PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Usage of Unscheduled Hospital Care by Homeless Individuals in Dublin, Ireland: A Cross-Sectional Study
AUTHORS	Ní Cheallaigh, Clíona; Cullivan, Sarah; Sears, Jess; Lawlee, Ann Marie; Browne, Joseph; Kieran, Jennifer; Segurado, Ricardo; O'Carroll, Austin; O'Reilly, Fiona; Creagh, Donnacha; Bergin, Colm; Kenny, RoseAnne; Byrne, Declan

VERSION 1 – REVIEW

REVIEWER	Kelly Doran, MD, MHS NYU School of Medicine, USA
REVIEW RETURNED	01-May-2017

GENERAL COMMENTS	Overall: This paper provides information about homeless vs. non-homeless attendees of an emergency department and inpatient service. While the methodology of using the electronic medical record address field to identify homelessness is limited, the authors do at least attempt to crudely evaluate sensitivity of their measure. Overall, the authors need to make a more compelling case for why their study is important, despite its limitations, in the context of the existing literature. Currently the paper reads more as presentation of results from baseline analyses such as might be conducted for a hospital quality improvement effort (if this was the case, it should be stated), rather than a focused academic endeavor. The authors could improve the paper by stating more clearly, beginning in the introduction and carrying consistently throughout, their a priori intentions and hypotheses. I think that there are several strengths of the study and it presents interesting information that likely deserves publication, but the paper should be improved by strengthening its focus as well as addressing the specific concerns outlined below. Title: What is the title of the paper? There is a different title listed in the BMJ system compared to the title that the authors list on the abstract. Introduction 1. Readers may need a definition of “sleeping rough.” 2. There are many different definitions of “homelessness.” The authors should give a citation / source for the definition they use in the 2nd sentence. 3. Overall the authors need to make a stronger case for why this study is needed and what gaps in the literature it fills, especially given that this study is a single hospital study and there are already multiple single hospital studies of homeless patients.

	The case for how this study contributes to our knowledge should be stated explicitly. It may be that the focus on age profile is the unique contribution this study makes given that I cannot recall many hospital-based studies examining age profile explicitly; if the authors had chosen this focus they should state so explicitly in the introduction and presentation of the results. Methods 1. The operational definition used for homelessness in the study differs from the definition given by the authors in the introduction (their introduction definition includes those living with family/friends or in a "squat", but the operational definition they use for this study does not include these categories.) 2. There are obvious limitations (some of which the authors acknowledge) to relying on medical record address to identify homelessness. First, the group of those living doubled up is missed (see above), which should be added in the limitations section. More problematically, however, using the address field relies on the accuracy with which the address information is recorded in the medical record. From my observations, this varies significantly by hospital. The authors should provide more detail on how address information is obtained from/for each patient at the study hospital. The authors should also provide details on how they determined the homeless hostel addresses and ensured none were missed. One strength of the study is that the authors do attempt to assess the sensitivity of the address information by examining address field for those known to be homeless and referred to Social Work—they found that 72% of 100 patients known to be homeless had an address on their record that identified them as homeless, which is decent but not stellar. This method also does not allow us to know how many patients might be truly homeless but not "identified" as such in either the address field or referral to social work (e.g., the true population of people who are homeless is unknown), nor does it speak to the percent identified as homeless in the address field who may not have actually been homeless. 3. What is "general medical take rota"? 4. Given a focus on age profile, it seems to be a limitation that patients admitted under the Geriatrics service were not included. It is also possible that the age stratification might vary for some of the other services listed as well (e.g., Oncology). Therefore, the age results given in the paper may not be representative of all hospital inpatients but rather those admitted to general medicine and not the other services. The authors should provide more information about why these services—particularly Geriatrics—were excluded, and how this might have influenced their results. 5. Were the analyses planned a priori? It is unclear, for example, whether the authors set out to examine age in detail or whether the authors decided to perform more analyses related to age post hoc after finding a significant mean age difference. Results 1. It is a strength that the authors took the time to estimate their catchment area total and homeless population. The authors should provide more details on the estimate of the homeless population in the catchment area (beyond saying it was "estimated by the Dublin Regional Homeless Executive")

	—is this is a single person who estimated the catchment? Do the authors account for the fact that many homeless people may be transient / move among locations and thus might come from areas outside the “catchment”? (As a related question, did the hostel addresses used fall only inside the “catchment area”?) More information seems important since the authors do present results and make some conclusions based on their catchment area estimates. If the catchment area estimates are problematic, then the authors may be better off presenting only the results that are known (i.e., based solely on the data they collected) rather than presenting data that uses a problematic denominator; or the authors could appropriately frame these results as crude estimates based on their estimated catchment denominator. 2. How did the authors determine cut-offs for “frequent” and “very frequent” ED use? 3. Table 1 – the presentation of % of individuals in the last two lines of the table is confusing. It’s unclear exactly what is being shown. 4. Tables / Results in general – the results of the statistical testing for comparisons are not always shown, and thus readers are sometimes left to trust the text in the results, or to make their own conclusions for those results not included in the text. For example, the authors state that the admission rates were similar for homeless and non-homeless, and in the table the rates appear to be 17.2% vs. 28.8%--was this difference indeed non-significant in statistical testing? It would be best to show the results. 5. Table 3 – it is interesting that both homeless and non-homeless presented for a wide variety of complaints; the authors could add a line showing that % presented for “other” complaints not captured in the top 10 shown. 6. Table 4 – what is “ITU”? Discussion 1. In the results the % of homeless who left without being seen is very high (40.7%). Could the authors comment on their thoughts regarding this finding? 2. The authors might devote more time to discussing some of the prior research showing premature aging by homeless populations (studies by Margot Kushel, Rebecca Brown). It seems that one conclusion that could be more explicitly stated is that even young homeless people are quite sick (multiple co-morbidities). 3. The limitations section should be expanded with some of the points made above.

REVIEWER	Nigel Hewett Pathway Charity London UK
REVIEW RETURNED	12-May-2017

GENERAL COMMENTS	A large scale and very worthwhile study that will make a significant contribution to improving our understanding of the the impact of social determinants of health on secondary care

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

Introduction
1. Readers may need a definition of “sleeping rough.”

Response: This has been added

2. There are many different definitions of “homelessness.” The authors should give a citation / source for the definition they use in the 2nd sentence.

Response: This has been amended

3. Overall the authors need to make a stronger case for why this study is needed and what gaps in the literature it fills, especially given that this study is a single hospital study and there are already multiple single hospital studies of homeless patients. The case for how this study contributes to our knowledge should be stated explicitly. It may be that the focus on age profile is the unique contribution this study makes given that I cannot recall many hospital-based studies examining age profile explicitly; if the authors had chosen this focus they should state so explicitly in the introduction and presentation of the results.

Response: Thank you for this helpful comment. This study fills gaps in two respects: 1) It was carried out in a high-income European country, with significant differences in the demographics of homeless individuals compared to the US (this has now been added to the discussion) and 2) It focuses on the association between age and multimorbidity in hospitalized homeless patients – demonstrating that, unlike housed patients young homeless people have multiple chronic diseases and use a lot of unscheduled healthcare – the association between age and these findings has not previously been examined. We have endeavoured to highlight this in the manuscript.

Methods
1. The operational definition used for homelessness in the study differs from the definition given by the authors in the introduction (their introduction definition includes those living with family/friends or in a “squat”, but the operational definition they use for this study does not include these categories.)

Response: We have clarified this in the text – our operational definition captures only a subset of those defined as homeless in the broader context in the introduction. We have highlighted this now as a limitation.

2. There are obvious limitations (some of which the authors acknowledge) to relying on medical record address to identify homelessness. First, the group of those living doubled up is missed (see above), which should be added in the limitations section. More problematically, however, using the address field relies on the accuracy with which the address information is recorded in the medical record. From my observations, this varies significantly by hospital. The authors should provide more detail on how address information is obtained from/for each patient at the study hospital. The authors

should also provide details on how they determined the homeless hostel addresses and ensured none were missed. One strength of the study is that the authors do attempt to assess the sensitivity of the address information by examining address field for those known to be homeless and referred to Social Work—they found that 72% of 100 patients known to be homeless had an address on their record that identified them as homeless, which is decent but not stellar. This method also does not allow us to know how many patients might be truly homeless but not "identified" as such in either the address field or referral to social work (e.g., the true population of people who are homeless is unknown), nor does it speak to the percent identified as homeless in the address field who may not have actually been homeless.

Response: Thank you. We have clarified the limitations of our method of identifying homelessness. We have also added information on address registration and on how we identified the addresses of homeless hostels.

Comment: What is "general medical take rota"?

Response: This is a UK/Irish term for acute unscheduled admissions under internal medicine, we have clarified this in the text

Comment: Given a focus on age profile, it seems to be a limitation that patients admitted under the Geriatrics service were not included. It is also possible that the age stratification might vary for some of the other services listed as well (e.g., Oncology). Therefore, the age results given in the paper may not be representative of all hospital inpatients but rather those admitted to general medicine and not the other services. The authors should provide more information about why these services—particularly Geriatrics—were excluded, and how this might have influenced their results.

Response: These services do not participate in the inpatient care of unselected acute internal medicine admissions. In addition, all of these services, except for Geriatrics, act as supraregional referral centres and their inpatients are likely to come from areas outside the catchment area of the hospital. The Geriatrics service does not provide inpatient care to homeless people, as their lack of a housed discharge destination would preclude them from the discharge planning process.

Comment: Were the analyses planned a priori? It is unclear, for example, whether the authors set out to examine age in detail or whether the authors decided to perform more analyses related to age post hoc after finding a significant mean age difference.

Response: We have endeavoured to clarify this in the manuscript. The analyses were planned a priori.

Results

1. It is a strength that the authors took the time to estimate their catchment area total and homeless population. The authors should provide more details on the estimate of the homeless population in the catchment area (beyond saying it was "estimated by the Dublin Regional Homeless Executive")—is this is a single person who estimated the catchment? Do the authors account for the fact that many homeless people may be transient / move among locations and thus might come from areas outside the "catchment"? (As a related question, did the hostel addresses used fall only inside the "catchment area"?) More information seems important since the authors do present results and make some conclusions based on their catchment area estimates. If the catchment area estimates are problematic, then the authors may be better off presenting only the results that are known (i.e., based solely on the data they collected) rather than presenting data that uses a problematic denominator; or the authors could appropriately frame these results as crude estimates based on their estimated catchment denominator.

Response: This has been amended, and we have highlighted that these estimates are crude (but the increased rate of use of acute unscheduled healthcare in homeless individuals is so dramatic that it would still exceed that of the housed population, even if we underestimated denominator (the number of homeless individuals in the catchment area)

Comment: How did the authors determine cut-offs for “frequent” and “very frequent” ED use?

Response: This has been removed

Comment: Table 1 – the presentation of % of individuals in the last two lines of the table is confusing. It’s unclear exactly what is being shown.

Response: This has been changed

Comment: Tables / Results in general – the results of the statistical testing for comparisons are not always shown, and thus readers are sometimes left to trust the text in the results, or to make their own conclusions for those results not included in the text. For example, the authors state that the admission rates were similar for homeless and non-homeless, and in the table the rates appear to be 17.2% vs. 28.8%--was this difference indeed non-significant in statistical testing? It would be best to show the results.

Response: This has been clarified by adding a new outcome category (assessed) and by reporting admission rates as a proportion of this in the table and text.
In addition, statistical testing has been carried out for categorical outcome variables with a chi-squared test and p-values reported.

Comment: Table 3 – it is interesting that both homeless and non-homeless presented for a wide variety of complaints; the authors could add a line showing that % presented for “other” complaints not captured in the top 10 shown.
This has been added

Response: This has been added

Comment: Table 4 – what is “ITU”?

Response: Intensive care unit – this has been added

Discussion

Comment: In the results the % of homeless who left without being seen is very high (40.7%). Could the authors comment on their thoughts regarding this finding?

Response: 2. This has been added – both the context and our hypotheses as to why they leave

Comment: The authors might devote more time to discussing some of the prior research showing premature aging by homeless populations (studies by Margot Kushel, Rebecca Brown). It seems that one conclusion that could be more explicitly stated is that even young homeless people are quite sick (multiple co-morbidities).

Response: Thank you for highlighting these great studies which I hadn’t come across previously. They have been incorporated into the manuscript.

Comment: The limitations section should be expanded with some of the points made above.

Response: This has been done

VERSION 2 – REVIEW

REVIEWER	Tom Brett School of Medicine The University of Notre Dame Australia, Fremantle Australia
REVIEW RETURNED	13-Jul-2017

GENERAL COMMENTS	This paper has the potential to be a very useful, interesting piece of research but unfortunately it lacks clear direction. It tends to flit between homelessness and multimorbidity on the one hand and hospital Emergency Department presentations and hospital internal medicine acute admissions on the other. An inevitable initial query is whether there are two separate pieces of research here and how best to analyse and present the data. I agree with an earlier reviewer who stated that the Introduction needs to be better focussed with clearly documented objectives and hypotheses and that this focus needs to be maintained in a natural progression through the Methods, Results and Discussion. However, after reading through the paper half a dozen times, I'm still struggling to find a clear statement of the age and sex distribution of the population being examined. This information needs to be presented in a free-standing table outlining: Number of patients Sex of patients – numbers and % Average age – overall, male, female Patients within defined age categories eg <25, 25-44, 45-64, 65-74 and 75+ years. One clear, free-standing Table at the start of the results should leave the reader in doubt as to what precisely the paper is all about. This is missing here and the paper suffers as a result. The operational definition of multimorbidity warrants a clearer statement than the two lines offered in the second part of the Introduction. A major fault is that there is no elucidation of what constitutes 'chronic conditions' and this has implications throughout. There is an abundance of published work available on multimorbidity in both mainstream and marginalised populations but the authors have not made use of it in their literature review. Unfortunately, clear conceptual clarity on what constitutes multimorbidity has not been well developed and the paper suffers because of it. In addition, the authors use the concept of co-morbidities inappropriately – the concept implies an index case of chronicity… You should concentrate on multimorbidities. A key question waiting to be answered is …what is the probability of multimorbidity as a function of age in the cohort(s) under investigation? Fortin and others have espoused the S-shaped curve showing the prevalence of multiple chronic conditions (multimorbidities) across age groups. It would be very helpful if the data in this paper were presented to show how these age groups (especially the 'young homeless') compare to previous research in the area and especially with the homeless and maginalised.

	It would be reasonable to expect that young age groups (<25 and 25-44 years) would show some early peaking of chronic conditions (multimorbidity) in the homeless cohort(s) of this study… At least that is what the published literature tells us! The authors need to be careful with the operational definition of multimorbidity they use… You could have adopted O'Halloran's definition of chronicity (ICPC-2 Fam Pract 2004) … conditions lasting at least 6 months, having a definite pattern of recurrence or deterioration and an impact on a person's quality of life. There is a real risk of flitting between acute conditions – pneumonia/bronchitis, abscess, haematemesis and chronic conditions – COPD/Asthma, IHD/A Fib, Heart failure. The paper fails to address this – in my opinion acute conditions should not be included as examples of multimorbidity. Hence the need to have a clear definition of multimorbidity and adhere to it throughout. Comments and suggestions The paper needs a re-think on how it is presented. It needs a better conceptual framework to be espoused/developed – and keep to it throughout. Multimorbidity is not mentioned in the Methods until the second last sentence... Is multimorbidity not the intended focus in this study? If so, it needs to be clearly spelled out in the Methods – remember the title of the paper! At present, multimorbidity seems to be an after-thought after the patterns and prevalence of Emergency Dept attendances and hospital internal admissions were analysed. As the paper is currently presented, the focus of the study on ED attendances and hospital admissions. (I note the early Reviewer felt similar and queried this). Sub-headings should be added to the Methods to clearly highlight and cover relevant statements on how the study was carried out. It may even be better to consider a two-paper presentation – one dealing with multimorbidity in homeless, young people presenting to hospital Emergency Departments and a second paper dealing with multimorbidity in homeless young people admitted to a large metropolitan hospital as part of internal medicine acute admissions. This clearly calls for a strict definition of 'young' to be adopted and adhered to – suggest <25 and 25 – 44 years ie under 45 years. The denominator population of 'homeless' is recognised as a difficult area. A rigorous approach is therefore needed. The statement that '…high prevalence of age-independent multimorbidity in young homeless adults had not previously been reported' is not correct. It would be better to compare the findings with previous research in the area and report on whether your research supports or refutes this earlier work. What is the prevalence of multimorbidity and how is it calculated? Is it the number of patients with 2+ chronic conditions as a proportion of the total sample? (i) those seen at Emergency Department and/or (ii) those admitted to hospital as part of internal medicine acute admissions? Could the reader have age-sex adjusted prevalence for both cohorts? Tables need re-working as bits missing Overall, the paper needs a major re-think and overhaul including a better literature review, a more focussed Introduction that clearly espouses what multimorbidity is, what homelessness is, the particular age group that is the focus of the study and what the hospital services provide.

	This should progressively funnel down to a clear statement of the Objectives of the study and the study hypotheses. Further suggestions re Methods: Study setting How the population denominator was established Clear operational definitions of multimorbidity and homeless How the data were analysed Results: Better use of Tables to summarise the data Reduction of the narrative Only include data on patients with genuine multimorbidity and the homeless (Acute conditions need to excluded unless the theme of the paper is amended) Discussion: What is new with this research Be open and candid with strengths and limitations Suggest including a statement that disease severity was not canvassed as part of the study. Were there other factors affecting homeless that were peculiar to South Dublin Metropolitan area – urban poverty, travelling community, new migrants, refugees, drug abuse, mental health services… References Need to adopt a uniform pattern as per journal guidelines! Reference 38 and 45 are similar. Needs proof reading… Overall – potential to be a useful addition to current knowledge but the paper needs a lot more work and some hard editing. Perhaps, revisit the focus of the paper(s).

VERSION 2 – AUTHOR RESPONSE

We would like to thank Dr Brett for his thorough and helpful review and for highlighting literature on multimorbidity in homelessness which we had not previously read. On reflection, we agree that the data is best presented as two separate papers. We have removed the data on multimorbidity, and will plan to carry out further work to characterise multimorbidity in our homeless population, probably using a CIRS score in a similar approach to that used by Dr Brett in his studies. We have also revised the layout and flow of the paper, and hope that the revised version is clearer and easier to read.

MJ Open Homeless M-M paper review
This paper has the potential to be a very useful, interesting piece of research but unfortunately it lacks clear direction. It tends to flit between homelessness and multimorbidity on the one hand and hospital Emergency Department presentations and hospital internal medicine acute admissions on the other. An inevitable initial query is whether there are two separate pieces of research here and how best to analyse and present the data.

Thank you. On reflection, we agree that this data would be better presented in two separate papers. We have removed the sections on multimorbidity, and will do further work on those for a future paper.

I agree with an earlier reviewer who stated that the Introduction needs to be better focussed with clearly documented objectives and hypotheses and that this focus needs to be maintained in a natural progression through the Methods, Results and Discussion.

This has been revised

However, after reading through the paper half a dozen times, I'm still struggling to find a clear statement of the age and sex distribution of the population being examined. This information needs to be presented in a free-standing table outlining:
Number of patients
Sex of patients – numbers and %
Average age – overall, male, female
Patients within defined age categories eg <25, 25-44, 45-64, 65-74 and 75+ years.

This has been added for ED attendances (Table 1) and inpatient admissions (Table 5)

One clear, free-standing Table at the start of the results should leave the reader in doubt as to what precisely the paper is all about. This is missing here and the paper suffers as a result.

The operational definition of multimorbidity warrants a clearer statement than the two lines offered in the second part of the Introduction. A major fault is that there is no elucidation of what constitutes 'chronic conditions' and this has implications throughout. There is an abundance of published work available on multimorbidity in both mainstream and marginalised populations but the authors have not made use of it in their literature review. Unfortunately, clear conceptual clarity on what constitutes multimorbidity has not been well developed and the paper suffers because of it. In addition, the authors use the concept of co-morbidities inappropriately – the concept implies an index case of chronicity… You should concentrate on multimorbidities.
A key question waiting to be answered is …what is the probability of multimorbidity as a function of age in the cohort(s) under investigation?
Fortin and others have espoused the S-shaped curve showing the prevalence of multiple chronic conditions (multimorbidities) across age groups. It would be very helpful if the data in this paper were presented to show how these age groups (especially the 'young homeless') compare to previous research in the area and especially with the homeless and maginalised.
It would be reasonable to expect that young age groups (<25 and 25-44 years) would show some early peaking of chronic conditions (multimorbidity) in the homeless cohort(s) of this study… At least that is what the published literature tells us!
The authors need to be careful with the operational definition of multimorbidity they use… You could have adopted O'Halloran's definition of chronicity (ICPC-2 Fam Pract 2004) … conditions lasting at least 6 months, having a definite pattern of recurrence or deterioration and an impact on a person's quality of life. There is a real risk of flitting between acute conditions – pneumonia/bronchitis, abscess, haematemesis and chronic conditions – COPD/Asthma, IHD/A Fib, Heart failure. The paper fails to address this – in my opinion acute conditions should not be included as examples of multimorbidity. Hence the need to have a clear definition of multimorbidity and adhere to it throughout.
These comments are very helpful, and will guide the revised multimorbidity paper.

Comments and suggestions
The paper needs a re-think on how it is presented. It needs a better conceptual framework to be espoused/developed – and keep to it throughout.
Multimorbidity is not mentioned in the Methods until the second last sentence...
Is multimorbidity not the intended focus in this study? If so, it needs to be clearly spelled out in the Methods – remember the title of the paper!
At present, multimorbidity seems to be an after-thought after the patterns and prevalence of Emergency Dept attendances and hospital internal admissions were analysed. As the paper is currently presented, the focus of the study on ED attendances and hospital admissions. (I note the

early Reviewer felt similar and queried this).
Please see above. The paper has now been split into two separate papers.

Sub-headings should be added to the Methods to clearly highlight and cover relevant statements on how the study was carried out.

This has been amended

It may even be better to consider a two-paper presentation – one dealing with multimorbidity in homeless, young people presenting to hospital Emergency Departments and a second paper dealing with multimorbidity in homeless young people admitted to a large metropolitan hospital as part of internal medicine acute admissions.
This clearly calls for a strict definition of 'young' to be adopted and adhered to – suggest <25 and 25 – 44 years ie under 45 years.
The denominator population of 'homeless' is recognised as a difficult area. A rigorous approach is therefore needed.
The statement that '…high prevalence of age-independent multimorbidity in young homeless adults had not previously been reported' is not correct. It would be better to compare the findings with previous research in the area and report on whether your research supports or refutes this earlier work.
What is the prevalence of multimorbidity and how is it calculated? Is it the number of patients with 2+ chronic conditions as a proportion of the total sample? (i) those seen at Emergency Department and/or (ii) those admitted to hospital as part of internal medicine acute admissions? Could the reader have age-sex adjusted prevalence for both cohorts?
Tables need re-working as bits missing

This will be incorporated into the revised paper on multimorbidity
Overall, the paper needs a major re-think and overhaul including a better literature review, a more focussed Introduction that clearly espouses what multimorbidity is, what homelessness is, the particular age group that is the focus of the study and what the hospital services provide. This should progressively funnel down to a clear statement of the Objectives of the study and the study hypotheses.

The introduction has been revised
Further suggestions re Methods:

Study setting
How the population denominator was established

This is included in methode
Clear operational definitions of multimorbidity and homeless

This is included in methods
How the data were analysed

Results:
Better use of Tables to summarise the data
Reduction of the narrative

This has been revised
Only include data on patients with genuine multimorbidity and the homeless
(Acute conditions need to excluded unless the theme of the paper is amended)
This has been revised

Discussion:
What is new with this research
Be open and candid with strengths and limitations
Suggest including a statement that disease severity was not canvassed as part of the study.
Were there other factors affecting homeless that were peculiar to South Dublin Metropolitan area – urban poverty, travelling community, new migrants, refugees, drug abuse, mental health services…
This has been revised

References
Need to adopt a uniform pattern as per journal guidelines! Reference 38 and 45 are similar. Needs proof reading…
These have been revised
Overall – potential to be a useful addition to current knowledge but the paper needs a lot more work and some hard editing. Perhaps, revisit the focus of the paper(s).

Hopefully the revision of the paper has improved focus and flow.

VERSION 3 – REVIEW

REVIEWER	Tom Brett School of Medicine The University of Notre Dame Australia, Fremantle, Australia
REVIEW RETURNED	18-Aug-2017

GENERAL COMMENTS	Thank you for the opportunity to review this revised paper. It reads much better and is more compact. There are a few typos and grammatical errors and some omissions Third author JS - position not listed Data were (rather than was) Demographics are now presented better for both groups For reader clarify, I would add an n= in Table 3 for both the Housed and the Homeless Table 7 is probably nor required - incorporate information into the text I'm not familiar with term 'white Irish' in second paragraph of Discussion. In paragraph 5 of Discussion, do you mean ...we excluded elective admissions to the geriatric service? I feel the Conclusion is rather tame, lacking a bit of punch. Maybe reflect on your objectives, the influence of social determinants of health and the poor investment therein with resultant greater downstream expensive healthcare usage as in EDs... Your populations are large. The multimorbidity angle paper should be written too.

VERSION 3 – AUTHOR RESPONSE

Thank you for the opportunity to review this revised paper.
It reads much better and is more compact.

Thank you!
There are a few typos and grammatical errors and some omissions
Third author JS - position not listed

Added
Data were (rather than was)

Changed
Demographics are now presented better for both groups
For reader clarify, I would add an n= in Table 3 for both the Housed and the Homeless

Added
Table 7 is probably nor required - incorporate information into the text

Agree, changed
I'm not familiar with term 'white Irish' in second paragraph of Discussion.

This is the term used in the Irish census
In paragraph 5 of Discussion, do you mean ...we excluded elective admissions to the geriatric service?

Clarified
I feel the Conclusion is rather tame, lacking a bit of punch. Maybe reflect on your objectives, the influence of social determinants of health and the poor investment therein with resultant greater downstream expensive healthcare usage as in EDs... Your populations are large.

The multimorbidity angle paper should be written too.