PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Variation in access to community rehabilitation services and length of stay in hospital following a hip fracture: a cross-sectional study
AUTHORS	Neuburger, Jenny; Harding, Karen; Bradley, Rachel; Cromwell, David; Gregson, Celia

VERSION 1 - REVIEW

REVIEWER	Jessica Jarvis University of Texas Medical Branch, USA
REVIEW RETURNED	19-May-2014

GENERAL COMMENTS	This study examined regional variation in availability of inpatient rehabilitation services on transfer rates from acute hospitals and lengths of stay (both acute and combined) in older adults with hip fracture. Combining several data sources (hospital records, patient interviews [I think], and census data) is a notable strength of the study. Moreover, studies identifying modifiable inequities in access to care are certainly valuable contributions to the literature. These potential strengths aside, there are several aspects of this manuscript that diminish both its clarity and its implications. Select comments and suggestions are listed below by section. ABSTRACT 1. “Hospital access” to community rehab is misleading. “Patient access” to community rehab would be more accurate, and is seemingly determined by his or her GP / PCT (pg 6, line 45). 2. Access to home-based rehab services is listed as a main outcome. This variable is not included in any analyses, yet is emphasized in the Conclusions. One PCT had no home-based services. Are we to assume equal access across the other six PCTs? 3. All Abstract Results are presented by PCT, not the 9-level variable described in the Methods and Results of the main paper. See additional comments below. 4. Lines 31-34: Switching from “poor access to community rehab beds” to “lacking home-based rehab” for same outcome (in same sentence) is confusing. 5. Lines 34-38: Values are provided for the adjusted difference in super-spell LOS, but not acute LOS. Also, the reference group for the difference is not indicated. INTRODUCTION 6. 60,000 older adults per year with hip fracture is a substantial burden. The significance could be enhanced by indicating the incidence relative to the total older adult population in England and if the number of older adults is increasing as it is in other countries.

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	7. The ambiguity regarding “super-spell” begins here and continues through the Methods and Results. I assume it is simply the total inpatient days (acute hospital and rehabilitation), whether the rehab occurred in the same facility or after transfer to another location. I also assume the acute and rehab lengths of stay could be calculated for all patients in the sample. However, it may be that both segments are available for some patients and only the combined super-spell is available for patients in certain settings. Regardless, clarity is needed throughout. Also, it would be informative to provide the mean and variation in acute length of stay as is given for the super-spell (pg 5, lines 39-40). 8. The purpose statement (pg 5, lines 49-54) highlights either a complete misunderstanding on my part or a critical mismatch between the stated objectives and the research design. Everyone in the sample received both acute and community rehab services. Thus, what is the comparison group when examining the relationship between use of community rehab services and length of stay? METHODS 9. Regarding the survey (questionnaire), did a single clinician interview all 1,376 patients? Also, did the provision of "local community rehabilitation services" include inpatient and home-based? Inpatient rehab admissions were determined by HES claims. Were interview data simply used to check agreement? And again, home-based info not presented in the Results. 10. Logistic regression with transfer to CRH as the target group implies some patients were not transferred. Yet, use of CRH was an inclusion criterion. 11. Uncertainty related to the sample / outcome notwithstanding, the entire approach could be simplified to both better match the objectives and facilitate reader understanding. It is clearly stated that a patient’s PCT determines his or her eligibility for community rehab (pg 6, lines 50-52). Thus, PCT is the logical variable of interest. Hospital of origin and all patient-level factors can still be in the model (and coefficients shown), but PCT should be the independent variable in all analyses and the primary topic of discussion. Currently, the initial half of the Results are stratified by hospital. This transitions to PCT by hospital and ultimately, a combination of PCT and hospital. The process of creating nine categories from hospital-PCT referrals and then reducing that to three categories is not only confusing, but it isn’t introduced until the Results section. Lastly, the research question and data structure are best suited for multilevel models (e.g. hierarchical linear or hierarchical generalized linear models), which would account for the clustering of patients within PCTs. RESULTS 12. Pg 8, lines 27-31: Need to clarify in text and/or table that orthogeriatrician sessions are 1 hr each. 13. Overall, the Results contain some potentially interesting findings, but they are difficult to follow and make it impossible to reach a clear conclusion. DISCUSSION 14. The Discussion is well-written and comprehensive. Unfortunately, it is based on the disjointed Results obtained from the current methodological approach. TABLE 1 15. What is #NOF?

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REVIEWER	Helen Handoll Teesside University, UK
REVIEW RETURNED	25-May-2014

GENERAL COMMENTS	I think this is a useful and well done study, and well and interestingly reported. In explanation for where I have put "N". 4. Some missing: selection criteria for the well defined region, missing questionnaire, why 11 months (rather than 1 HES year), why < 5 days between acute and CRH (what happens in between?). What is the basis for the categories for transfer rate (Table 4). Were these prespecified and what it was 10%? 7. I am concerned that the authors are overdoing / over-interpreting the statistics. The potential confounders are powerful and I also wonder if there should be an adjustment for clustering. I am not expert enough to tell; hence my suggestion for a specialist statistical review. I would like to see the mortality data presented. 11 & 12. Clearly this links with the above point. Thus while, the article gives valuable insights on the variation in practice, I think it probably overplays the tests for significance. Some potential questions for discussion / limitations include: How many of these patients were from a nursing home and sent back there; direct from the acute hospital? This could be a key confounder. Is 'bed blocking', reflecting waiting for a suitable nursing home, a problem for any of these hospitals? What were the length of stay distributions like (were there some extreme outliers?). While there were some data on morbidity, post-op complications (e.g. joint infection) can greatly extend length of stay and other general infection (e.g. norovirus) can play havoc with bed management too. Potential limitations but also were these something you could / did ask about / get data on? Greater recognition required of the recent reorganisation of health services. Thus it would seem useful to do the same research and report on this say in 3 years time. More is needed on what extra value this adds to the insights on trends / activity from the NHFD. Recognition and some assessment of the quality and completeness of HES data. Perhaps some insights on how many you would have missed if you had set the transfer target to < 2 days? More discussion on ongoing orthogeriatican care - it perhaps isn't surprising that this stopped at acute care? How can this be remedied? "Super-spell" seems a rather inept descriptor for extended stay or total stay. I appreciate that it is used in the NHFD and NICE guideline but I suggest that it would be better not to perpetuate its

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	usage. Did all participants have surgery (likely but a few may not; useful to comment on this). Figure 2: a) and b) need labels. Also need to make it clearer why there are 9 spots (acute hospital + PCTs) rather than 4 (for acute hospitals). Table 1. Not sure that PA: Programmed activity is used. Data supplement Table A. Seems important that PCT 6 serves 2 hospitals. Please add in something about this.

REVIEWER	Steven Ariss University of Sheffield, School of Health and Related Research, UK
REVIEW RETURNED	02-Jun-2014

GENERAL COMMENTS	4) it would be useful to have sight of the standardized questionnaire used to establish service provision and access criteria. 5) access to HES data and anonymisation process are not described. 6) It is not clear whether CRH included home-based care/day care etc CRH could have a clearer definition 8) NAIC report 2012 is used, why not 2013? 11) The discussion section 'findings in context: there are several assumptions in paragraph one, which are not supported by the findings or cited literature. 12) Whilst I have stated that limitations are discussed adequately, there are some issues, which might require attention detailed below. A limitation of the study is that it does not account for re-admissions to acute services from community services. This is potentially important when reporting super-spells (e.g. a fast average discharge to community services might result in higher re-admission rates and therefore extended average super-spells). Page 8 Line 45-50: this point is not very clear. Do the authors mean that access has more to do with allocation of places rather than just the number of available places? The PCT context is, to a certain extent, not relevant in the modern health economy. Some discussion of this would be useful to contextualise the findings. One patient characteristic that is an important determining factor for reducing LOS is living arrangements. This is not explored and I would consider this a limitation. Page 12 Line 45-50. Why do the authors consider lack of information about the use of social care beds unlikely to bias estimates? Page 12 Line 53-Page 13 Line 3. The PCT with missing records

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	accounts for about one third of discharges from one acute hospital. However, it is not clear which hospital this is. Is it the hospital with the lowest rates of discharge to PCTs? Page 14 lines 5-7. Discussion about home-based and bed-based care is a little confusing as there does not seem to be a distinction made between these in the analysis. See earlier comment about CRH definition.

VERSION 1 – AUTHOR RESPONSE

Reviewer Name Jessica Jarvis
Institution and Country University of Texas Medical Branch, USA

ABSTRACT
1. “Hospital access” to community rehab is misleading. “Patient access” to community rehab would be more accurate, and is seemingly determined by his or her GP / PCT (pg 6, line 45).

>> We have amended the text accordingly. We have also explained more clearly, throughout the paper, that patient access to community rehabilitation services depends upon both the acute hospital in which they are treated and the Primary Care Trust of their GP.

2. Access to home-based rehab services is listed as a main outcome. This variable is not included in any analyses, yet is emphasized in the Conclusions. One PCT had no home-based services. Are we to assume equal access across the other six PCTs?

>>We have now clarified that we used findings of an organisational survey of orthogeriatricians to describe access to services, including home-based rehabilitation. It was not possible to quantify use of home-based rehabilitation from the administrative hospital-based data source that we used; we simply know whether a service was or was not available.

3. All Abstract Results are presented by PCT, not the 9-level variable described in the Methods and Results of the main paper. See additional comments below.

>>We have clarified that the results relate to eight patient groups with varying levels of access to community rehabilitation services, dependent upon the combination of both their acute hospital and PCT. As an aside, it is now eight not nine groups for purposes of simplicity (see response to reviewer 3).

4. Lines 31-34: Switching from “poor access to community rehab beds” to “lacking home-based rehab” for same outcome (in same sentence) is confusing.

>>We have made this suggested amendment.

5. Lines 34-38: Values are provided for the adjusted difference in super-spell LOS, but not acute LOS. Also, the reference group for the difference is not indicated.

>> In response to reviewer comments elsewhere, we have simplified the analysis and now simply present associations between rate of transfer to a community rehabilitation hospital and the acute hospital LOS and combined LOS.

INTRODUCTION

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6. 60,000 older adults per year with hip fracture is a substantial burden. The significance could be enhanced by indicating the incidence relative to the total older adult population in England and if the number of older adults is increasing as it is in other countries.

>> We have now noted that the figure will increase with the ageing of the population. The incidence is similar to that in Western Europe (eg, around 1 in a 1,000) but we decided against including any further statistics in the introduction.

7. The ambiguity regarding “super-spell” begins here and continues through the Methods and Results. I assume it is simply the total inpatient days (acute hospital and rehabilitation), whether the rehab occurred in the same facility or after transfer to another location. I also assume the acute and rehab lengths of stay could be calculated for all patients in the sample. However, it may be that both segments are available for some patients and only the combined super-spell is available for patients in certain settings. Regardless, clarity is needed throughout. Also, it would be informative to provide the mean and variation in acute length of stay as is given for the super-spell (pg 5, lines 39-40).

>> We have altered the manuscript throughout in response to this comment, plus comments of another reviewer. We now refer to the combined length of stay, rather than the super-spell. It is correct that we don’t know the combined length of stay for patients who are transferred to residential care homes for temporary rehabilitation. However, this represents a very small number and proportion of patients in our sample (eg, 2-3%), otherwise data are complete. We present medians rather than means.

8. The purpose statement (pg 5, lines 49-54) highlights either a complete misunderstanding on my part or a critical mismatch between the stated objectives and the research design. Everyone in the sample received both acute and community rehab services. Thus, what is the comparison group when examining the relationship between use of community rehab services and length of stay?

>> We have altered the manuscript throughout to clarify that we are interested in community rehabilitation services provided outside the acute hospital, to which access varied. In our quantitative analysis, we looked at the relationship between transfer to community rehabilitation hospitals outside the acute hospital and length of stay. One hospital transferred very few of their patients out of the acute hospital for rehabilitation, in part because there was very limited access to such services.

METHODS

9. Regarding the survey (questionnaire), did a single clinician interview all 1,376 patients? Also, did the provision of "local community rehabilitation services" include inpatient and home-based? Inpatient rehab admissions were determined by HES claims. Were interview data simply used to check agreement? And again, home-based info not presented in the Results.

>> We have amended the manuscript to clarify that the organisational survey was of orthogeriatricians, rather than patients, to determine for each hospital, their access to rehabilitation services for their patient population. A copy of the structured questionnaire is now included as material in a supplementary appendix.

10. Logistic regression with transfer to CRH as the target group implies some patients were not transferred. Yet, use of CRH was an inclusion criterion.

>> We have clarified our description of the inclusion criteria. Transfer to CRH was not an inclusion criterion.

11. Uncertainty related to the sample / outcome notwithstanding, the entire approach could be

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simplified to both better match the objectives and facilitate reader understanding. It is clearly stated that a patient's PCT determines his or her eligibility for community rehab (pg 6, lines 50-52). Thus, PCT is the logical variable of interest. Hospital of origin and all patient-level factors can still be in the model (and coefficients shown), but PCT should be the independent variable in all analyses and the primary topic of discussion. Currently, the initial half of the Results are stratified by hospital. This transitions to PCT by hospital and ultimately, a combination of PCT and hospital. The process of creating nine categories from hospital-PCT referrals and then reducing that to three categories is not only confusing, but it isn't introduced until the Results section. Lastly, the research question and data structure are best suited for multilevel models (e.g. hierarchical linear or hierarchical generalized linear models), which would account for the clustering of patients within PCTs.

>> We have simplified our analyses and descriptions throughout the paper. Our original reason for reducing our patient groups to three categories with "low" "medium" and "high" rates of transfer was to aid understanding. Given that it didn't achieve this purpose, we have reverted to simple rank-based correlation coefficients to describe associations.

RESULTS
12. Pg 8, lines 27-31: Need to clarify in text and/or table that orthogeriatrician sessions are 1 hr each.

>> Thank you, one session is equivalent to four hours of either morning or afternoon work and we have clarified this in the legend of Table 1

13. Overall, the Results contain some potentially interesting findings, but they are difficult to follow and make it impossible to reach a clear conclusion.
>> We agree that this analysis raises some interesting findings; we have simplified the manuscript throughout, to show our clearer conclusions.

DISCUSSION

14. The Discussion is well-written and comprehensive. Unfortunately, it is based on the disjointed Results obtained from the current methodological approach.
>> Thank you, we have now addressed this as referred to above and revised the discussion accordingly.

TABLE 1
15. What is #NOF?
>> This is a short clinical abbreviation for fractured neck of femur; we have now amended this for clarity.

Reviewer Name Helen Handoll
Institution and Country Teesside University, UK

I think this is a useful and well done study, and well and interestingly reported. In explanation for where I have put "N".

Are the methods described sufficiently to allow the study to be repeated?
Some missing: selection criteria for the well defined region, missing questionnaire, why 11 months (rather than 1 HES year), why < 5 days between acute and CRH (what happens in between?). What is the basis for the categories for transfer rate (Table 4). Were these prespecified and what it was 10%?

>> We used 11 months so that we did not downward bias estimates of length of stay – our data

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extract from HES only contains discharge dates going up to 30 March 2012, with the result that a high % of those admitted in April 2012 had incomplete LOS; we now explain this point in the methods.
>> In fact the <5 day threshold makes no difference as a criteria, so we have removed this sentence in the methods. Out of 375 patients that were counted as transfers to a CRH, 361 (96%) had the same recorded date of discharge/transfer from acute hospital and admission to CRH. Eight patients (2 %) had a discrepancy of one day between the dates, four patients (1 %) had a discrepancy of two days; and two patients (<0.5%) had a discrepancy of 3 or 4 days. Because all of these patients had a transfer to or from another NHS institution coded in their records, we assume that date discrepancies are due to coding error. If we restricted the definition to those with the same day, it would make no difference to our results.
>> Based on all reviewers' comments we have decided to remove presentation of the categorisation of the transfer rate in favour of simple rank-based correlation between observed transfer rate and median LOS.

If statistics are used are they appropriate and described fully?
I am concerned that the authors are overdoing / over-interpreting the statistics. The potential confounders are powerful and I also wonder if there should be an adjustment for clustering. I am not expert enough to tell; hence my suggestion for a specialist statistical review. I would like to see the mortality data presented.

>> We agree that there is potential for confounding. However, when we adjusted for patient's age, sex, comorbidity, socio-economic deprivation and rural habitation, it made no difference to the association between transfer rate and LOS. This is perhaps because the factors that have most influence on LOS (eg, age) did not vary substantially between the eight patient groups with varying access to community rehabilitation services. Consequently, it was institutional factors that dominated the association.
>> In order to reduce the emphasis on statistical interpretation, we have simplified the analyses, as part of which we have moved description of adjusted analyses to a supplementary appendix.
>> We did consider clustering: as well as checking correlations between adjusted rate of transfer and adjusted LOS, we also checked correlations between random-effects (shrunken residuals) from multilevel models. However, because it did not alter our conclusions, we stuck to the simpler analysis. It makes no difference to our results because the number of clusters is small (N = 8) and the number of patients per cluster is large (minimum of 64 patients, average of 176 patients). If the reviewer thinks it would be helpful, we would be happy to include these extra analyses as supplementary data; however, given the general reviewer feedback that our analyses should be simplified, we have not done so currently.

Are the discussion and conclusions justified by the results?
Are the study limitations discussed adequately?
Clearly this links with the above point. Thus while, the article gives valuable insights on the variation in practice, I think it probably overplays the tests for significance. Some potential questions for discussion / limitations include:

How many of these patients were from a nursing home and sent back there; direct from the acute hospital? This could be a key confounder.

Is 'bed blocking', reflecting waiting for a suitable nursing home, a problem for any of these hospitals? What were the length of stay distributions like (were there some extreme outliers?).

>>Across the 4 acute hospitals the proportion of patients admitted from a nursing home or residential care was similar (17% to 19%), but the proportion discharged from the acute trust to a nursing home or residential care was more variable (14% to 24%). These figures come from the NHFD audit report

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2011/2. Some of the discharges to nursing homes will be new placements, which generally do 'block beds', but it is not possible from our data to determine who these individuals were. It is also not possible to determine the direction of bias this would impose. For example, more affluent areas may have a higher proportion of 'self-funding' patients who do not rely on social services to agree funding before placement and therefore they are generally discharged sooner. However, the hospitals span a number of social services and the extent of heterogeneity in these services is not clear. We have now added discussion of this point to the limitations section.

>>Length of stay in hospital was positively skewed. The 95% percentile LOS in the acute hospital was 51 days, and the 95% percentile combined LOS in the acute hospital and CRH was 73 days. There were also some outliers with extremely long LOS. For this reason, we used geometric means in our original analysis, which places less weight on extreme values than the arithmetic mean. In the revision, we have simplified the main analysis and no longer presented adjusted differences in LOS. For this reason, we now present median LOS (figures are very close to the geometric means).

While there were some data on morbidity, post-op complications (e.g. joint infection) can greatly extend length of stay and other general infection (e.g. norovirus) can play havoc with bed management too. Potential limitations but also were these something you could / did ask about / get data on?

>>Unfortunately we lacked data on general infection (e.g. norovirus). If a prosthetic joint becomes infected it generally requires revision surgery. Rates of reoperation were low and similar across these 4 hospitals (<3.5%), albeit there were issues with missing data in the NHFD 2011/2 for this variable. We have now added discussion of this point to the limitations section.

Greater recognition required of the recent reorganisation of health services. Thus it would seem useful to do the same research and report on this say in 3 years time.

>> Yes, we agree, we tried to use the most recent complete dataset; however, this clearly relates to PCT rather than the new CCGs now structuring our health care services. As the CCGs in this study area in SW England mirror the map of old PCTs we judged our analysis to still be relevant to the 'New NHS'. We have now included comment on this point in the discussion. We also refer to PCTs as 'former' PCTs.

More is needed on what extra value this adds to the insights on trends / activity from the NHFD.
>>We have tried to make our contribution clearer in the introduction (paragraph 3). Whereas the NHFD reports focus on care of hip fracture patients in the acute hospital, our study focuses on variation in access to and use of community rehabilitation services for these patients, and the impacts on length of stay. We have been able to show that a higher rate of transfer to CRH is associated with a shorter length of stay in the acute hospital, but a longer combined length of stay, suggesting reduced efficiency.

Recognition and some assessment of the quality and completeness of HES data. Perhaps some insights on how many you would have missed if you had set the transfer target to < 2 days?
>> Two of the authors (JN and DC) carried out work for the NHFD to assess the quality of HES data using a linked extract of HES and NHFD. This work established robust methods for identifying patients with a hip fracture in HES, and calculating hospital and super-spell LOS. This report is available via the NHFD website at: http://www.nhfd.co.uk/20/hipfractureR.nsf/ResourceDisplay

>> Regarding your point about the transfer target of < 2 days, please see our response to your first comment made above.

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More discussion on ongoing orthogeriatican care - it perhaps isn't surprising that this stopped at acute care? How can this be remedied?
>>We agree that this is not surprising, but felt this was an interesting point to highlight given NICE's recommendations regarding continuity of hip fracture care, as mentioned in the discussion: findings in context. Community geriatrics is an expanding sub-specialty within geriatric medicine and it may be that this role will offer opportunities for continuing of care with ongoing geriatrician input for those patients in community beds; of course this will be dependent upon the commissioning of appropriate services. We add discussion of this point the section 'findings in context'.

"Super-spell" seems a rather inept descriptor for extended stay or total stay. I appreciate that it is used in the NHFD and NICE guideline but I suggest that it would be better not to perpetuate its usage.
>>Thank you, this point was also raised by reviewer 1; we have now amended the manuscript in favour of the term combined length of stay, rather than super-spell.
Did all participants have surgery (likely but a few may not; useful to comment on this).
>>In the year 2011/2, nationally (data from NHFD) 2.6% of hip fractures were managed non-operatively. Across the 4 acute hospital trusts rates recorded in the NHFD 2011/2 ranged from 1.2% to 2.0%. These patients were included in the analyses; but given the low and similar rates across all 4 hospitals, combined with the fact that none of these patients would have been transferred to CRHs (a non-operative/palliative approach generally negates CRH rehabilitation), we felt their inclusion was unlikely to bias our results. We have now added comment regarding this to the limitations.

Figure 2: a) and b) need labels. Also need to make it clearer why there are 9 spots (acute hospital + PCTs) rather than 4 (for acute hospitals).

>>After considering all reviewers' comments as a whole we have decided to remove Figure 2 from this manuscript, as these analyses we judged too confusing. We replace this with an alternative unadjusted figure with clear labelling.

Table 1. Not sure that PA: Programmed activity is used.
>>We have removed the term PA and amended wording for clarity.
Data supplement Table A. Seems important that PCT 6 serves 2 hospitals. Please add in something about this.

>>Thank you, in fact 3 hospitals service 2 PCTs, we have now included this point in the results: access to community rehabilitation services, with reference to Table A.

Reviewer Name Steven Ariss
Institution and Country University of Sheffield, School of Health and Related Research, UK

Please state any competing interests or state 'None declared': None declared

Are the methods described sufficiently to allow the study to be repeated?
It would be useful to have sight of the standardized questionnaire used to establish service provision and access criteria.
>>We have now included the semi-structured questionnaire which was used to support interviews between the study orthogeriatrician and each of the four hospital orthogeriatricians as supplementary material.

Are research ethics (e.g. participant consent, ethics approval) addressed appropriately?
Access to HES data and anonymisation process are not described.
>> Hospital episode statistics were made available by the NHS Health and Social Care Information Centre (copyright 2012, reused with the permission of the Health and Social Care Information

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Centre). An anonymised copy of the HES database is securely stored, managed and accessed at the Royal College of Surgeons Clinical Effectiveness Unit. The database manager, Lynn Copley, provided an extract for this work, analysed by JN who is based within the CEU. An anonymised patient identifier, derived from the patient's NHS number, is used to match admissions of the same patient to different hospitals.

Are the outcomes clearly defined?
It is not clear whether CRH included home-based care/day care etc CRH could have a clearer definition
>> We defined a community rehabilitation hospital (CRH) as a local NHS institution providing on-site integrated health and social care with specifically inpatient access to physiotherapy for the purpose of rehabilitation; this contrasts with home-based rehabilitation and care services provided after discharge from a hospital in a patient's own home. We have now added this definition to the methods.

Are the references up-to-date and appropriate?
NAIC report 2012 is used, why not 2013?
>>We have now updated the introduction to include discussion of this reference

Are the discussion and conclusions justified by the results
The discussion section 'findings in context: there are several assumptions in paragraph one, which are not supported by the findings or cited literature.
>> This paragraph describes the potential context of our findings, based upon our opinions having looked after patients with hip fractures for some years; but as the evidence in support of these opinions is limited, we have now said so, added a reference, and reworded to emphasise the point that these are opinions not statements.

Are the study limitations discussed adequately?
Whilst I have stated that limitations are discussed adequately, there are some issues, which might require attention detailed below.

I would recommend this paper for publication, following some fairly minor revisions (detailed in the comments to authors and above), mostly for clarity.

A limitation of the study is that it does not account for re-admissions to acute services from community services. This is potentially important when reporting super-spells (e.g. a fast average discharge to community services might result in higher re-admission rates and therefore extended average super-spells).

>>We had originally hoped to include analysis of readmissions in this paper. In the end, we did not because of low numbers and consequent unstable estimates, which were sensitive to sample exclusions. In the HES sample analysed, the rate of readmission to the acute hospital was 3.7% among patients transferred to a CRH, compared to 2.0% among patients not transferred to a CRH (ie, discharged home). Adding the LOS in the acute hospital to the combined LOS does not alter the relationship between transfer rate and combined LOS.

>> Considering readmissions also raised questions about how to define this usefully. From the patient's point of view as well as the clinical perspective, transfer from a CRH back to the acute hospital, even within the same acute trust, should be counted as a readmission, since it represents clinical deterioration and need for acute care. This is how we defined it. However, readmission is often not measured this way, and the measure most commonly used is emergency readmission to the trust, which misses out within trust transfers (e.g., Quality Watch report http://www.qualitywatch.org.uk/focus-on/hip-fracture).

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Page 8 Line 45-50: this point is not very clear. Do the authors mean that access has more to do with allocation of places rather than just the number of available places?
>> Yes thank you, we have used the wording you suggest to clarify that access to CRH beds was dependent upon allocation of beds and demands on CRH beds from people other than hip fracture patients.

The PCT context is, to a certain extent, not relevant in the modern health economy. Some discussion of this would be useful to contextualise the findings.
>> Thank you, we have now included discussion of the move to CCGs and the relevance to our study

One patient characteristic that is an important determining factor for reducing LOS is living arrangements. This is not explored and I would consider this a limitation.
>> According to the 2011/12 NHFD report, the proportion of patients admitted from home ranged from 75% to 81%. We agree that we did not use the information from HES on source of admissions because we did not consider the information to be sufficiently detailed (i.e., >90% are admitted from "usual place of residence" which includes warden-controlled accommodation).

Page 12 Line 45-50. Why do the authors consider lack of information about the use of social care beds unlikely to bias estimates?
>> Our organisational survey established limited use of social care funded beds for rehabilitation. Across the four hospitals, our estimated rates of transfer to CRH from HES were only a few percentage points lower than NHFD estimated rates of discharge to a rehabilitation unit, which includes social-care funded beds, reported in the 2011/12 NHFD report. Hence, the numbers are too low to distort the differences in transfer rates. Further, only if stays in social-care funded rehab beds were unusually short would the observed association between transfer rate and combined LOS be biased upward.

Page 12 Line 53-Page 13 Line 3. The PCT with missing records accounts for about one third of discharges from one acute hospital. However, it is not clear which hospital this is. Is it the hospital with the lowest rates of discharge to PCTs?
>> It is Hospital D with the highest rate of transfer to CRH. For simplicity, we have now excluded patients from this PCT from our analysis altogether. This just reduces the number of groups in our analysis from nine to eight, representing different levels of access to community rehabilitation services. This exclusion has minimal impact on our estimates.

Page 14 lines 5-7. Discussion about home-based and bed-based care is a little confusing as there does not seem to be a distinction made between these in the analysis. See earlier comment about CRH definition.
>>We have now clearly defined community and home based rehabilitation in the methods

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