

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Influence of social deprivation on provision of bariatric surgery - 10-year comparative ecological study between two UK specialist centres
AUTHORS	Bhandari, Shivam; Alam, Mushfique; Matthews, Jacob; Rudge, Gavin; Noble, Hamish; Mahon, David; Richardson, Martin; Welbourn, Richard; Super, Paul; Singhal, Rishi

VERSION 1 - REVIEW

REVIEWER	Elaine Burns Imperial College London, UK
REVIEW RETURNED	29-Jul-2015

GENERAL COMMENTS	<p>This is a well-written manuscript that seeks to address an important question- the variable access to bariatric services in the United Kingdom.</p> <p>The paper seeks to examine the impact of a patients social deprivation score on the likelihood of receiving bariatric surgery over a ten year period. The study compares two centres that vary significantly in terms of geography and social deprivation.</p> <p>I am unclear on the data sources used by the group to obtain the obesity prevalence estimates. How is obesity defined for this cohort? In much of the publically available data, obesity is defined as a BMI >30. NICE guidelines (2014 after the study duration) suggest bariatric surgery in those patients that have a BMI of 40 kg/m² or more, or between 35 kg/m² and 40 kg/m² and other significant disease (for example, type 2 diabetes or high blood pressure) that could be improved if they lost weight. The authors need to be very clear (if applicable) that the denominator of obesity prevalence used is not the same as the number of patients that may be eligible for such bariatric surgery.</p> <p>Another key area that requires significant clarification is the justification for choosing two areas with such different characteristics for comparison. The authors touch on this in their discussion but the areas differ considerably in social deprivation status and obesity levels. In addition, the urban versus rural effect will be significant. Indeed ethnic variation will be also different. These factors make it difficult to draw meaningful conclusions about the results. The role of private practice should also be explored with examining surgery rates and social deprivation. The private sector may play a greater role in one unit over the other.</p> <p>Variation in time should also be considered. The authors include a 10-year time frame. As demonstrated by the Public Health England</p>
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	<p>statistics (http://www.noo.org.uk/NOO_about_obesity/adult_obesity/UK_prevalence_and_trends), obesity rates (BMI >30) have significantly increased during the study period as well as a substantial increase in bariatric surgery rates, changes with centralization of services and referrals patterns. It would be useful at the very least to include year of surgery in the linear model.</p> <p>It would be useful to clarify further the methodology used to define excluded patients based on catchment areas.</p> <p>I am unclear how many operated patients were included in the study. This is important to understand the power of the data included and the similarity of the two units.</p> <p>Further exploration of the possible biases would be useful. The authors rightly point out that the data reflect surgery carried out rather than referral to services. A comment on the surgical practice of both centres would be important. One centre may have a higher propensity to operate than the other skewing the data significantly.</p> <p>The paper would benefit from being reviewed by a statistician.</p> <p>Finally the conclusions of the paper should be tempered given the many possible confounding variables that the authors have been unable to comment on with the data available. From such a study design a causal link cannot be demonstrated.</p>
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REVIEWER	ANNA PEETERS BAKER IDI HEART AND DIABETES INSTITUTE AUSTRALIA
REVIEW RETURNED	17-Aug-2015

GENERAL COMMENTS	<p>This study addresses a topical issue of whether receipt of bariatric surgery is influenced by socio-economic position. Some interesting results are presented using a combination of data sources that suggest some differences in treatment levels according to socio-economic position. The main issue with the article is that two UK specialist centres are chosen and it is not clear why these were selected, nor what any observed differences between the centres might mean. In my opinion the apparent differences between the centres weaken any conclusions from the paper.</p> <p>Specific comments include:</p> <ol style="list-style-type: none"> 1. Article summary point 4 is difficult to understand 2. How reliable is the method used to define the catchment area around each hospital? 3. Does the accuracy of obesity prevalence estimation vary with area level deprivation? 4. Was ethics obtained for this study? 5. The conclusion on page 9 lines 51-3 does not seem to relate to the results reported in that paragraph 6. Was a direct comparison between centre 1 and 2 tested? The magnitude of difference does not seem very great and may be simply random variation 7. When discussing centre 2 it would be important to conclude that the lines increased with increasing area level deprivation
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	<p>8. As discussed above I am not sure what you can conclude from the apparent differences between centres 1 and 2. You state this confirms a postcode lottery but if this is already known what does your study add?</p> <p>9. Why are Centres 1 and 2 generally relevant areas?</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1 Comments

Reviewer 1; Comment 1: I am unclear on the data sources used by the group to obtain the obesity prevalence estimates. How is obesity defined for this cohort? In much of the publicly available data, obesity is defined as a BMI >30. NICE guidelines (2014 after the study duration) suggest bariatric surgery in those patients that have a BMI of 40 kg/m² or more, or between 35 kg/m² and 40 kg/m² and other significant disease (for example, type 2 diabetes or high blood pressure) that could be improved if they lost weight. The authors need to be very clear (if applicable) that the denominator of obesity prevalence used is not the same as the number of patients that may be eligible for such bariatric surgery.

Thank you for your comments. We agree that the source of obesity prevalence data is not made fully clear. Obesity prevalence estimates by middle-layer super output area (MSOA) were obtained from model-based estimates from Public Health England for 2006-2008 based on data from the Health Survey for England. We have ensured that this is cited in the methods section (reference number 23). We also agree that using a cross sectional estimate of obesity is less reliable given the rising prevalence of obesity in both populations during the study period, and have identified this limitation in the discussion section. Furthermore, obesity as BMI > 30 does not equate to eligibility for bariatric surgery under NICE guidelines, and therefore we have had to assume a constant relationship between BMI > 30 and BMI > 40 in our study, which we have included in our discussion.

Discussion: “IMD score and Public Health England obesity estimates, although the most recent available, were from 2010 and 2006 respectively. We used a cross sectional estimate of obesity in all of our populations, when clearly obesity rates are changing constantly. We assume, although cannot prove conclusively, that obesity rates have been generally rising...

There is a paucity of data on prevalence of BMI>40 in the population and so we have used BMI>30 as an indicative variable. This would appear reasonable, as it has been shown from data from Health Survey for England (HSE) that the UK’s median BMI has increased over time but there has been a larger increase in the 95th percentile. Nonetheless, there remains an assumption in this study that the relationship between BMI>30 and BMI>40 is more or less constant across different regional populations.”

Reviewer 1; Comment 2: Another key area that requires significant clarification is the justification for choosing two areas with such different characteristics for comparison. The authors touch on this in their discussion but the areas differ considerably in social deprivation status and obesity levels. In addition, the urban versus rural effect will be significant. Indeed ethnic variation will be also different. These factors make it difficult to draw meaningful conclusions about the results. The role of private practice should also be explored with examining surgery rates and social deprivation. The private sector may play a greater role in one unit over the other.

Thank you for your comments. We purposefully selected two populations which differ in their sociodemographic distribution in order to assess the effect of this on the provision of bariatric surgery. In our opinion, NICE guidelines on who should be offered specialist weight management services including bariatric surgery are clearly based on BMI and the presence of relevant co-morbidities (such as diabetes or cardiovascular disease). Therefore there *ideally* should be no influence based on urban vs rural residence or social deprivation status. However, we do agree that ethnic variation may play a role due to differing health behaviours, cultural attitudes and engagement with health services. Bariatric services at both centres are similarly established and are expected to have similar surgical experience. Finally, we agree private practice could play an important part however this group of patients are self selected rather than provided with treatment. Secondly, data were not available for

sufficient private providers in both regions and catchment areas would not be definable. Nonetheless, further research investigating this topic in more detail will need to consider inclusion of private procedures in their analyses.

We have redrafted the first part of the methods section to include an expanded justification for sample selection.

Methods: “Two centres were chosen for which there were sufficient demographic and geographical data available for consecutive patients who had received bariatric surgery over a 10 year period (2003-2013). These centres were deemed appropriate for the study as they are within two different health economies and are both similarly well established and have similar sized bariatric teams. Finally, both are fully subject to NICE guidelines on provision of obesity services but are different in sociodemographic distribution, which allows for investigation into whether this difference is associated with a difference in provision of surgery.”

Reviewer 1; Comment 3: Variation in time should also be considered. The authors include a 10-year time frame. As demonstrated by the Public Health England statistics (http://www.noo.org.uk/NOO_about_obesity/adult_obesity/UK_prevalence_and_trends), obesity rates (BMI >30) have significantly increased during the study period as well as a substantial increase in bariatric surgery rates, changes with centralization of services and referrals patterns. It would be useful at the very least to include year of surgery in the linear model.

Thank you very much for your comments and advice. Fortunately the HSE data used by PHE to model obesity at a local level were captured part way through the period of observation. Whilst we accept that the use of point prevalence estimates as variables in studies where data were captured longitudinally is not ideal, we feel that error arising from this is not great. Using the reference cited either side of the 2006 estimate there has been nationally about a 2% increase between our start point and the point estimates and about 1.5-2% increase from point estimate to endpoint.

We have updated our methodology to incorporate time in our linear model by splitting the study period into three separate time periods in order to investigate the change in the relationships between obesity and deprivation with provision of surgery as time progressed. Our 'Methods' section has been significantly updated and our 'Results' section rewritten to report our new findings. New figures have been generated in order to display the results of our model for each epoch.

Discussion: “IMD score and Public Health England obesity estimates, although the most recent available, were from 2010 and 2006 respectively. We used a cross sectional estimate of obesity in all of our populations, when clearly obesity rates are changing constantly. We assume, although cannot prove conclusively, that obesity rates have been generally rising. This raises the question of the validity of using a cross sectional estimate in this work. The alternative would have been to model obesity prevalence over time. This would have been methodologically very difficult in the resources available to the authors and the precision of such an estimate would make the use of it in a model statistically difficult. We were able however to model the changes in the overall relationships over time by making models for each epoch. This was a pragmatic choice rather than performing a conventional time series analysis, but we did succeed in showing that the modelled associations changed over time in both centres and in slightly different ways.”

Reviewer 1; Comment 4: It would be useful to clarify further the methodology used to define excluded patients based on catchment areas.

Thank you, we agree that our description could be expanded. We have strengthened our description of sample selection in the methods section.

Methods: “We were interested in surgery in populations with reference to population characteristics. This is problematic as hospitals do not have agreed catchment areas based on residence for most services, and may provide services for people living some distance away if they have no local provider. We therefore determined notional catchment areas where virtually all treated cases resident there would have been treated in one of our study centres.

Analysis of national Hospital Episode Statistics (HES) data identified suitable areas. These were co-terminus with existing administrative boundaries and were geographically contiguous, resulting in two separately isolated health economies containing a centre for bariatric surgery that served separate population groups. In the case of the West Midlands, our area was defined by residence in the Local Authority areas of Birmingham City Council and the Metropolitan Borough Council of Solihull. In the case of the South West, our area was defined by the local authority areas of North Somerset Council contiguously south to Plymouth City Council."

Reviewer 1; Comment 5: I am unclear how many operated patients were included in the study. This is important to understand the power of the data included and the similarity of the two units.

1250 bariatric cases were included in the analysis - 501 cases in Centre 1 (Birmingham) and 749 cases in centre 2 (South West). This is stated in the first paragraph of the results section. Furthermore, the sample sizes have been added to the demographic summary table (Figure 1).

Results: "Data were collected from 1250 bariatric cases (501 from Centre 1 and 749 from Centre 2) which were geographically mapped and included in the statistical analysis."

Reviewer 1; Comment 6: Further exploration of the possible biases would be useful. The authors rightly point out that the data reflect surgery carried out rather than referral to services. A comment on the surgical practice of both centres would be important. One centre may have a higher propensity to operate than the other skewing the data significantly.

Thank you for this advice. We acknowledge the following sources of bias and or confounding in the conclusions: local commissioning factors, ethnicity, gender and referral to specialist weight services. To expand the discussion further regarding sources of bias we have added a second paragraph into the subsection labelled "*Meaning of the study: possible explanations and implications for clinicians and policymakers*", to add further qualifications to our conclusions and reinforce the need for limitations to be considered.

Discussion: "Furthermore, differences in surgical practice between the two centres, for example if one centre had a higher propensity to operate than the other, may skew the results of this study. However, this would suggest variation in adherence to NICE guidelines between the two regions."

Reviewer 1; Comment 7: The paper would benefit from being reviewed by a statistician.

GR was the author who devised the statistical methods used in this study and carried out the analysis. Additional statistical support was sought from the University of Birmingham since initial submission which GR has discussed with the Editorial Office. Following review of the manuscript, the methodology has been updated as per *Reviewer 1 Comment 3*. Should the editor request an independent review from a statistician we would welcome this decision.

Reviewer 1; Comment 8: Finally the conclusions of the paper should be tempered given the many possible confounding variables that the authors have been unable to comment on with the data available. From such a study design a causal link cannot be demonstrated.

Thank you for this crucial comment. We agree that a causal link cannot be demonstrated from this study due to its design and confounding sources of bias for which data were unfortunately not available. We have commented on this in the discussion, removed a stated suggestion that Centre 2 may be targeting its bariatric services more effectively (which our data cannot prove sufficiently) and have also expanded the final comments of the discussion to describe that our study demonstrates a need to better understand the relationship between obesity and the commissioning of bariatric surgery across the UK in order to ensure an evidence based approach to service provision.

Discussion: "A crude interpretation of our findings could point to possible inequalities in access in Centre 1, however we must temper our conclusions with regard to the many other supply and demand side factors that can affect this relationship."

~~Removed: "Observations resulting from this investigation suggest that in Centre 2, the service is better targeted to those based on clinical need as social deprivation has less effect on the provision of surgery. On the other hand, it could be suggested that demand in the area of Centre 1 is not being adequately met and that an increase in capacity may be required in regions such as this."~~

~~Discussion: "As a nation, we spend a large amount of money on the treatment of obesity, including bariatric surgery and much more money on treating the consequences of obesity. In this paper we argue for a more scientific approach to understanding the complex relationship between supply and demand for bariatric treatment. We do not propose that we have built a definitive model of these factors in this simple observational study. However given the policy imperative of providing treatment for those most in need equitably, we conclude that a much larger study with more recent and more robust population obesity prevalence estimates, but with a broadly similar methodological approach, could usefully inform service development."~~

Reviewer 2 Comments

Reviewer 2; Comment 1: Article summary point 4 is difficult to understand.

Thank you for this comment. This particular summary point aimed to state that ideally social deprivation should have no influence on provision of bariatric surgery, yet our data demonstrated evidence that less deprived areas have higher uptake of bariatric services.

In light of changes to our linear model following comments from Reviewer 1 we have changed this conclusion accordingly.

Article Summary: "As a service that should be provided based on clinical need, bariatric surgery should be strongly related to obesity prevalence, and this relationship should be constant against across socio-economic strata. However, we have found this is not the case in one of the two populations studied."

Reviewer 2; Comment 2: How reliable is the method used to define the catchment area around each hospital?

Thank you, please see our response to 'Reviewer 1 Comment 4' where we have outlined an expanded explanation regarding hospital catchment areas. On the issue of reliability, we accept that both centres do not have pre-determined catchment areas, but we feel that usage of Hospital Episode Statistics to determine a geographical area where each centre would be the most local provider.

Reviewer 2; Comment 3: Does the accuracy of obesity prevalence estimation vary with area level deprivation?

Thank you for this interesting question. We have obtained our obesity prevalence data from 2006 Public Health England data which we believe is the most reliable data source to be used in this study. A limitation likely to have more impact would possibly be the fact that from 2006 to 2013 prevalences may have changed, and we have incorporated a time element into our linear models to further investigate this effect.

Reviewer 2; Comment 4: Was ethics obtained for this study?

Thank you for this question. Ethical approval was not required for this study as the data used is routinely collected. Location points on maps produced have a deliberate error margin added to further protect patient confidentiality.

Reviewer 2; Comment 5: The conclusion on page 9 lines 51-3 does not seem to relate to the results reported in that paragraph.

Thank you, we agree that this paragraph is slightly unclear. In light of our newer analysis incorporating a time element our results section has been significantly modified and we hope our explanations of the data are easier to understand.

Reviewer 2; Comment 6: Was a direct comparison between centre 1 and 2 tested? The magnitude of difference does not seem very great and may be simply random variation.

Thank you for this question. We agree that many comparisons made between the two populations included in our study are inferences from analysing them individually. A direct statistical comparison between two centres in the relationships between all the study variables would be difficult to interpret and would likely not be meaningful given the concurrent limitations of the study. Our data highlights the observable differences in changing relationships between provision of surgery, obesity prevalence and social deprivation over time. Further research on the provision of bariatric surgery will need to overcome these difficulties, particularly if commissioning of surgery is to be evidence-based as recommended in our conclusions.

Reviewer 2; Comment 7: When discussing centre 2 it would be important to conclude that the lines increased with increasing area level deprivation.

Thank you for this observation. As stated previously, we have updated our manuscript with more up to date results based on refinements to our statistical methodology and our conclusions are now based on the updated analyses.

Reviewer 2; Comment 8: As discussed above I am not sure what you can conclude from the apparent differences between centres 1 and 2. You state this confirms a postcode lottery but if this is already known what does your study add?

Thank you for your question. Concern over the possibility of a postcode lottery has been suggested by the Royal College of Surgeons of England, however our study is the first to investigate and report on this issue statistically and highlights the need to adopt an evidence-based approach as this issue is investigated in future. We have updated our 'Discussion' section to expand on this conclusion.

Reviewer 2; Comment 9: Why are Centres 1 and 2 generally relevant areas?

Thank you for your comments. We agree that the two study populations derived in our study have significant demographic differences. These two regions were chosen so that we can investigate the relationships between some of these very differences and the provision of bariatric surgery in these areas. Our study has shown not only that provision of surgery in relation to local obesity is not only different between the two study populations but also that over time these relationships have changed differently. These conclusions may echo ongoing concerns over the emergence of a 'postcode lottery' in the provision of bariatric surgery.

VERSION 2 – REVIEW

REVIEWER	Chong-Chi Chiu Chi Mei Medical Center, Taiwan
REVIEW RETURNED	29-Mar-2017

GENERAL COMMENTS	<ol style="list-style-type: none"> 1. This study was generally well designed. 2. The authors replied almost all questions raised by the reviewers 3. The authors used Index of Multiple Deprivation score to evaluate socioeconomic deprivation, which was different from other studies. This increased the reliability of data collection. 4. As the authors mentioned, total demand for bariatric surgery in both centres was not really investigated by this study. However, many obese patients should not receive surgery initially but try diet modification or even intra-gastric balloon placement. So if the authors could describe some information of these aspects, this may provide more information about prevalence and severity of obesity in both different regions. 5. This manuscript could be accepted for publication after revision.
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REVIEWER	Chun-Che Huang Institute of Health Policy and Management, National Taiwan University, Taipei, Taiwan
REVIEW RETURNED	01-Apr-2017

GENERAL COMMENTS	<p>The authors have revised their manuscript according to the reviewers' comments. For the improvement, several minor issues of this study are needed to be more clarified.</p> <p>In the Methods section, 1) the authors only reported the final sample without clear reporting the sample selection. How many patients were excluded from study sample by these exclusion criteria? Please describe in detail the inclusion and/or exclusion criteria. 2) However, was the more powerful analytic method used (did the analysis control for any baseline differences (i.e., other patient's sociodemographics and annual surgical volume of each centre) between the two study populations/centres)? Please clarify this.</p> <p>In the Discussion section, 1) data were collected from the two centres which could limit the generalization of results. In addition, other unmeasured factors, such as surgical volume of centres and practice size (number of bariatric surgeons) may also influence on provision of bariatric surgery. Please make clear about the limitation of the study.</p>
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REVIEWER	Nicholas Finer University College London Hospitals
	I am co-author with some of the authors on an unrelated report and paper.
REVIEW RETURNED	07-Apr-2017

GENERAL COMMENTS	<p>This paper sets out to explore the impact of socioeconomic deprivation on the provision of bariatric surgical care in two different regions of England. This is an important topic but I am not sure that I fully understand the validity of the techniques used and how these really add to our understanding of the provision and uptake of bariatric surgery in England.</p> <p>The over-riding concern that I have is that we already know that much fewer than 1% of patients eligible under NICE guidance receive surgery, so there is little likelihood that a sample of 1200 patients is representative of the population who could receive surgery. Looking for patterns of variation within an at best 1% sample seems prone to substantial error, and undermine the power of the comparisons between the two centres. Furthermore, there is already ample evidence for a postcode lottery driven by different decisions on eligibility and volume between commissioning bodies.</p> <p>Other issues include: what was the incidence of surgery done in the private sector (data that might be available from the NSBR) and could this bias the SE distribution in the NHS service (issue raised by previous reviewer)? How long did patients wait between referral and surgery – this could undermine the attribution of epoch to any individual patient. Was there a difference in the type of procedures</p>
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	<p>performed between the two centres? Was the presence of co-morbidity (particularly T2D) similar in the two samples? One could envision that a difference in this could relate both to SE status, ethnicity and surgical 'prioritisation'.</p> <p>Can the authors give data on the total number of referrals to each unit during the time periods? This would be valuable in showing the problem was in referral rather than unit activity and or selection. Indeed, if these data exist they might be a fairer way of exploring the impact of SE status on access to surgery than starting with those who received it.</p> <p>Page 5, L5: I am not sure that WHO global estimates are germane to this paper.</p> <p>Page 5, L39: This sentence doesn't make sense</p> <p>Page 5, L45: Provision or uptake?</p> <p>Page 5, L56: Is this so? Funding bodies may dictate the volume they commission, but are surely not involved in individual clinical decisions?</p> <p>Page 6, L6: It has, not may! See the variation in care maps from D of H.</p> <p>Page 7, L 27: But are commissioning and eligibility guidelines and contracts the same between the two centres? See page 12 discussion.</p> <p>Page 9, L13: Statistically but not clinically significant.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer 1 Comments

Reviewer 1; Comment 4: As the authors mentioned, total demand for bariatric surgery in both centres was not really investigated by this study. However, many obese patients should not receive surgery initially but try diet modification or even intra-gastric balloon placement. So if the authors could describe some information of these aspects, this may provide more information about prevalence and severity of obesity in both different regions.

Thank you for your comment regarding the lack of data about patients referred to weight management services but did not undergo bariatric surgery. Unfortunately, we do not have sufficiently reliable data on the treatment history prior to surgery routinely for either of the two bariatric units included in our study. Nonetheless, we would like to point out that all patients included should have attempted conservative measures to lose weight before surgery would be considered. We have added an additional sentence to the "Strengths and limitations of this study" subsection in the Discussion to reinforce this.

Discussion: "Unfortunately, the authors were unable to reliably obtain data on the treatment history of included patients prior to surgical intervention, such as weight loss via conservative measures which is suggested by NICE prior to any bariatric surgery."

Reviewer 2 Comments

Reviewer 1; Comment 1: The authors only reported the final sample without clear reporting the sample selection. How many patients were excluded from study sample by these exclusion criteria? Please describe in detail the inclusion and/or exclusion criteria?

Thank you for your question seeking clarification of the sample selection in our study. We feel that we have explained our inclusion criteria and methodology behind deriving the two catchment areas studied, but agree that it is not clear in our Methods section that patients who were not resident sufficiently locally to either of the bariatric centres were excluded from the analysis. We have clarified this further in the Methods section and have included information in the Results section on the numbers excluded.

Methods: "Any patients who received surgery at either of the two centres selected but were not resident in either of the notional catchment areas were excluded from the study. As the West Midlands catchment area was smaller, we expected there to be fewer observations from this centres compared to the South West, despite the similar overall caseload of both bariatric units."

Results: "At Centre 1, 1875 patients underwent bariatric surgery during the study period. Of these, 414 were resident in the derived catchment area. 14 further patients were excluded due to missing or incomplete data, leaving 414 cases included for analysis. For Centre 2, there were 1196 documented cases with complete case note data of which 749 were resident in our study area. Therefore, data were collected from 1163 bariatric cases in total (414 from Centre 1 and 749 from Centre 2) which were geographically mapped and included in the statistical analysis."

Reviewer 2; Comment 2: However, was the more powerful analytic method used (did the analysis control for any baseline differences (i.e., other patient's sociodemographics and annual surgical volume of each centre) between the two study populations/centres)? Please clarify this.

Thank you for this comment which raises the issue of adjusting for surgical volume in our refined generalised linear model. The sample sizes were very different but both units were very similar in size. As we excluded patients not resident in Birmingham, the West Midlands centre had many fewer observations in the study. We have reinforced this point in the revisions made in the reviewer's previous comment. However, in general we do not see the value in adjusting for unit size in a study of just two centres, although in our planned future analysis of national trends we would of course make an adjustment for surgical volume as it could be hypothesised that small services behave differently to larger ones.

Reviewer 2; Comment 3: Data were collected from the two centres which could limit the generalization of results. In addition, other unmeasured factors, such as surgical volume of centres and practice size (number of bariatric surgeons) may also influence on provision of bariatric surgery. Please make clear about the limitation of the study.

Thank you, we agree that the issue of potential differences in surgical volume between bariatric units needs to be highlighted in our manuscript. The reviewer is correct to identify that variety in the size of bariatric units affects the generalisability of this study in which two centres of similar size are studied. We have therefore highlighted this point in our Discussion section.

Discussion: "Secondly, there are likely to be several other factors that influence both the demand for and provision of bariatric surgery in the two regions studied. These include referral behaviours of primary care providers, patient ethnicity and the presence of co-morbidities. For example, estimated type 2 diabetes prevalence in adults is different in the two populations. In North Somerset and

Plymouth the percentage of over 17 year olds is estimated as 5.54% whereas in Birmingham it is 7.61%. Although surgical volume was judged to be similar in the two regions selected for this study, the certain variety in the size of bariatric units across the UK will affect the generalisability of the results of this study to the rest of the country.”

Reviewer 3 Comments

Reviewer 3; Comment 1: I am not sure that I fully understand the validity of the techniques used and how these really add to our understanding of the provision and uptake of bariatric surgery in England.

Thank you for your comment. The techniques involved modelling the extent to which different variables predicted surgical volume in two populations. We concede that these methods are more appropriate to a large scale analysis of many geographically contiguous populations, and this is in fact the aim of the study team moving forward with subsequent work. We are presenting here a smaller analysis of just two centres to demonstrate how the relative effect of these variables changes in two different centres and to inform the next study which is in preparation. We have added to the discussion section to explain this for further clarity to readers.

Discussion: “However, given the policy imperative of providing treatment for those most in need equitably, we conclude that a much larger study investigating more demographic variables and in a larger study population, but with a broadly similar methodological approach, could usefully inform service development.”

Reviewer 3; Comment 2: The over-riding concern that I have is that we already know that much fewer than 1% of patients eligible under NICE guidance receive surgery, so there is little likelihood that a sample of 1200 patients is representative of the population who could receive surgery. Looking for patterns of variation within an at best 1% sample seems prone to substantial error, and undermine the power of the comparisons between the two centres. Furthermore, there is already ample evidence for a postcode lottery driven by different decisions on eligibility and volume between commissioning bodies.

The reviewer is absolutely correct in identifying the extreme variance between eligibility and treatment. The assertion that patients that eventually receive treatment are highly unlikely to be representative of the eligible population is probably correct, although difficult to prove conclusively. However there is still much utility in studying them as we can make some inferences about what is driving demand and supply in different populations using these methods. As in our point above, this utility will not be fully realised until we are able to extend the analysis to a much larger area.

Reviewer 3; Comment 3: What was the incidence of surgery done in the private sector (data that might be available from the NSBR) and could this bias the SE distribution in the NHS service (issue raised by previous reviewer)? How long did patients wait between referral and surgery – this could undermine the attribution of epoch to any individual patient.

Thank you for this important question. We acknowledge the point about private sector treatment affecting the numerator of treated patients. We cannot obtain accurate data on private sector treatment in our populations as the completeness of data on private patients cannot be ascertained even from the NBSR. Furthermore, the NBSR does not collect any identifiers of residential location such as postcode. Therefore, we have added a sentence in the strengths and weaknesses section of the Discussion to explicitly acknowledge this issue.

Discussion: “Furthermore, the role of the private sector in either of the two studies was not investigated due to a lack of reliable data. Indeed, data sources such as the National Bariatric Surgery

Registry (NBSR) do not collect any data pertaining to residential location, therefore locally resident patients who received bariatric surgery privately are not able to be identified. The completeness of the remaining data in such registries for private patients is also not guaranteed.”

Reviewer 3; Comment 4: Was the presence of co-morbidity (particularly T2D) similar in the two samples? One could envision that a difference in this could relate both to SE status, ethnicity and surgical ‘prioritisation’.

Thank you for raising this issue which we have addressed in our response to Reviewer 2, Comment 3.

Reviewer 3; Comment 4: Can the authors give data on the total number of referrals to each unit during the time periods? This would be valuable in showing the problem was in referral rather than unit activity and or selection. Indeed, if these data exist they might be a fairer way of exploring the impact of SE status on access to surgery than starting with those who received it.

We thank the reviewer for commenting on this important limitation of our study. We have addressed this issue as part of our response to Reviewer 1, Comment 4.

Reviewer 3; Comment 5: Page 5, L5: I am not sure that WHO global estimates are germane to this paper.

Thank you for your comment. WHO estimates were used to give some wider context to the obesity problem globally as an introduction to the issues investigated by our study. We were careful to include UK estimates, which were more relevant to the study. To add to this introduction with more specific context, we have included additional information focussing on the UK.

Introduction: “In the United Kingdom, the prevalence of obesity has increased from approximately 15% in 1993 to 27% in 2015.”

Reviewer 3; Comment 6: Page 5, L39: This sentence doesn’t make sense

Thank you for your comment, we agree that this sentence is not quite clear. We have amended the text as below.

Introduction: “Similar findings were reported from two earlier American studies which concluded that a significant proportion of those who would benefit from bariatric surgery are less likely to receive it due to socioeconomic deprivation.”

Reviewer 3; Comment 7: Page 5, L45: Provision or uptake?

Many thanks to the reviewer for noticing this error which we have now corrected.

Introduction: “However, research on populations with free universal healthcare also suggest that there are disparities in the uptake of bariatric surgery with regard to socioeconomic status.”

Reviewer 3; Comment 8: Page 5, L56: Is this so? Funding bodies may dictate the volume they commission, but are surely not involved in individual clinical decisions?

Thank you for your question. We felt that we were not implying that funders had a role in selecting individuals for treatment, however local funding policies impose many constraints on the volume of patients treated and this was the point that we were attempting to make in this sentence. We do agree

however that this sentence in isolation may be misleading and have therefore changed it as below.

Introduction: "Decisions regarding the provision of specialist weight management services to obese patients lie with local healthcare funding bodies."

Reviewer 3; Comment 9: Page 6, L6: It has, not may! See the variation in care maps from D of H.

Thank you for this comment. We have amended this sentence as per the reviewer's suggestion.

Introduction: "This has led to the emergence of a 'postcode lottery' in bariatric surgery, in which the likelihood that an eligible patient will receive surgery depends on their location of residence and not just their clinical need or the fulfilment of criteria."

Reviewer 3; Comment 10: Page 7, L 27: But are commissioning and eligibility guidelines and contracts the same between the two centres? See page 12 discussion.

Thank you for this question. We were satisfied that both centres treat to guideline and that eligibility criteria are the same. However we do recognise that a degree of variation in clinical decision making will exist between centres, which has been highlighted in the 'strengths and weaknesses' section of the Discussion accordingly.

Reviewer 3; Comment 11: Page 9, L13: Statistically but not clinically significant.

Thank you to the reviewer for identifying this error which we have now corrected. The reviewer is correct to point out that the difference between clinical and statistical significance needs to be clearly communicated we have amended this sentence to reflect this.

Introduction: "Figure 1 summarises the demographics of the patients included in the study. The age distribution of patients treated in Centre 1 was significantly lower than in Centre 2 (median age 45 years vs. 46 years, $p=0.01$; Wilcoxon rank test)."

VERSION 3 - REVIEW

REVIEWER	Chun-Che Huang National Taiwan University, Taipei, Taiwan
REVIEW RETURNED	11-Jun-2017

GENERAL COMMENTS	I am happy with the revision and have no further comments on this manuscript.
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