PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (**http://bmjopen.bmj.com/site/about/resources/checklist.pdf**) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Persistent spatial clusters of high body mass index in a Swiss urban population as revealed by the 5-year GeoCoLaus longitudinal study
AUTHORS	Joost, Stéphane; Duruz, Solange; Marques-Vidal, Pedro; Bochud, Murielle; Stringhini, Silvia; Paccaud, Fred; Gaspoz, Jean-Michel; Theler, Jean-Marc; Chételat, Joël; Waeber, Gerard; Vollenweider, Peter; Guessous, Idris

VERSION 1 - REVIEW

REVIEWER	Bing Zhang National Institute for Nutrition and Health, China CDC P. R. of China
REVIEW RETURNED	21-Oct-2015

GENERAL COMMENTS	This manuscript is very interesting in spatial cluster of adults BMI. Main question is: 1.It missed some factors such as occuption, individual income and family income, health insurrance in individual level. no data for control and analysis. 2. the factor represented neighborhood level is very limited, only income. as we know, community environment plays an important role in obesity, including lot of factors, restaurant, food store, supermarket and etc. also road, park and exercise place will affect individual energy balance. I did not see those aspect in this study. 3. We can have an image that high and low of BMI is different distribution in a city through the current study.

REVIEWER	Colin Rehm Montefiore Medical Center New York, NY United States
REVIEW RETURNED	01-Nov-2015

GENERAL COMMENTS	This paper evaluated spatial clustering of high BMI and change in BMI in a cohort of Swiss adults living in Lausanne. The authors used the Getis---Ord Gi statistic to evaluate clustering of high BMI and incident obesity, which is a local measure of spatial autocorrelation/dependency (I use these terms loosely throughout the review, understanding that there are technical distinctions between them). The most important contribution of this work is the presentation of data on incidence obesity, which has not been frequently examined in an explicitly spatial context. My comments are separated into major issues, minor issues/clarifications and recommended text edits. My text

	edits/grammatical editing do not cover 100% of necessary changes. <u>Major issues</u> The authors rely exclusively on the G_i statistic for their primary analysis (though they did use the local Moran's I statistic in supplemental analysis), which is a reasonable measure of local spatial autocorrelation. Throughout the paper the authors employ qualitative descriptions to describe the changes in spatial autocorrelation before and after adjusting for socio---demographic variables, for example. Given the statistical rigor with which the authors evaluated local spatial relationships I would recommend employing a global measure of spatial autocorrelation to quantify the extent by which adjustment for these socio---demographic factors "attenuated" or "reduced" the degree of spatial dependence. For example, in both Figures 1 and 2 the authors demonstrate substantial attenuation of the observed clustering of low BMI (less so for high BMI) after adjusting for median income. It would be very informative to add a statement to the effect of, "In unadjusted analyses the global Moran's I was 0.68 for BMI at baseline. After adjusting for median income, the Moran's I statistic was 0.42, suggesting that some, but not all of the spatial dependence observed for BMI is due to this single area---based socio---demographic variable". I am not suggesting that the authors adopt a global over local perspective, but the addition of a global perspective will strengthen the paper and its contribution considerably. The authors could present global Moran's I after adjustment for the set of covariates described in line 239. The authors note in the limitations section that they "preferred the Getis---Ord G_i statistic to other statistics like Moran's I as our interest focused primary on the detection of local clusters of high and low BMI values." This is fine, but there is no law against presenting both. Particularly, if the authors would like to strengthen their claims RE: attenuation or lack of attenuation upon covariate adjustment, I can think of no other alternative. The definition of incident obesity is problematic, particularly the use of an increase of 0.5 BMI units amongst the overweight participants. The authors provide no reference or discussion justifying this decision. If the authors wish to keep this as part of their outcome definition much more discussion is warranted. Primary analyses should focus on the real definition of incident obesity, or conversely another measure of weight gain (e.g., 5% weight gain or gaining 10 kg). On line 297 the authors mention "excess weight" as part of their definition. If this is the case, please define in a more biologically meaningful manner. For example, an individual

	who has BMI of 25.6 and goes up to 26.2 would be considered to have experienced excess weight gain, but an individual with a BMI of 24 going up to 27 would not, even though this later experience is potentially more harmful, all things being equal. The sentence, “Incident obesity was found in 1,545 adults (395 with new obesity)…” does not make sense. By definition only new obesity should be included in the definition of incident obesity. Individuals who are already obese are not at risk of becoming obese. As an aside, the authors will also want to note that measurements were only made twice, meaning that some people who may have become obese and subsequently lost a bit of weight would not be included. This is a source of outcome measurement error, and should be briefly noted. <u>Minor issues/clarifications</u> If permitted by journal, combining Design, Settings and Participants would make for a less choppy and more readable abstract. On page 5 you mention that this study was focused on the Caucasian population, but on page 8 you mention that non---Caucasians were included. However, this covariate is not included in the Supplemental. Please note the average population of the statistical sectors to place in context for readers familiar with other geographic aggregations (page 8, line 144). It would be helpful to note whether individuals residing in the same household could participate in the study. If the number is high, the authors may want to account for this in their discussion, if it is low, it would be helpful to note the percent. The authors present numerous secondary/sensitivity analyses. One sensitivity analysis the authors may want to consider is using a more conservative alpha---level. For example, how much does the primary figure vary if using an alpha---level of 0.01 instead of 0.05. I can see one critique being the potential for a multiple testing problem. If the authors employed some sort of false discovery correction please mention. If not, please comment in the limitations. Please clarify whether postal address is always or sometimes the place of residence at some point in the paper. This term has different meanings in different countries, for example, in the rural US the postal address will rarely represent the residential

	address. If the postal address does often identify a place other than the place of residence, please note as a limitation. Page 9, line 182; please specify alternative models, do you mean models with different covariates? If so, please provide some examples. Also examples of the different spatial models would be welcome. The value of the local Moran's I analysis in addition to the G statistics does not add very much. I would think a global measure would be more useful. Throughout the Results section please avoid using technical terms to describe (e.g., describe what G statistics means rather than say what the G statistic value was). Give example of how attenuation was evaluated (qualitatively or quanitively?), page 11, line 219. "Fairly stable", page 12, line 41. Please expand if possible, suggests that they were not stable. Next line, how exactly was "increasing" of clusters determined? Please expand on east---to---west pattern? At what scale was this observed in Canada? If at the provincial level this adds little information and seems coincidental, if in a similar sized city why would this be relevant. I am not saying I think it is unimportant, but more detail needs to be provided. On page 14, line 285 the authors mention population density, but their analysis makes not use of built environment measures. Could population density explain some of the residual spatial dependence in obesity? Page 15, and throughout the paper; residual confounding by unmeasured/difficult to measure socioeconomic factors is an issue throughout the paper as the authors acknowledge. However, in their discussion the authors discuss many potential variables that could be incorporated to reduce this concern, specifically population density and living in city---owned/subsidized housing. The confidence with which the authors state that the use of individually geo---referenced data detect different patterns that would not have been identified with aggregate data is not justified (unless the authors know something not presented). Would suggest simply stating the advantage of non---aggregated data rather than making this strong statement. In many places, use of aggregated data is the only option. Figure 1, please interpret the change in the low clusters in the southeast part of the city before and after adjusting for income.

	This can be added the footnote and is essential for readers not familiar with this methodology. <u>Text edits</u> Page 8, line 41: “GeoDa” not “Geoda”. The discussion of “high class” and “low class” clusters is somewhat awkward, as it makes the reader think about social class, please consider revising. “Gini coefficient” not “coefficients” Line 348, add “the” between “as” and “food”. Line 358, this is actually only true for the baseline analysis; the follow---up analysis made use of arbitrary BMI categories. Line 358, please describe why this approach was preferred. As noted above, I have major concerns regarding the reliance of local measures.

VERSION 1 – AUTHOR RESPONSE

Reviewer #1: Dr Bing Zhang
Institution and Country: National Institute for Nutrition and Health, China CDC P. R. of China.

This manuscript is very interesting in spatial cluster of adults BMI.

Main question is:
1. It missed some factors such as occuption, individual income and family income, health insurrance in individual level.

Answer:
Indeed but the variables mentioned (profession, individual income, family income, health insurance) were not available for this analysis. We acknowledged this in the limitation section (page 18, line 394-398). As regards individual information related to the professional activity, self-reported information on education in 5 classes was used (see lines 126-127 of the initial manuscript).

Text modification (page 18, lines 395-399):
“We considered several individual-level covariates but other data such as individual income were not available and population density not accounted for; thus, residual confounding cannot be excluded.”

2. no data for control and analysis.

Answer:
The results of analyses measuring spatial autocorrelation and spatial dependence cannot be validated by a set of control data. This would require in our case an independent second cohort of a large number of individuals living in the same municipality. It is not possible to reproduce such spatial patterns on a small number of individuals. Indeed, density matters and to guarantee spatial representativeness we would have to apply different spatial lags.
In fact, the way this type of analysis is validated is by means of many Monte-Carlo random

permutations of BMI values among participants, their geographic position remaining fix (see our original explanations in the supplementary material, lines 84-91).

3. The factor represented neighborhood level is very limited, only income. as we know, community environment plays an important role in obesity, including lot of factors, restaurant, food store, supermarket and etc. also road, park and exercise place will affect individual energy balance. I did not see those aspect in this study.

Answer:
We agree with Dr Bing Zhang that in general a correlation is expected between BMI spatial clusters of adults and many components of the community environment (restaurants, food store, supermarkets) constituting neighborhood-related risk factors of obesity (self-reported information on physical activity was used in this study, see line 129 of the initial manuscript). But among these risk factors, income level is thought to be of major importance (Guessous et al. 2014) while other associations between other community environmental attributes and obesity have been inconsistent. Thus, as a first step, we deliberately decided to focus on neighborhood median income.
Accordingly, a sentence was added in the introduction to clearly mention that income is not the only neighborhood-level variable of importance (see page 4, lines 87-88 of the new manuscript).

Text modification (page 4, lines 87-89):
"Although a number of neighborhood-related risk factors of obesity exist, income level is thought to be of major importance (Guessous et al. 2014) while associations between other community environmental attributes and obesity have been inconsistent."

Reviewer #2: Dr Colin Rehm
Institution and Country: Montefiore Medical Center, New York, NY, United States

This paper evaluated spatial clustering of high BMI and change in BMI in a cohort of Swiss adults living in Lausanne. The authors used the Getis-Ord Gi statistic to evaluate clustering of high BMI and incident obesity, which is a local measure of spatial autocorrelation/dependency (I use these terms loosely throughout the review, understanding that there are technical distinctions between them). The most important contribution of this work is the presentation of data on incidence obesity, which has not been frequently examined in an explicitly spatial context. My comments are separated into major issues, minor issues/clarifications and recommended text edits. My text edits/grammatical editing do not cover 100% of necessary changes.

Major issues
1. The authors rely exclusively on the Gi statistic for their primary analysis (though they did use the local Moran's I statistic in supplemental analysis), which is a reasonable measure of local spatial autocorrelation. Throughout the paper the authors employ qualitative descriptions to describe the changes in spatial autocorrelation before and after adjusting for socio-demographic variables, for example. Given the statistical rigor with which the authors evaluated local spatial relationships I would recommend employing a global measure of spatial autocorrelation to quantify the extent by which adjustment for these socio-demographic factors "attenuated" or "reduced" the degree of spatial dependence. For example, in both Figures 1 and 2 the authors demonstrate substantial attenuation of the observed clustering of low BMI (less so for high BMI) after adjusting for median income. It would be very informative to add a statement to the effect of, "In unadjusted analyses the global Moran's I was 0.68 for BMI at baseline. After adjusting for median income, the Moran's I statistic was 0.42, suggesting that some, but not all of the spatial dependence observed for BMI is due to this single area-based socio- demographic variable". I am not suggesting that the authors adopt a global over local perspective, but the addition of a global perspective will strengthen the paper and its contribution

considerably. The authors could present global Moran's I after adjustment for the set of covariates described in line 239.

Answer:
In the figures, we also provided quantitative information to describe the changes in spatial autocorrelation before and after adjusting for socio-demographic variables with the number of individuals belonging to each cluster class (hot and cold spots). We decided to add this quantitative information in the text (see page 10, lines 228 and 231, and page 11, lines 248 and 252 of the new manuscript).

Text modification (page 10, lines 228-231):
"…BMI cluster areas (14.9% instead of 18.9% of individuals). Attenuation was important in the North (district 14), but did not affect the same category of cluster in the West (districts 2—4). The adjustment attenuated low BMI cluster areas (18.4% instead of 35.8% of individuals) in particular in the East (districts 7—9 and 11)…"

Text modification (page 11, lines 248-252):
"The adjustment for neighborhood-level income globally attenuated the high BMI cluster areas (10% instead of 16.7% of individuals). These hot spots persisted in the West (districts 2—4, 17), the high cluster in the North disappeared (14), while part of the cold spots in the East were attenuated, especially in districts 6—9 and 11 (for a global decrease from 32% to 12.7%, see Figure 2b)."

We initially did not provide the global Moran's I as we expected it to show a value close to zero. Indeed, analyses carried out in Geneva (Guessous et al. 2014) already showed that the spatial dependence of BMI values in urban areas is very local and that no signal is likely to be perceived at the scale of the whole city. Indeed in the city of Lausanne, global Moran's I of the different BMI variables calculated is as follows:

- Raw BMI baseline I=0.011
- Adj BMI baseline I=0.0044
- Raw BMI follow-up I=0.0094
- Adj BMI follow-up I=0.0031

But Dr Rehm is right and it is useful to transmit this information. Following the suggestion, we added a sentence on global Moran's I to strengthen the paper as it permits to emphasize the local regime of the spatial dependence in BMI values. See lines 236-240, and 256-259 of the new manuscript.

Text modification (page 11, lines 236-240):
"In unadjusted analyses the global Moran's I (see supplementary material) was 0.011, and 0.0044 after adjusting for neighborhood-level median income, which is close to spatial independence in both cases. These significant values of Moran's I (p=0.01) show a decrease of global spatial autocorrelation between the two situations but above all highlight the local regime of spatial dependence in the distribution of BMI values."

Text modification (page 12, lines 257-260):
"At follow-up, the global Moran's I was 0.0094, and 0.0031 after adjusting for neighborhood-level median income. These values show a decrease of global spatial autocorrelation in the adjusted case, but here again highlighting a behavior close to spatial independence in the two situations."

2. The authors note in the limitations section that they "preferred the Getis-Ord Gi statistic to other statistics like Moran's I as our interest focused primary on the detection of local clusters of high and low BMI values." This is fine, but there is no law against presenting both. Particularly, if the authors

would like to strengthen their claims RE: attenuation or lack of attenuation upon covariate adjustment, I can think of no other alternative.

Answer:
We agree and added the value of the global Moran's I (see our answer to question 1 raised by Dr Rehm) to complete the information on the spatial behavior of BMI values in the city of Lausanne.

3. The definition of incident obesity is problematic, particularly the use of an increase of 0.5 BMI units amongst the overweight participants. The authors provide no reference or discussion justifying this decision. If the authors wish to keep this as part of their outcome definition much more discussion is warranted. Primary analyses should focus on the real definition of incident obesity, or conversely another measure of weight gain (e.g., 5% weight gain or gaining 10 kg). On line 297 the authors mention "excess weight" as part of their definition. If this is the case, please define in a more biologically meaningful manner. For example, an individual who has BMI of 25.6 and goes up to 26.2 would be considered to have experienced excess weight gain, but an individual with a BMI of 24 going up to 27 would not, even though this later experience is potentially more harmful, all things being equal.

Answer:
Following the recommendation of Dr Rehm, we chose to focus our analysis on a clear definition of weight gain, i.e. 5% of BMI increase. See lines 156-158 of the new manuscript.

Text modification (page 7, lines 156-158):
"Spatial dependence of BMI among participants with weight gain" (subtitle) and then "We then explored the spatial dependence of follow-up BMI among participants showing a BMI increase ≥ 5% between baseline and follow-up as used elsewhere".

With a reference to "Hart V, Reeves KW, Sturgeon SR, et al. The Effect of Change in Body Mass Index on Volumetric Measures of Mammographic Density. Cancer Epidemiol Biomarkers Prev, 2015;24:1724–30. doi:10.1158/1055-9965.EPI-15-0330"

Text modification (page 15, lines 327-335):
"Our longitudinal data also enabled the mapping of weight gain (≥5% of BMI increase), which appeared to be spatially scattered all over the city. In addition, we found that even among participants who gained weight, clusters of high and low BMI could be identified, and that these clusters corresponded – albeit less pronounced – to BMI clustering found among all participants. This is the first study to explore and report such correspondence of BMI clustering. This result suggests that the spatial clustering of high and low BMI observed in the general adult population remains identical among the limited group of persons having gained weight between baseline and follow-up: individuals gained more weight where high BMI clusters were observed among all participants."

4. The sentence, "Incident obesity was found in 1,545 adults (395 with new obesity)…" does not make sense. By definition only new obesity should be included in the definition of incident obesity. Individuals who are already obese are not at risk of becoming obese.

Answer:
Correct, see answer to question 3.

5. As an aside, the authors will also want to note that measurements were only made twice, meaning that some people who may have become obese and subsequently lost a bit of weight would not be included. This is a source of outcome measurement error, and should be briefly noted.

Answer:
Correct. The new definition used (5% of BMI-unit gain) does not expose us to such a configuration.

Minor issues/clarifications
1. If permitted by journal, combining Design, Settings and Participants would make for a less choppy and more readable abstract.

Answer:
We agree with the reviewer and redirect the remark to BMJ open.

2. On page 5 you mention that this study was focused on the Caucasian population, but on page 8 you mention that non-Caucasians were included. However, this covariate is not included in the Supplemental.

Answer:
Ethnicity (Caucasian vs. non-Caucasian) was included in the models (see lines 127-128 of the original manuscript and Table S1).

3. Please note the average population of the statistical sectors to place in context for readers familiar with other geographic aggregations (page 8, line 144).

Answer:
The average population per statistical sector is 1,687. We added this information on page 7, lines 146-147 of the new manuscript.

Text modification (page 7, lines 146-147):
"(average population of the statistical sectors is 1,687)"

4. It would be helpful to note whether individuals residing in the same household could participate in the study. If the number is high, the authors may want to account for this in their discussion, if it is low, it would be helpful to note the percent.

Answer:
Individuals residing in the same household could participate in the study. Although this scenario is very unlikely it cannot be excluded as 23% of addresses among the 6,481 participants are not unique. The names of participants are not available and there is no other mean to quantify this risk.
We added this limitation to our study on pages 17-18, lines 395-396: "(for instance, individuals residing in the same household could have participated in the study)".

5. The authors present numerous secondary/sensitivity analyses. One sensitivity analysis the authors may want to consider is using a more conservative alpha-level. For example, how much does the primary figure vary if using an alpha-level of 0.01 instead of 0.05. I can see one critique being the potential for a multiple testing problem. If the authors employed some sort of false discovery correction please mention. If not, please comment in the limitations.

Answer:
The alpha level is corrected for multiple comparisons with the Bonferroni/Sidak procedure described in Anselin (1995). This information was added in the new manuscript on line 172. We also added a map in the supplementary material (see Figure S2) showing the local significance using different alpha-levels for raw BMI at baseline (see line 173-174 of the new manuscript). This maps gives an idea of how much the primary figure may vary according to different alpha-levels.

6. Please clarify whether postal address is always or sometimes the place of residence at some point in the paper. This term has different meanings in different countries, for example, in the rural US the postal address will rarely represent the residential address. If the postal address does often identify a place other than the place of residence, please note as a limitation.

Answer:
The postal address is always the place of residence. To be as clear as possible, we substituted “house” with “place of residence” on line 42 of the new manuscript, and specified “place of residence” where necessary (lines 148, 162), or substituted “postal address with “place of residence” or “residence” (lines 179, 184, 198, 282, 406 and in the supplementary material).

7. Page 9, line 182; please specify alternative models, do you mean models with different covariates? If so, please provide some examples. Also examples of the different spatial models would be welcome.

Answer:
Yes we mean models with different covariates. On line 185 of the new manuscript, we substituted the sentence “c) analyses using different adjustment models” with “c) analyses implementing BMI adjustment with different covariates (e.g. education level and median income; education level only; all socio-economic variables, etc.)”, and we also provided the list of the different spatial lags used on line 187 of the new manuscript.

8. The value of the local Moran’s I analysis in addition to the G statistics does not add very much. I would think a global measure would be more useful.

Answer:
See answer to question 1.

9. Throughout the Results section please avoid using technical terms to describe (e.g., describe what G statistics means rather than say what the G statistic value was).

Answer:
At the beginning of the result’s sections on both spatial dependence of BMI and spatial dependence on BMI increase, we now explain the meaning of a “hot spot” versus “cold spot”: e.g. on line 218 of the new manuscript, we wrote “2,935 (45.3%) individuals presented no BMI spatial dependence, 1,224 (18.9%) belonged to spatial clusters where individuals locally showed a BMI proportionally higher than within the whole study area (high BMI cluster class or hot spots); 2,322 (35.8%) belonged to spatial clusters where individuals locally showed a BMI proportionally lower than within the whole study area (low BMI cluster class or cold spots)”.
We did the same on line 265 for BMI increase.

10. Give example of how attenuation was evaluated (qualitatively or quantitatively?), page 11, line 219.

Answer:
See answer to Dr Rehm’s question 1.

11. “Fairly stable”, page 12, line 41. Please expand if possible, suggests that they were not stable. Next line, how exactly was “increasing” of clusters determined?

Answer:

Upon suggestion of Dr Rehm, we now provide analyses using weight gain defined as ≥5% BMI gain and have rewritten this section (see hereunder). The increase or decrease of clusters is expressed in terms of change in the number of individuals they are constituted of.

Text modification (pages 12-13, lines 262-280):
"Spatial dependence of BMI among participants showing weight gain" (subtitle)
"Weight gain (≥5% of BMI increase between baseline and follow-up) was found in 1,351 adults (max BMI increase=35.6%, mean=9.73, median=8.24; 59% women, mean age 50.76±10.1) and was spatially scattered all over the city. Among these adults, 1,109 (82.1%) individuals presented no spatial dependence in raw BMI, 107 (7.9%) belonged to spatial clusters where individuals locally showed a BMI increase proportionally higher than within the whole study area (hot spot), and 135 (10%) belonged to spatial clusters where individuals locally showed a BMI increase proportionally lower than within the whole study area (cold spot) (Figure 3, panel a). Hot spots were distributed in the West (districts 2—4 and 16) and in the center to a lesser extent (districts 1 and 10), whereas cold spots were distributed mainly in the East (districts 8, 9 and 11), with a central spot too (districts 1, 3 and 5). Adjustment for neighborhood-level income (Figure 3, panel b) did not change the general spatial pattern described above, but altered the intensity of spatial dependence. Indeed, 105 (instead of 107) individuals constituted stable hot spots in the West, while 162 (instead of 135) constituted cold spots. The latter are concentrated in the central part of districts 1, 2, 3, and 15. In the eastern part of the city (districts 8, 9 and 11), median income neutralized 72% (61/84) of the cold spots. Finally, adjusting for individual-level characteristics (supplementary Figure S7) globally neutralizes the local BMI clusters mentioned above but led to the emergence of a cold spot in the North (57 individuals)."

12. Please expand on east-to-west pattern? At what scale was this observed in Canada? If at the provincial level this adds little information and seems coincidental, if in a similar sized city why would this be relevant. I am not saying I think it is unimportant, but more detail needs to be provided.

Answer:
We agree that the comparison with data collected at the provincial level in Canada added little information and we have deleted this sentence.

13. On page 14, line 285 the authors mention population density, but their analysis makes not use of built environment measures. Could population density explain some of the residual spatial dependence in obesity?

Answer:
We agree that population density could explain some of the residual spatial dependence in obesity, and this is also the case of other explanatory factors. To clarify the selection of analyses we decided to implement, we now acknowledge this limit as follows:

Text modification (page 18, lines 396-399):
"We considered several individual-level covariates but other data such as individual income were not available and population density not accounted for, thus residual confounding cannot be excluded."

14. Page 15, and throughout the paper; residual confounding by unmeasured/difficult to measure socioeconomic factors is an issue throughout the paper as the authors acknowledge. However, in their discussion the authors discuss many potential variables that could be incorporated to reduce this concern, specifically population density and living in city-owned/subsidized housing.

Answer:
See answer to question 13.

15. The confidence with which the authors state that the use of individually geo-referenced data detect different patterns that would not have been identified with aggregate data is not justified (unless the authors know something not presented). Would suggest simply stating the advantage of non-aggregated data rather than making this strong statement. In many places, use of aggregated data is the only option.

Answer:
We have rephrased this sentence as follows. Text modification (page 17, lines 373-374):
“Our spatial approach allowed us to detect different patterns within statistical sectors that may have not been identified based on aggregated data.”
The rest of the paragraph refers to spatial approaches in general.

16. Figure 1, please interpret the change in the low clusters in the southeast part of the city before and after adjusting for income. This can be added the footnote and is essential for readers not familiar with this methodology.

Answer:
Given that such interpretations are given in the main body of the manuscript and because footnotes are already long, we suggest not to add interpretations in the footnote of the figures.

Text edits

17. Page 8, line 41: “GeoDa” not “Geoda”.

Answer:
Changed accordingly, line 163 of the new manuscript.

18. The discussion of “high class” and “low class” clusters is somewhat awkward, as it makes the reader think about social class, please consider revising.

Answer:
We agree and substituted “high class” and “low class” with “hot spots” and “cold spots” respectively, fitting the wording used by Ord and Getis (1995). See lines 176-177 and 220-222 of the new manuscript.

19. “Gini coefficient” not “coefficients”

Answer:
Changed accordingly, line 296 of the new manuscript.

20. Line 348, add “the” between “as” and “food”.

Answer:
Changed accordingly, line 381 of the new manuscript.

21. Line 358, this is actually only true for the baseline analysis; the follow-up analysis made use of arbitrary BMI categories.

Answer:
We agree and have deleted this sentence.

22. Line 358, please describe why this approach was preferred. As noted above, I have major

concerns regarding the reliance of local measures.

Answer:
See our answers to questions 1 and 2.

VERSION 2 – REVIEW

REVIEWER	Bing Zhang National Institute for Nutrition and Health, China CDC
REVIEW RETURNED	24-Nov-2015

GENERAL COMMENTS	It already anwser my question.

REVIEWER	Colin D Rehm Montefiore Medical Center, Bronx, NY USA
REVIEW RETURNED	04-Dec-2015

GENERAL COMMENTS	The authors did a nice job revising this paper. The only remaining comment I have is that when interpreting the Moran's I values that are very close to zero the authors might consider interpreting any value <0.05 as practically equivalent to zero. A Moran's of 0.01 vs. 0.008 is not a particularly meaningful measure of global spatial autocorrelation regardless of whatever p-value is observed.