

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Examining the relationship between poverty and length of stay: a repeated cross-sectional study of pediatric hospitalizations in Chile
AUTHORS	Borrescio-Higa, Florencia; Santistevan, Dominiquo

VERSION 1 – REVIEW

REVIEWER	john capitman CVHPI/Fresno State USA
REVIEW RETURNED	04-Oct-2019

GENERAL COMMENTS	<p>All of the "no" comments in the above are centered on three issues.</p> <p>1) The abstract/introduction to the paper seems overly concise. The authors have not explained why they focus on public vs. private hospital with an implicit assumption that their behavior around public pay patients will be different. The authors have not provided references and explications of why one might anticipate shorter lengths of stay for children from poorer areas---although the discussion focuses on why more affluent parents with better coverage might accept longer stays, there is no explication in the introduction of why among income public insurance recipients might receive shorter stays in poorer communities. In this context, it would be helpful to discuss how hospitals are reimbursed and what about this process might incentivize shorter stays for public pay patients. The authors also note that patient/family responsibility for public insurance vary by income levels: it is not clear from the text if that means this applies to buying insurance coverage or to out-of-pocket costs. Again this might help develop an argument for why facilities or families in poorer communities might face greater incentives to reduce LOS.</p> <p>My second and perhaps larger concern---acknowledged in the discussion---is that the paper only explores length of stay among those who use insurance. It is hard to interpret the lower LOS in lower income communities without knowing if the likelihood of hospitalizations is also lower. The authors need to explain the decision to explore only LOS. Do they lack data on population sizes and coverage rates by municipality?</p> <p>My third concern is related to the second. The authors focus on overall LOS by children but do not include data on reason for the hospitalization. Studies in the US and elsewhere have suggested that poverty/inadequate primary care are associated with higher rates of hospitalization for some conditions but not others, and a large literature suggests that specific environmental factors such as air and water pollution that are correlated with community poverty influence morbidity rates and interact with coverage differences in explaining utilization. More detail on how diagnoses or other indicators of need for/reason for hospitalization should be</p>
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	provided or at least a rationale for not doing so should be included..
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REVIEWER	Carley Cincinnati Children's, USA
REVIEW RETURNED	23-Dec-2019

GENERAL COMMENTS	<p>I very much appreciate the opportunity to review the manuscript, "Income-based Disparities in Pediatric Inpatient Stays in Chile." In this study, the authors ask a research question of great importance, and I am highly supportive of this type of research. In reviewing the current manuscript, I have a few major concerns and a few minor concerns that I detail below.</p> <p>MAJOR:</p> <ul style="list-style-type: none"> • The authors report relationship between change in poverty level (more/less) and change in LOS (increase/decrease), yet it is unclear whether they studied change over time for any particular population. Rather, it seems that they are describing higher/lower LOS in relation to higher/lower poverty levels. Could the authors assess which is the appropriate terminology and then make it clearer why they use the terminology and apply it consistently throughout the abstract and manuscript? <ul style="list-style-type: none"> o On a related note, it may be helpful to the readers' understanding the results for the readers to have a more robust presentation of if and how the independent and/or explanatory variables changed over time. • In their Discussion, the authors state that their "findings point to a disparity in the utilization of health care services" (page 16, line 26) and their "results are consistent with an over-provision of medical services for richer municipalities, an under-provision of care for poorer ones, or both at the same time" (page 17, lines 28-33). Without data or results related to frequency of admission, reason for admission, and/or outcomes of admission, I find it difficult (perhaps impossible) to determine the significance or implications of the difference in LOS. Perhaps children from higher poverty municipalities are hospitalized more often, for lesser reasons, and therefore experience greater hospitalization though lower LOS on average? If this were the case, children from municipalities with higher poverty could be receiving more acute care services and experiencing more hospital days than those from municipalities with lower poverty. Without this analysis, the authors' discussion and conclusion go well beyond what they are reasonably able to conclude with the data and findings they present. • At the start of the Discussion, the authors state, "At the highest levels of poverty, the relationship between poverty rates and LOS becomes positive" (page 15, lines 34-35), yet there is little, if any, discussion of these results. In fact, it seems that there is no discussion of the non-linearity of the results. The authors should devote more space to these findings. <p>MINOR</p> <ul style="list-style-type: none"> • The authors alternate between using the term "income" and using the term "poverty" when describing the independent variable. While related, income and poverty level are not interchangeable. Since their study uses poverty level rather than income in their regression, I advise that they eliminate the use of the term "income" and use the term "poverty" consistently throughout the title, abstract, manuscript, and tables/figures. • The presentation of all variables is lacking. For example:
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	<p>o Independent variable: How is municipality poverty level determined? What is the source of the data? Frequency of data collection for this variable?</p> <p>o Dependent variable: How is LOS measured? What are the units of measure for the LOS?</p> <p>o Explanatory variables: How are these additional variables collected? What are their source (the same source as LOS)? Frequency of collection (with each hospitalization event)? Are the ICD-10 codes used only to create the complex problem variable?</p> <ul style="list-style-type: none"> • What is the significance in the large SDs for all variables (LOS, age, poverty rate, poverty rate by quintile)? • I'm not entirely certain what the high-turnover hospitalization analysis adds to the findings of this study. The results of the regression seems sufficient for the point made with the secondary analysis with the high-turnover hospitalization. Moreover, the long-stay hospitalization analysis is mentioned but not presented. I would recommend that the authors omit these analyses and utilize the space to address the above concerns. • Before publication, the manuscript would benefit from review for optimal use of the English language. <p>Again, I am very appreciative of this opportunity, and I am delighted to see this kind of research question being asked and studied.</p>
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VERSION 1 – AUTHOR RESPONSE

Reply to Reviewer 1

Dear Dr. John Capitman,

Thank you for your comments and suggestions on our paper. They have been very helpful.

We have now revised the paper to incorporate these considerations. We believe that the revised version has been improved, and hope that you will agree.

Before explaining the specific responses to your comments, we will outline the main changes that we have made.

We have strengthened the main results by adding one specification that allows us to control for the reason for the hospitalization, and also an analysis on hospitalization rates. We have also added an analysis that includes municipality level fixed effects to account for municipality-specific factors (such as environmental factors) which are related to poverty and could affect hospital utilization.

We have clarified the role of reimbursement rates and refined the sample selection to analyse the case of public pay patients at a public hospital in order to hold reimbursement rates constant. Following Reviewer's 2 suggestion, we have dropped the analysis on highturnover hospitalizations, and deepened the discussion on the non-linearity.

We have removed the terminology that was confusing and rewritten several sections of the manuscript accordingly.

A detailed response to your specific comments is provided below. The responses follow the same structure as your report.

The abstract/introduction to the paper seems overly concise. The authors have not

explained why they focus on public vs. private hospital with an implicit assumption that their behavior around public pay patients will be different.

Response:

Thank you for your comment. We have revised the introduction and the abstract

accordingly to address your concerns. We answer each one individually below. Our objective is to understand whether there are disparities in the level of hospital care driven by municipality poverty rates, that go beyond the insurance status of individuals. While ideally one would have a full set of socio-economic characteristics of the patients, the individual level insurance status is the best socio-economic characterization available in the data.

This is why we start the analyses with the distinction of private and public insurance holders, which accounts for 91.5 % of all hospital discharges.

In the previous version of the manuscript we then turned our attention to a dimension that is relevant in this context, which is that of public pay patients in private hospitals. However, this represents a very small proportion of the sample, only 7 percent.

We have given a lot of thought to this, and your other related concerns. We think that the key point in this part of the analysis is to show that within a single reimbursement scheme, poverty rates are still relevant in explaining LOS. We have therefore decided to drop the public pay at private hospital analysis, and replace it with its complement, public pay patients at public institutions, which actually do hold reimbursement schemes constant (column 6 in Table 2). We have rewritten this part of the manuscript to properly explain the aim of the several stratification analyses.

The authors have not provided references and explications of why one might anticipate shorter lengths of stay for children from poorer areas----although the discussion focuses on why more affluent parents with better coverage might accept longer stays, there is no explication in the introduction of why among income public insurance recipients might receive shorter stays in poorer communities. In this context, it would be helpful to discuss how hospitals are reimbursed and what about this process might incentivize shorter stays for public pay patients.

3

Response:

Thank you for this comment. You will find in the new version of the manuscript that we include evidence that children from poorer municipalities are hospitalized less often (Table 3 column 4) and for shorter periods (Table 2, and Table 3 columns 1 and 2), even when controlling for the type of diagnosis (Table 3 column 3). This is consistent with a disparity in the utilization of hospital care for children that is related to poverty. We have revised the manuscript (and rewritten the introduction and discussion sections in particular) to reflect these findings.

While one would argue that financial incentives encourage private providers with an emphasis on profit to perform more procedures than non-profit hospitals, once we control for type of hospitals and type of insurance in our econometric analysis, we are essentially controlling for reimbursement rates. The fact that the main results hold in the “public insurance in a public hospital” only sample, implies that reimbursement rates are not driving the results in this setting. We find that, for example, a public pay child in a public hospital from a poor municipality has a shorter LOS compared to a public pay child in a public hospital from a richer municipality. We have added this information in the Analysis and Discussion sections.

Following your comments and those of the other reviewer we have delved more deeply into what is driving our main results, and have found a potential explanation in the long waiting lists that affect public institutions, which are concentrated among the lowest two tiers of public insurance (A and B), putting more pressure in the operations of the institutions that care for the poorer individuals.

1,2 This higher pressure may have some effect on length of stay because hospitals respond to demand with higher bed turnover rates. We have rewritten the Discussion section in order to provide this explanation in more detail and the relevant references.

1 Subsecretaría de Redes Asistenciales, Ministerio de Salud. GLOSA 06: Lista de espera GES y no GES.

2017.

2 Bedregal P, Ferrer JC, Figueroa B, et al. La Espera En El Sistema de Salud Chileno: Una Oportunidad

Para Poner a Las Personas Al Centro. Vol 12.; 2017.

4

The authors also note that patient/family responsibility for public insurance vary by income levels: it is not clear from the text if that means this applies to buying insurance coverage or to out-of-pocket costs. Again this might help develop an argument for why facilities or families in poorer communities might face greater incentives to reduce LOS.

Response:

Thank you for pointing this out. We have revised the description of the public insurance schemes to make it clear who pays for what, and who can opt to receive care at a private institution (paragraph 2 in page 6).

My second and perhaps larger concern---acknowledged in the discussion---is that the paper only explores length of stay among those who use insurance. It is hard to interpret the lower LOS in lower income communities without knowing if the likelihood of hospitalizations is also lower. The authors need to explain the decision to explore only LOS. Do they lack data on population sizes and coverage rates by municipality?

Response:

Thank you for this comment. The reason why we focus only on insured individuals is because uninsured patients only represent 2.7% of our sample. In the local context, since Chilean public insurance covers individuals who are poor or indigent without any contribution nor copayments, uninsurance rates are not such a pressing problem in relation to poverty, as in other settings. In fact, uninsurance rates are roughly evenly distributed across income quintiles, with a slightly lower percentage among the poorest one.

3 We have

now included a clearer explanation of this in the Background section (paragraph 2, page 6).

3 <https://www.minsal.cl/portal/url/item/cc345de4c9de0ea7e040010164012de8.pdf>

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The objective of this paper is to analyze LOS as an intensive margin of care (once in the hospital, how long is the treatment), instead of the extensive margin, that has to do with the frequency of admission. We have revised the introduction to clarify this point.

Nevertheless, in order to address your point on the likelihood of admission, we computed the hospitalization rates for each municipality, and analyzed their relation to poverty levels in Table 3 column 4. We find that as poverty increases, the hospitalization rates decrease, even when controlling for the type of insurance, type of hospital, and rural status of the municipality. This suggests that children from higher poverty municipalities are not hospitalized more often (for potentially lesser reasons).

My third concern is related to the second. The authors focus on overall LOS by children but do not include data on reason for the hospitalization. Studies in the US and elsewhere have suggested that poverty/inadequate primary care are associated with higher rates of hospitalization for some conditions but not others, and a large literature suggests that specific environmental factors such as air and water pollution that are correlated with community poverty influence morbidity rates and interact with coverage differences in explaining utilization. More detail on how diagnoses or other indicators of need for/reason for hospitalization should be provided or at least a rationale for not doing so should be included.

Response:

Thank you for this comment. We have added two new parts to our analysis that address

this point.

First, we add municipality fixed effects to control for municipality-specific characteristics (such as environmental factors) which are correlated with poverty and can affect utilization. (Table 3 columns 1 and 2). We find that the coefficient on poverty retains the same magnitude and significance in each new specification.

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Second, in order to account for the reason of hospitalization, we create a subsample with only diagnosis codes for which there are over 1000 cases, which accounts for 68% of the total sample.⁴

We proceed to replicate the main analysis, including the diagnosis code as a control in column 3 of Table 3. The results remain robust, that is both the magnitude and the significance remain similar to the main results. The interpretation of this specification is that even when controlling for the diagnosis code (and all of the other characteristics), poverty rates are still negatively associated with the length of stay.

Reply to Reviewer 2

Dear Dr. Carley Riley,

Thank you for your comments and suggestions on our paper. All of your comments were very thoughtful. We have now revised the paper to incorporate these considerations. We believe that the revised version has been improved, and hope that you will agree.

Before explaining the specific responses to your comments, we will outline the main changes that we have made.

We have strengthened the main results by adding one specification that allows us to control for the reason for the hospitalization, and also an analysis on hospitalization rates. We have also added an analysis that includes municipality level fixed effects to account for municipality-specific factors (such as environmental factors) which are related to poverty and could affect hospital utilization.

We have clarified the role of reimbursement rates and refined the sample selection to analyse the case of public pay patients at a public hospital in order to hold reimbursement rates constant. Following your suggestion, we have dropped the analysis on high-turnover hospitalizations, and deepened the discussion on the non-linearity.

We have removed the terminology that was confusing and rewritten several sections of the manuscript accordingly.

A detailed response to your specific comments is provided below. The responses follow the same structure as your report.

2

I very much appreciate the opportunity to review the manuscript, "Income-based Disparities in Pediatric Inpatient Stays in Chile." In this study, the authors ask a research question of great importance, and I am highly supportive of this type of research. In reviewing the current manuscript, I have a few major concerns and a few minor concerns that I detail below.

MAJOR

The authors report relationship between change in poverty level (more/less) and change in LOS (increase/decrease), yet it is unclear whether they studied change over time for any particular population. Rather, it seems that they are describing higher/lower LOS in relation to higher/lower poverty levels. Could the authors assess which is the appropriate terminology and then make it clearer why they use the terminology and apply it consistently throughout the abstract and manuscript?

Response:

Thank you for this comment. The estimation strategy of this paper is based on the use of fixed effects for identification. The use of geographical area (region or municipality) fixed

effects allows to estimate this relationship not by comparing high versus low poverty area (which one could argue may not be a valid comparison) but off the variation within each area over time.

Further, municipality fixed effects allow to control for any unobserved differences between municipalities.

We understand that the terminology may become confusing, because we estimate the relationship between municipality level poverty and LOS (more vs less), but econometrically speaking, the correct way to interpret the beta coefficient is in terms of “an increase of 1 unit in the independent variable is associated with a certain change in the dependent variable” (increase/decrease).

We have now revised the terminology and have rewritten several sections of the paper to make this clear.

3

On a related note, it may be helpful to the readers’ understanding the results for the readers to have a more robust presentation of if and how the independent and/or explanatory variables changed over time.

Response:

Thank you for pointing this out. We have now expanded Table 1 to include information on the level of each relevant variable over time.

In their Discussion, the authors state that their “findings point to a disparity in the utilization of health care services” (page 16, line 26) and their “results are consistent with an over-provision of medical services for richer municipalities, an under-provision of care for poorer ones, or both at the same time” (page 17, lines 28-33). Without data or results related to frequency of admission, reason for admission, and/or outcomes of admission, I find it difficult (perhaps impossible) to determine the significance or implications of the difference in LOS. Perhaps children from higher poverty municipalities are hospitalized more often, for lesser reasons, and therefore experience greater hospitalization though lower LOS on average? If this were the case, children from municipalities with higher poverty could be receiving more acute care services and experiencing more hospital days than those from municipalities with lower poverty. Without this analysis, the authors’ discussion and conclusion go well beyond what they are reasonably able to conclude with the data and findings they present.

Response:

This is a good point, and we have given a lot of thought in how to answer it. We have tackled two of the three dimensions that you mention: frequency of admission, reason for admission, and/or outcomes of admission

In terms of the frequency of admission, we computed the hospitalization rates for each municipality, and analyzed their relation to poverty levels in column 4 of Table 3. We find that as poverty increases, the hospitalization rates decrease, even when controlling for the

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type of insurance, type of hospital, and rural status of the municipality. This suggests that children from higher poverty municipalities are not hospitalized more often, for lesser reasons.

With respect to the reason for admission, we have now created a subsample with only diagnosis codes for which there are over 1000 cases, which accounts for 68% of the total sample.¹

We proceed to replicate the main analysis, including the diagnosis code as a control in column 3 of Table 3. The results remain robust, that is both the magnitude and the significance remain similar to the main results. The interpretation of this specification is that even when controlling for the diagnosis code (and all of the other characteristics), poverty rates are still negatively associated with the length of stay.

Finally, as for the outcomes of admission, the data does not allow us to dig deeper. The

only available outcome is dead vs. alive discharge, and the sample of dead discharges is (fortunately) too small for us to study. As mentioned in the paper, the dataset does not allow to identify patients across hospitalization events, so that we cannot track readmissions.

We believe that with this new evidence, we can more confidently state that there is a disparity in the utilization of hospital care services, consistent with an under-provision of care for poorer municipalities, as children from poorer municipalities are hospitalized less often and for shorter periods.

At the start of the Discussion, the authors state, "At the highest levels of poverty, the relationship between poverty rates and LOS becomes positive" (page 15, lines 34-35), yet there is little, if any, discussion of these results. In fact, it seems that there is no discussion of the non-linearity of the results. The authors should devote more space to these findings.

Response:

1 Note that the 1000 threshold of cases was chosen arbitrarily, and that results hold for any number.

Results

available upon request.

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Thank you for pointing this omission. We have now included a more detailed discussion on the non-linearity in the Discussion section (page 17).

MINOR

The authors alternate between using the term "income" and using the term "poverty" when describing the independent variable. While related, income and poverty level are not interchangeable. Since their study uses poverty level rather than income in their regression, I advise that they eliminate the use of the term "income" and use the term "poverty" consistently throughout the title, abstract, manuscript, and tables/figures.

Response:

You are absolutely correct. We have revised the paper to correctly reflect that the analysis is based on poverty, not income.

The presentation of all variables is lacking. For example:

- Independent variable: How is municipality poverty level determined? What is the source of the data? Frequency of data collection for this variable?
- Dependent variable: How is LOS measured? What are the units of measure for the LOS?
- Explanatory variables: How are these additional variables collected? What are their source (the same source as LOS)? Frequency of collection (with each hospitalization event)? Are the ICD-10 codes used only to create the complex problem variable?
- What is the significance in the large SDs for all variables (LOS, age, poverty rate, poverty rate by quintile)?

Response:

Thank you for pointing out these omissions. We have rewritten several paragraphs in the Data description section to add the missing information.

6

In terms of your last point, large SDs point to a big variability in the dataset associated with working with the universe of hospital discharges in a country where there are very large disparities across municipalities.

I'm not entirely certain what the high-turnover hospitalization analysis adds to the findings of this study. The results of the regression seems sufficient for the point made with the secondary analysis with the high-turnover hospitalization. Moreover, the long-stay hospitalization analysis is mentioned but not presented. I would recommend that the authors omit these analyses and utilize the space to address the above concerns.

Response:

Thank you for the suggestion. We have now dropped these analyses.

Before publication, the manuscript would benefit from review for optimal use of the English language.

Response:

Thank you for the suggestion. We have worked with a professional copy-editing service, which has helped improve the readability and quality of the manuscript.

Again, I am very appreciative of this opportunity, and I am delighted to see this kind of research question being asked and studied.

VERSION 2 – REVIEW

REVIEWER	John Capitman Central Valley Health Policy Institute Fresno State University, Fresno CA USA
REVIEW RETURNED	10-Feb-2020

GENERAL COMMENTS	<p>The author(s) have significantly improved the paper and responded to most request for clarification. I think there are two areas that need to be addressed. This is a multilevel study and the goal is to establish the impact on pediatric admission and length of staying in relation neighborhood poverty, institution type, and year while controlling for person level factors such as insurance status, gender, and age. The authors have conducted OLS regression with log transform of the dependent variable and some of the independents in both usual and quadratic equation. I am not sure that this approach correctly addresses the effects of clustering by hospital and place (I would have used multilevel regression with MLE estimates of effects). They made additional statistical choices I did not fully understand....Given that the dependent variable of pediatric utilization really is formed by likelihood of utilization and utilization (days of care) for users, the authors selected to focus on days of care among users, which makes the story feel incomplete. Perhaps these choices can be defended a bit more....And, I do think an additional statistical review is appropriate. If the approach taken is acceptable, it would be helpful to explain how this approach addresses the concerns about standard errors etc addressed in the multilevel models (I think the reference to the clustering of error terms at the region reflects that the author(s) are aware of the need for more explanation here.</p> <p>One other suggestion. The complex relationship between neighborhood poverty and pediatric lengths of stay (with fewer admissions in poorer areas with some caveats) is explained by the authors in two ways: poorer people have less adequate access to care because of supply and reimbursement barriers.....and...lower income parents may be less willing to agree to extended hospital stays because of out-of-pocket expenses. This second explanation would appear to violate the multilevel interpretative fallacies (See Diez-Roux https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1508189/pdf/amjph0014-0044.pdf) and it also seems contradicted by supplemental analyses referenced in the text did not show an effect for public insurance co-pay amount. The authors would be more prudent and responsible to seek systems-level explanations for differences in length of stay (and likelihood of admissions) like role of primary care access in mediating both in-patient admissions and discharge, availability of primary care services in lower income communities, travel or other built environmental factors that make it harder for</p>
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	families to manage extended stays, availability of supportive community services in lower income places seem like possible explanations and there are many more that don't end up "blaming the victims" of unequal health care and social safety nets.
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REVIEWER	Carley Riley Cincinnati Children's Hospital, USA
REVIEW RETURNED	24-Feb-2020

GENERAL COMMENTS	I appreciate the opportunity to review the revised manuscript, and I appreciate how diligent and thorough the authors have been in tending to the considerations raised by the other reviewer and me. The revised manuscript -- and what it contributes to the pediatric health disparities literature -- is much stronger. I look forward to your team's future contributions to our collective understanding of disparities in pediatric healthcare delivery, systems, and outcomes.
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VERSION 2 – AUTHOR RESPONSE

Reply to Reviewer 1

Dear Dr. John Capitman,

Thank you once more for your comments and suggestions on our paper. They have been very helpful. We have now revised the paper to incorporate these considerations. We believe that the revised version has been improved, and hope that you will agree.

A detailed response to your specific comments is provided below. The responses follow the same structure as your report.

Two Areas that need to be addressed

1. *This is a multilevel study and the goal is to establish the impact on pediatric admission and length of staying in relation neighborhood poverty, institution type, and year while controlling for person level factors such as insurance status, gender, and age. The authors have conducted OLS regression with log transform of the dependent variable and some of the independents in both usual and quadratic equation.*

I am not sure that this approach correctly addresses the effects of clustering by hospital and place (I would have used multilevel regression with MLE estimates of effects)...

Given that the dependent variable of pediatric utilization really is formed by likelihood of utilization and utilization (days of care) for users, the authors selected to focus on days of care among users, which makes the story feel incomplete.

Perhaps these choices can be defended a bit more.... And, I do think an additional statistical review is appropriate.

If the approach taken is acceptable, it would be helpful to explain how this approach addresses the concerns about standard errors etc. addressed in the multilevel models (I think the reference to the clustering of error terms at the region reflects that the author(s) are aware of the need for more explanation here.

Response:

Dependent Variable

We agree that pediatric utilization is formed by both the likelihood of utilization and the days of care for users. However, we have defined the main focus of our analysis as hospital length of stay, which is a measure of treatment intensity received by children, conditional on being admitted.¹ We follow several papers in choosing to analyze the intensive margin of care by focusing on LOS.²⁻⁴

We do study the likelihood of admission by calculating average hospitalization rates across municipalities, and present these results in Table 3. Naturally we do not observe individual level characteristics of those who are not admitted. You have raised a very interesting point nevertheless, and we have now submitted an application to study ER data, to analyze the likelihood of being admitted to inpatient care. This will be the base for future research.

Empirical Strategy

- Model

We have adjusted the terminology and rewritten several paragraphs in the analysis section to clarify that when we refer to fixed effects models, we are referring to a special class of multilevel analysis models.⁵

After a careful revision of several alternative models, we have decided that a fixed effects model is the most appropriate. Fixed effect models -- a primary model for causal inference with longitudinal data -- allow adjustment for unobserved time-invariant confounders (such as omitted variables, endogeneity, or selection bias). For example, unobserved area level characteristics such as environmental factors or access to primary care may be correlated with both poverty rates and LOS, resulting in a biased estimation of the effect of poverty rates on LOS in an OLS regression. The fixed effects model has been used in different settings for similarly structured (multilevel) data (though not necessarily hospital discharges).^{2,6-9}

Note that the fixed effects (FE) model is estimated through ordinary least squares.¹⁰ Crucially, the FE estimator allows for arbitrary correlation between the unobserved effect and the explanatory variables in any time period.¹⁰ In this sense, we believe that the model with municipality fixed effects correctly accounts for all municipality level characteristics, such as access to primary care, environmental factors, and transportation facilities, in order to control for the heterogeneity across municipalities. This implies that the model allows us to estimate the causal effect of municipality level poverty rates on LOS, controlling for a set of individual and group level characteristics.

Following your suggestion, we have added a paragraph in the Analysis section, defending our choice of empirical strategy and listing its advantages. We have also edited several paragraphs in this section for clarity.

- Clusters

You are absolutely correct that the use of clustered standard errors accounts for the residual correlation among individuals within groups.^{10,11}

Though there is no consensus on which level of clustering is appropriate, we take the most conservative approach of clustering at the same level as the fixed effect (region or municipality). In order to address your concern that the relevant level of clustering might be the hospital, we have now included in our analysis a series of regressions with clusters at the hospital level. Appendix Table 2 presents the estimation of fixed effects models to show that the main results are robust to the changes in the specification.

2. *The complex relationship between neighborhood poverty and pediatric lengths of stay (with fewer admissions in poorer areas with some caveats) is explained by the authors in two ways: poorer people have less adequate access to care because of supply and reimbursement barriers.....and...lower income parents may be less willing to agree to extended hospital stays because of out-of-pocket expenses.*

This second explanation would appear to violate the multilevel interpretative fallacies (See Diez-Roux <https://www.ncbi.nlm.nih.gov/pmc/articles/PMC1508189/pdf/amjph00014-0044.pdf>) and it also seems contradicted by supplemental analyses referenced in the text did not show an effect for public insurance co-pay amount.

The authors would be more prudent and responsible to seek systems-level explanations for differences in length of stay (and likelihood of admissions) like role of primary care access in mediating both in-patient admissions and discharge, availability of primary care services in lower income communities, travel or other built environmental factors that make it harder for families to manage extended stays, availability of supportive community services in lower income places seem like possible explanations and there are many more that don't end up "blaming the victims" of unequal health care and social safety nets.

Response:

We have added Diez-Roux (1998) to our references, thank you for pointing it out. Indeed, both individual and group level characteristics matter in explaining the relationship between municipality poverty rates and pediatric LOS.

You are right to point out that there is no evidence to suggest that the willingness to accept potentially unnecessary treatment is a relevant explanation, and we have therefore decided to remove it from both the Discussion and Introduction sections.

We now state in the Discussion section that the most plausible explanation is related to primary care access and hospitals' operational constraints, including long waiting lists. These explanations are both consistent with evidence presented elsewhere.^{12–15}

References

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4. Almond D, Doyle JJ. After Midnight: A Regression Discontinuity Design in Length of Postpartum Hospital Stays. *American Economic Journal: Economic Policy*. 2011;3(August):1-34.
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