrolled population. Another possibility would be to conduct
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	<p>this analysis using a subgroup of enrolled individuals whose enrollment was continuous over the period of their data.</p> <p>5. In Table 2 the authors also report monthly cost per claimant. Is this cost representative of charges to providers, or true costs paid to providers by the insurance company? Again, in the U.S. the true costs paid by insurers are typically private and not available to researchers except in specific cases.</p> <p>6. Was any data missing? The authors don't specify.</p> <p>7. The authors find that costs declined relatively little compared to utilization. How do they interpret this finding? It seems quite striking but there is no discussion of it in their discussion. Does this finding have implications for the quality of care provided during the pandemic?</p> <p>8. I find Tables 3 and 4 difficult to interpret. In Table 3, the authors categorize claims by disease categories. The result for mental health care is notable and jumps out from the others. But the other categories of care feel pretty diffuse to me. Management for most of these disease categories can run the gamut from routine care to urgent to emergent, in many settings (outpatient, inpatient, emergency room, etc.). So it's difficult to draw conclusions about the overall quality of care of what types of care declined relative to others. Similarly, to me as a U.S. researcher the categories of services in Table 4 are almost uninterpretable. What types of medical care falls into "specialist consultations" vs "specialist fees"? What is package pricing and accommodations? What are treatment room charges? What is cash benefit? These terms have little meaning for me as a U.S. health policy researcher. I'd suggest the authors construct more clinically meaningful service categories, for instance, outpatient vs inpatient care, GP vs specialist care, surgical care, etc.</p> <p>9. In the Discussion the authors mention that private providers treated NHS patients during the pandemic. Do NHS patients treated by private providers appear in the numerator of the rates they are reporting? Do they appear in the denominator? The authors need to specify. Can the authors tell in their data whether an individual who received a specific service had private insurance or not? The authors state that because private providers cared for NHS patients their data "could be considered a reasonable representation of the impact of the pandemic..." That's quite a strong claim that they do not back up with data. Were the NHS patients cared for by private providers representative of NHS enrollees otherwise, or is it possibly they were cared for non-randomly?</p> <p>10. I find the Discussion section regarding their mental health findings a bit confusing. The authors cite a study that the quality of mental health care declined, and say that their findings support this, but they found that mental health care provision increased. I believe the authors are conflating 1) the status of mental health needs in the U.K, 2) the volume of mental health care provided and 3) the quality of mental health care. A rise in the volume of mental health care is suggestive of a rise in mental health needs, but not necessarily the same thing. In some circumstances a rise in consumption of mental health care could reflect an improvement in the underlying mental health of the population, as more people obtain the care they need. From a policy perspective, it seems to be that the rise in mental care use during COVID is a victory for the health system in that they were able to respond to the underlying and changing needs of their population during an unprecedented time of fear and anxiety. On the other hand, they have no evidence either way that the quality of mental health care</p>
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	<p>was equivalent to, better, or worse than that provided pre-pandemic.</p> <p>11. Is utilization for COVID-related care included in these overall findings? It's interesting that respiratory disease care declined so much overall, despite the fact that COVID overwhelmed hospital systems. Do the authors have any explanation for this?</p> <p>12. The authors should note in their limitations that their results are specific to the U.K., particularly the impact of social distancing and lockdown restrictions on healthcare utilization. These policies were implemented very differently from nation to nation, for instance, in the U.S. lockdowns tended to be less restrictive and lifted earlier than in some other countries, and this likely influenced healthcare utilization.</p> <p>13. The authors should specify whether this work was IRB-approved or exempt, and through what approval mechanism this was determined. Similarly, funding sources should also be made explicit.</p>
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REVIEWER	Lang, Eddy University of Calgary, Emergency Medicine
REVIEW RETURNED	23-Mar-2021

GENERAL COMMENTS	<p>Thank you for the opportunity to review this manuscript describing the impact of the COVID-19 pandemic in the UK on healthcare utilization. The authors rely on detailed claims data to conduct their analysis and compare the period prior to the March 2020 lockdowns to the following months leading up to August. As has been described in much of the peer-reviewed literature (Moynihan et al BMJ Open 2021 https://pubmed.ncbi.nlm.nih.gov/33727273/) these phenomena have been very widespread and likely global in nature. This article provides additional useful insight into the categories of complaints and care affected as well as the types of providers and services involved in this remarkable phenomenon. The paper is well-written and the analysis is straight-forward and robust. Tables 3 and 4 are particularly useful in demonstrating the phenomenon being described. The paper also offers insight as to the impact of the pandemic on mental health care as well as cancer care. I would suggest if possible that a more graphical presentation of the reduction in utilization would be helpful so as to help distinguish the initial shock of the March events in relation to what is presented as a gradual recovery through to August but which may actually represent a new steady state.</p> <p>The discussion seems to be framed on the assumption that the reduction in care is something of a deficit in services that must be replaced or returned to a normal state of resiliency. Other important considerations, albeit hypotheses should be considered. For one, the eradication of the influenza season through public health measures may shed some insight on the effect that the public health measures i.e. masking, physical distancing, would likely have had on a range of respiratory conditions namely, COPD, asthma and non-COVID pneumonias. The drop in injury related care is also a likely manifestation of the reduction in physical activities across the population related to these same measures. Finally I think there could also be merit in suggesting that the reduction in healthcare utilization may have preferentially impacted low-value and potentially unnecessary care and that there are likely some valuable lessons to be learnt in that context. https://pubmed.ncbi.nlm.nih.gov/32665257/</p> <p>Also while I appreciate that this project is low-risk from a patient confidentiality perspective there are other considerations in</p>
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	seeking ethics board approval that were not taken into consideration by this project. For example is the research question meritorious, are the methods sound and are the potential harms of publishing these results greater than the benefits.
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Dr. Nora Becker, Michigan Medicine

Comments to the Author:

Overall I think this is a really interesting paper. While the analysis is descriptive, it provides timely and suggestive data about how COVID-19 has impacted utilization of health services in the UK. I do have some clarifying questions and suggestions for the authors to improve the manuscript, mostly having to do with their descriptions of the data, and how to interpret their results. I also have concerns about the generalizability of the data and would like more detail on the population from which the data is drawn.

- 1. The primary outcome the authors report in Table 1 is the frequency of claims per 1,000 enrollees. Is the frequency of claims the same as the number of medical encounters? I have only worked with U.S. insurance claims, however in those claims a given medical encounter may contain multiple claim lines per encounter. The frequency of claims, therefore, would not be an accurate representation of the number of medical encounters. The authors should describe what is captured when they report claims frequency in more detail and explain how to interpret this number for an international audience. I found the secondary outcome percent of claimants with a claim in a given month (as described in Table 2) to be more comprehensible.**

Reply: Thank you for this feedback in particular as insurance processes and datasets do vary internationally. In this instance, we were working from a dataset that simply gave a frequency count in predetermined categories. This means as you correctly suggested, it does not necessarily represent a single medical encounter. We have amended the text to clarify this and offer an example (p.4)

- 2. Does the data represent the entire universe of private health claims in the U.K.? The methods state that data was obtained from a “global insurance provider” operating in the U.K. Who is included in this population?**

Reply: As the UK has a national health service, the privately insured population is relatively small at ~13% as suggested in the introduction. Across this population there are a variety of insurers to choose from. This dataset comes from one of these so it does not represent the entire privately insured population but it is a sample. More client specific population detail was not available but in general, this insured population is represented by white collar or corporate clients.

- 3. Membership in the data fluctuates by month. How did it change during 2020? Did people appear to lose their private insurance? Were there dramatic shifts in the distribution of the population by age, gender, income? What information is available about the enrollees in the data?**

Reply: The data presented here is all employer sponsored health insurance. Contracts usually last 3-4 years before renewal but the total membership fluctuates monthly and yearly as employers choose which carrier to engage with. We saw small reduction in total live membership but we can't be more specific as it is industry sensitive information.

If members dis-enrolled in private insurance could they still obtain care in the NHS? Is it possible that reductions in private care caused spillover effects, i.e., relative increases in NHS services as a result of individuals disenrolling?

In the UK, everyone is entitled to use NHS services regardless of whether they have private insurance or not. In fact, to access many private health care services, patients must first be referred by an NHS GP. As the majority of the UK public does not depend on private healthcare, it was unlikely there was any discernible increases in NHS services.

However, as mentioned in the discussion, there has been a collaboration between private and NHS services during the pandemic where the NHS took over all COVID-19 related care and private providers took over non-COVID-19-related care for patients regardless of whether they had private cover or not. These patients were of course not included our dataset but the triage criteria that was then applied across the UK was the same for all patients. This meant whether a patient had private coverage or not, patient care was accessed based on a pre-established needs criteria and there was no separate pathways based on public or private care status during this period of the pandemic.

- 4. Relatedly, the authors provide only descriptive statistics. A regression analysis examining changes in healthcare utilization could potentially adjust for changes in the underlying distribution of the enrolled population. Another possibility would be to conduct this analysis using a subgroup of enrolled individuals whose membership was continuous over the period of their data.**

Reply: This is another area that would have been very insightful to explore but as there were a variety of factors we could not control for we did not want to overstate any results. Sub-group analysis would also have been helpful but we did not have access to individual membership details so we have no data on continuous enrolment.

- 5. In Table 2 the authors also report monthly cost per claimant. Is this cost representative of charges to providers, or true costs paid to providers by the insurance company? Again, in the U.S. the true costs paid by insurers are typically private and not available to researchers except in specific cases.**

Reply: These costs were the cost billed which represents the charges billed by providers. We have clarified this in the manuscript (p.4)

- 6. Was any data missing? The authors don't specify.**

Reply: The dataset we were given was not the raw data and had been prepared in advance so we had no missing data content with and this is now noted (p.4)

- 7. The authors find that costs declined relatively little compared to utilization. How do they interpret this finding? It seems quite striking but there is no discussion of it in their discussion. Does this finding have implications for the quality of care provided during the pandemic?**

Reply: Table 2 shows a marked reduction in the percentage of covered individuals who claimed for medical care, however, for those who did claim the average cost was not markedly different from pre-pandemic amounts. This suggests that for those that did access care received approximately the same care from a costs perspective.

- 7. I find Tables 3 and 4 difficult to interpret. In Table 3, the authors categorize claims by disease categories. The result for mental health care is notable and jumps out from the others. But the other categories of care feel pretty diffuse to me. Management for most of these disease categories can run the gamut from routine care to urgent to emergent, in many settings (outpatient, inpatient, emergency room, etc.). So it's difficult to draw conclusions about the overall quality of care of what types of care declined relative to others. Similarly, to me as a U.S. researcher the categories of services in Table 4 are almost uninterpretable. What types of medical care falls into "specialist consultations"**

vs “specialist fees”? What is package pricing and accommodations? What are treatment room charges? What is cash benefit? These terms have little meaning for me as a U.S. health policy researcher. I’d suggest the authors construct more clinically meaningful service categories, for instance, outpatient vs inpatient care, GP vs specialist care, surgical care, etc.

Reply: Thank you for this feedback. As we were only able to gain access to a dataset prepared in advance with these pre-specified categories, we were unable to expand more on the conditions categories. However, we were able to obtain brief descriptions of the service categories which are attached as supplementary material now (p.7). We accept that lack of granular data is a strong limitation in this study but hope the benefit of being able to track some overall healthcare trends will contribute to the larger evidence-base that will no doubt eventually be established.

Table 3 broadly represent the top level ICD coding categories for diagnoses. Table 4 represents the benefit categories, which although not standard across the UK private health insurance landscape, are frequently used categories by the majority of insurers.

9. In the Discussion the authors mention that private providers treated NHS patients during the pandemic. Do NHS patients treated by private providers appear in the numerator of the rates they are reporting? Do they appear in the denominator? The authors need to specify. Can the authors tell in their data whether an individual who received a specific service had private insurance or not? The authors state that because private providers cared for NHS patients their data “could be considered a reasonable representation of the impact of the pandemic with noncovid” That’s quite a strong claim that they do not back up with data. Were the NHS patients cared for by private providers representative of NHS enrollees otherwise, or is it possibly they were cared for non-randomly?

Reply: The dataset used for this study did not include any NHS patients but as mentioned in an earlier reply to comment #3, we touched on the unique situation within the UK in relation to provision of non-COVID-19 related services and how these were rolled out in the same way (i.e., based on agreed clinical necessity criteria) regardless of insurance status. This is the driver behind the comment that the type and level of access was being provided across the country based on the same criteria so the sample we present would have been treated the same way.

10. I find the Discussion section regarding their mental health findings a bit confusing. The authors cite a study that the quality of mental health care declined, and say that their findings support this, but they found that mental health care provision increased. I believe the authors are conflating 1) the status of mental health needs in the U.K, 2) the volume of mental health care provided and 3) the quality of mental health care.

Reply: This may be cleared up by confirming that the reference was to the quality of UK mental health not healthcare (ie., the state of mental health got worse). We have reworded it slightly so as be more explicit as per below.

*“A recently published study comparing mental health trends longitudinally for over 40,000 participants found that by the end of April 2020, **the quality of the mental health of adults in the UK had decreased in comparison to pre-COVID-19 years**¹⁷”*

You are right we have no information about the quality of healthcare provided mental or otherwise.

A rise in the volume of mental health care is suggestive of a rise in mental health needs, but not necessarily the same thing. In some circumstances a rise in consumption of mental health care could reflect an improvement in the underlying mental health of the population, as more people obtain the care they need. From a policy perspective, it seems to be that the rise in mental care use during COVID is a victory for the health system in that they were able to respond to the underlying and changing needs of their population during an unprecedented

time of fear and anxiety. On the other hand, they have no evidence either way that the quality of mental health care was equivalent to, better, or worse than that provided pre-pandemic.

Reply: We absolutely agree, a rise in the volume of those accessing mental health care does not necessarily mean a rise in mental health needs. However, as some literature reported worsening of mental health status overall for adults across the UK, we attempted to present these results as a point of interest and suggest that it is worth addressing. The UK is a nation where there has been a major push through public health campaigns to de-stigmatise mental health issues as this has been a barrier for vulnerable populations who are in great need. Any changes in this positive direction may be of interest for many.

11. Is utilization for COVID-related care included in these overall findings? It's interesting that respiratory disease care declined so much overall, despite the fact that COVID overwhelmed hospital systems. Do the authors have any explanation for this?

Reply: Due to the private and public healthcare collaboration COVID-19 patients or those worried about COVID-19 would have been triaged to NHS hospitals so this would most likely explain some decline for private respiratory care. At the same time, many who would normally access care for minor respiratory concerns were either in lockdown (which also substantially reduced the impact of the flu season) or hesitant to visit clinic or hospital settings as there were still many unknowns about COVID-19 transmission during the first wave.

12. The authors should note in their limitations that their results are specific to the U.K., particularly the impact of social distancing and lockdown restrictions on healthcare utilization. These policies were implemented very differently from nation to nation, for instance, in the U.S. lockdowns tended to be less restrictive and lifted earlier than in some other countries, and this likely influenced healthcare utilization.

Reply: Thank you, this is a good point to specify in this section too so it has been added. We are also aware of the vast array of policies globally so we hope that the title of the manuscript prepares the reader for this being very relevant to the UK specifically.

13. The authors should specify whether this work was IRB-approved or exempt, and through what approval mechanism this was determined. Similarly, funding sources should also be made explicit.

Reply: In the UK the system is not the same as the US where IRBs operate under a federal system so we can't specify IRB exempt. However this work would be the equivalent of exempt as the original patients cannot be traced because the dataset has been robustly anonymised. There was no personal data (e.g., DOB, address or medical history) involved at any point so no patient could ever be identified during the analysis or potentially post-publication. This is in line with BMJ Open author guidelines as well.

Funding source is explicitly reported on p. 12 under footnotes as specified by BMJ Open guidelines.

Reviewer: 2

Dr. Eddy Lang, University of Calgary

Comments to the Author:

Thank you for the opportunity to review this manuscript describing the impact of the COVID-19 pandemic in the UK on healthcare utilization. The authors rely on detailed claims data to conduct their analysis and compare the period prior to the March 2020 lockdowns to the following months leading up to August. As has been described in much of the peer-reviewed literature (Moynihan et al BMJ Open 2021

[https://urldefense.com/v3/https://pubmed.ncbi.nlm.nih.gov/33727273/;!!FEyU5bU!RuSkBm3WcW_mQhcAM4wWjX4Th0Zw7beGNkhiY-nnEgjoUdhhdtoXNI4GcmtlrA\\$](https://urldefense.com/v3/https://pubmed.ncbi.nlm.nih.gov/33727273/;!!FEyU5bU!RuSkBm3WcW_mQhcAM4wWjX4Th0Zw7beGNkhiY-nnEgjoUdhhdtoXNI4GcmtlrA$)) these

phenomena have been very widespread and likely global in nature. This article provides additional useful insight into the categories of complaints and care affected as well as the types of providers and services involved in this remarkable phenomenon. The paper is well-written and the analysis is straight-forward and robust. Tables 3 and 4 are particularly useful in demonstrating the phenomenon being described. The paper also offers insight as to the impact of the pandemic on mental health care as well as cancer care.

I would suggest if possible that a more graphical presentation of the reduction in utilization would be helpful so as to help distinguish the initial shock of the March events in relation to what is presented as a gradual recovery through to August but which may actually represent a new steady state.

Reply: Thank you for this suggestion. We did initially consider creating a graphical representation for the paper but felt the tables offer more granular detail and describe the data well. If the editor feels that a graphical version would be more appropriate for the paper we will be happy to oblige

The discussion seems to be framed on the assumption that the reduction in care is something of a deficit in services that must be replaced or returned to a normal state of resiliency. Other important considerations, albeit hypotheses should be considered. For one, the eradication of the influenza season through public health measures may shed some insight on the effect that the public health measures i.e. masking, physical distancing, would likely have had on a range of respiratory conditions namely, COPD, asthma and non-COVID pneumonias. The drop in injury related care is also a likely manifestation of the reduction in physical activities across the population related to these same measures.

Reply: These are very helpful points and they have been added to the discussion (p. 9).

Finally I think there could also be merit in suggesting that the reduction in healthcare utilization may have preferentially impacted low-value and potentially unnecessary care and that there are likely some valuable lessons to be learnt in that context.
[https://urldefense.com/v3/https://pubmed.ncbi.nlm.nih.gov/32665257/;!!FEyU5bUI!RuSkBm3WcW_mQhcAM4wWjX4Th0Zw7beGNkhiY-nnEgjoUdhhdtoXNI4GUGiXMB\\$](https://urldefense.com/v3/https://pubmed.ncbi.nlm.nih.gov/32665257/;!!FEyU5bUI!RuSkBm3WcW_mQhcAM4wWjX4Th0Zw7beGNkhiY-nnEgjoUdhhdtoXNI4GUGiXMB$)

Reply: This is a very worthwhile discussion that we would have liked to have included but as mentioned in our limitations we had no data addressing motivations or drivers of objective health behaviours (i.e., submitted healthcare claims) so we refrained from commenting.

Also while I appreciate that this project is low-risk from a patient confidentiality perspective there are other considerations in seeking ethics board approval that were not taken into consideration by this project. For example is the research question meritorious, are the methods sound and are the potential harms of publishing these results greater than the benefits.

Reply: As this paper was not attached to an academic institution or government organisation it was not possible to have it automatically submitted to a review board. Both lead authors (AH, PM) are attached to separate academic institutions where they have substantial experience with ethical considerations for research including publication practice. There were no potential harms of publishing the results as there was no patient identifying data included during any part of the research process. Raw data was not worked with but instead we were provided with a completely anonymised dataset with no possible avenue for tracing back to patients at any point. We leave the editors of BMJ Open to determine whether this is a meritorious research.

VERSION 2 – REVIEW

REVIEWER	Becker, Nora Michigan Medicine
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REVIEW RETURNED	08-Jun-2021
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GENERAL COMMENTS	<p>The authors have done a good job clarifying the results in response to my comments and the comments of the other reviewer. A few thoughts and responses to the updated manuscript:</p> <ol style="list-style-type: none"> 1. I agree with the other reviewer's comment that Table 1 would be much more effectively presented as a Figure. 2. More details are still required about the actual source of the data that they used. They state that is it one of the private providers in the UK, but they give no additional detail about how many total enrollees were included in the data, what approximate percentage of the total privately insured population this data source represents, or how this population may differ or be similar from the broader privately insured population or the general population of the UK. This is basic information about the data source that is expected in all manuscripts using administrative data. Also, it is typical in manuscripts using secondary data analysis for an introductory Table to be included that includes these details and whatever data is available about the population, even in aggregate, such as the average age, the distribution of gender, age groups included in the data, race/ethnicity, location, etc. As currently written it is totally opaque to me what population this data represents. 3. While the authors make it clear in their response to reviewers, they should clarify in the manuscript itself that 1) the claims they analyzed did not include COVID-related care for the enrollees in the study, as that was provided by NHS providers, and 2) health care utilization among individuals who were not enrolled with this particular insurer are not included in the data, and that the enrolled population was not constant over time in the data, i.e., individuals could enroll or disenroll at any time. I would be cautious with the use of the word "longitudinal" as they use it in the paper; this typically suggests that individuals in the data are being followed longitudinally but this is incorrect for some (unclear how many) of the individuals in the dataset. They state in their response to reviewers that "we saw small reduction in total live membership but we can't be more specific as it is industry sensitive information." If they are not permitted to share this information with readers it should be explicitly stated in the manuscript as this information typically should be included in an analysis of this kind. 4. I appreciate that the authors provided additional information about the collaboration between the NHS and private providers. However, now that I understand this better, this raises another important limitation of the analysis: their analysis is at the level of the payor, not the level of the provider, and so it is theoretically possible that some of the reductions in utilization they see in their population could be due to decreased access for these privately insured patients as the providers they typically saw took on the additional care of publicly-insured patients, too. It's not possible to say from the data.
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REVIEWER	Lang, Eddy University of Calgary, Emergency Medicine
REVIEW RETURNED	31-May-2021

GENERAL COMMENTS	Thank you for the opportunity to review this revised manuscript. The changes are appreciated and add clarity to the manuscript.
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	Most of the issues I raised in my review were addressed in this revision.
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VERSION 2 – AUTHOR RESPONSE

Reviewer: 1
 Dr. Nora Becker, Michigan Medicine

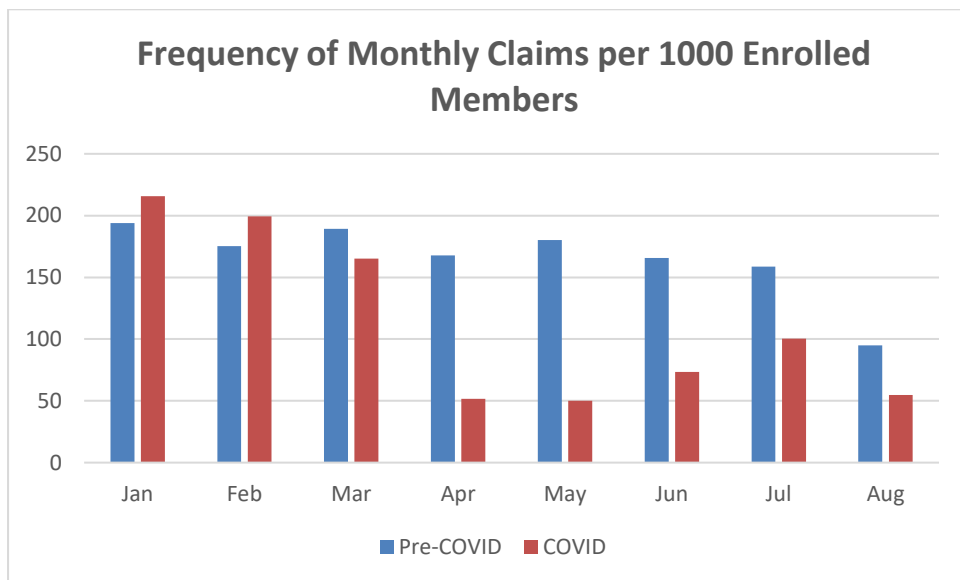
Comments to the Author:

The authors have done a good job clarifying the results in response to my comments and the comments of the other reviewer. A few thoughts and responses to the updated manuscript:

1. I agree with the other reviewer’s comment that Table 1 would be much more effectively presented as a Figure.

Reply: We do still feel that the table format is more informative than a graphical representation, we have, however, now added the below as an option and ask the editor to make a call on which would be most appropriate in the final manuscript.

Figure 1. Frequency of claims by month per 1000 enrolled members from 2018/2019* to 2020



2. More details are still required about the actual source of the data that they used. They state that is it one of the private providers in the UK,

Reply: We appreciate that you probably didn’t see author names or affiliations, however, it will be clear to the reader when this information is included in the final manuscript that this is Cigna UK data

but they give no additional detail about how many total enrollees were included in the data

Reply: We have included details of the size of the population studied. It was at all points greater than 260,000 and less than 300,000 lives. This included now on p.5

what approximate percentage of the total privately insured population this data source represents,

Reply: There are no definitive stats on the proportion of the UK population that have private health insurance cover. However, according to the Commonwealth Fund it is estimated that approximately 10.5% of the population has some form of private health coverage, with the majority being employer

purchased. Using basic math this would equate to nearly 7 million people. Our sample of privately insured individuals with a corporate Cigna plan therefore presents approximately 4.2% of the privately insured population.

or how this population may differ or be similar from the broader privately insured population or the general population of the UK.

Reply: There is no industry wide data available that describes the demographic profile of the privately insured population. However, it is unlikely that the population used in for the purposes of this analysis differ significantly from this overall group; being predominantly “white-collar” employees of medium to large employers in the UK.

This is basic information about the data source that is expected in all manuscripts using administrative data. Also, it is typical in manuscripts using secondary data analysis for an introductory Table to be included that includes these details and whatever data is available about the population, even in aggregate, such as the average age, the distribution of gender, age groups included in the data, race/ethnicity, location, etc. As currently written it is totally opaque to me what population this data represents.

Reply: As noted previously we were constrained by the granularity of the data that was available to us, however, we have been able to include further demographic information that helps describe the population in more detail. In the results section (p.5), we added:

“While not being able to confirm exact membership enrolment or divulge clientele details as it is industry sensitive information, it is possible to report that the minimum average monthly membership was >260,000. This is taking into consideration a small reduction seen in total live membership during the early months of the pandemic.”

3. While the authors make it clear in their response to reviewers, they should clarify in the manuscript itself that

- a) the claims they analyzed did not include COVID-related care for the enrollees in the study, as that was provided by NHS providers, and 2) health care utilization among individuals who were not enrolled with this particular insurer are not included in the data, and that the enrolled population was not constant over time in the data, i.e., individuals could enroll or disenroll at any time.**

Reply: Clarification has been added (p.9) as per below:

“Our data could therefore be considered to be a reasonable representation of the impact of the pandemic on the different aspects of non-COVID care across the nation. This means the data for this study did not include any COVID-19 related care utilization.”

- b) I would be cautious with the use of the word “longitudinal” as they use it in the paper; this typically suggests that individuals in the data are being followed longitudinally but this is incorrect for some (unclear how many) of the individuals in the dataset.**

Reply: The longitudinal reference has been clarified in the methods section (p.4).

- c) They state in their response to reviewers that “we saw small reduction in total live membership but we can’t be more specific as it is industry sensitive information.” If they are not permitted to share this information with readers it should be explicitly stated in the manuscript as this information typically should be included in an analysis of this kind.**

Reply: In the results section (p.5), we added:

“While not being able to confirm exact membership enrolment or divulge employer details as it is industry sensitive information, it is possible to report that the minimum average monthly membership was >260,000. This is taking into consideration a small reduction seen in total live membership during the early months of the pandemic.”

4. I appreciate that the authors provided additional information about the collaboration between the NHS and private providers. However, now that I understand this better, this raises another important limitation of the analysis: their analysis is at the level of the payor, not the level of the provider, and so it is theoretically possible that some of the reductions in utilization they see in their population could be due to decreased access for these privately insured patients as the providers they typically saw took on the additional care of publicly-insured patients, too. It’s not possible to say from the data.

Reply: This is indeed the point we are trying to make with this paper. It is very likely that the reductions seen were due, at least in part, to decreased access to care during the first wave of the pandemic. By showing the extent of this we hope that future service planning will take into account the size and proportion of the need for extra care provision because of this. Much of the public discourse in the UK (and indeed other countries like the USA) has been around the potential impact the pandemic has had on people delaying seeking important care, either because of fear of COVID-19 or because that care was just not accessible.

Reviewer: 2

Dr. Eddy Lang, University of Calgary

Comments to the Author:

Thank you for the opportunity to review this revised manuscript. The changes are appreciated and add clarity to the manuscript. Most of the issues I raised in my review were addressed in this revision.