PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Overweight and Obesity on the Island of Ireland: An Estimation of Costs
AUTHORS	Dee, Anne; Callnan, Aoife; Doherty, Edel; O'Neill, Ciaran; McVeigh, Treasa; Sweeney, Mary; Staines, Anthony; Kearns, Karen; Fitzgerald, Sarah; sharp, Linda; Kee, Frank; Hughes, John; Balanda, Kevin; Perry, Ivan

VERSION 1 - REVIEW

REVIEWER	Marie Kruse University of Southern Denmark Denmark
REVIEW RETURNED	29-Aug-2014

GENERAL COMMENTS	This is a traditional cost-of-illness/burden of disease study that provides some useful figures for the societal costs of overweight and obesity in two jurisdictions: the republic of Ireland and Northern Ireland. The main justification of this study is the very large cost figures, pointing to the urgency of political action towards the incidence (and prevalence?) of obesity in the two jurisdictions. The authors explicitly state in the introduction, that burden of illness estimates can provide useful information for priority setting, health technology assessment, etc. The overall challenge of the paper is that it doesn't quite provide any estimates that are useful for e.g. health technology assessments. A HTA would generally include an economic evaluation, where cost estimates and effect measures (e.g. QALY's or LYG) for two interventions are compared, enabling decision makers to choose the right intervention against obesity. If this study had produced figures for the marginal costs of a new case of obesity, it could be applied in the decision making process. Such a figure should compare the cost of a new case of obesity with the cost of a normal-weight person. And it should be incidence based. If one case of obesity is prevented, the society will save $X. Or: if one obese person becomes normal-weight (and remains so), the society will save $Y. The overall critique of this study is that it provides figures that are not useful for the decision making process because they are not marginal (per person), not attributable (i.e. costs of normal weight people are not subtracted), and they are not incidence based. It may very well be, that 1.1 and 0.5 billion EURO are numbers that are useful in a political debate, however beyond that debate there's not much you can do with it. I suggest that the authors produce a whole new set of results, per person and distinguishing between incidence and prevalence. They should also distinguish between overweight and obesity because several studies find that overweight does not cause excess health problems compared to normal-weight. They should also update their

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	terminology to current uses in health economics. I.e. ‘health care costs’ instead of ‘direct costs’, and ‘productivity costs’ instead of ‘indirect costs’ or ‘productivity loss costs’. My final suggestion in this round would be that the authors are more critical towards their own use of top-down estimates. Such estimates are average and not marginal and therefore (and for other reasons) often over-estimates.

REVIEWER	David Madden School of Economics, University College Dublin, Ireland.
REVIEW RETURNED	25-Nov-2014

GENERAL COMMENTS	My assessment on the paper is minor/major revision. I confess I don't really like these "cost of illness" type study, I feel they suffer from methodological weaknesses which I outline in my comments. However, what the authors do is standard enough for this type of exercise. But I would at the least like to see them acknowledge or discuss the issues I raise in my report. I have a number of general comments, followed by some specific comments. The first general comment is: why an all-Ireland approach? My guess is because this was the condition laid down by the funder, but in terms of the academic merits of the paper it makes little sense. While the demographic and cultural profiles of the two jurisdictions are likely to be similar, the method of healthcare delivery is not. So this complicates comparison (a point which is acknowledged but not really developed in the text). If the authors really wanted to exploit the North-South differences then surely a preferable approach would be to look at differences in various measures of healthcare costs which they assign to obesity, and then decompose these differences. So, it should be possible to see what element of the difference arises because of scale of population, what element arises owing to different prevalence and PAFs, and what element arises owing to actual prices of procedures or drugs. This would enable the construction of a counter-factual along the lines of: what would be the obesity related cost of (for example) stroke in Northern Ireland if it has the same demographic and prevalence profile as the Republic. Of course, this does ignore an element of endogeneity in that the mode of healthcare delivery may well influence prevalence of disease, but I feel it would go some way towards exploiting the North-South dimension in a way that the current paper does not. While I am not insisting that such analysis should be included in this paper, it gives some idea of what form of analysis a North-South comparison might take. As it stands, the comparative analysis is imposed on the data without really doing anything. The authors should consider dropping it, or at least finding some way of exploiting it properly. The second main point I wish to make concerns the type of counter-factual implied in the analysis (and indeed implied in all analysis of this nature). The exercise is designed to arrive at some estimate of the costs of obesity in Ireland. So presumably the authors are trying to discover how much lower healthcare costs would be in the absence of obesity. And they do this by calculating risk ratios for various conditions for those with obesity compared to those without (are these raw risk ratios, or are other covariates controlled for? – this is not entirely clear). And then they calculate PAFs etc. I think this approach would be fine if obesity was randomly allocated across

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the population. But my guess is that obesity is not randomly allocated across the population and that there are unobserved factors associated with obesity which are also associated with the various obesity-related conditions listed here. It also seems more plausible to me that these unobserved factors which are correlated with obesity will also be correlated with these other conditions. Thus if we were to wave a magic wand and eliminate obesity, we would still observe higher RRs for the formerly-obese, higher RRs which arise owing to these unobserved factors. All of which implies that the costs presented here as arising from obesity are probably overestimates of the costs arising from obesity per se.

Endogeneity also raises its head in another of the costs which are estimated, that of lost productivity. The authors cost the lost hours arising from obesity related conditions at the average wage costs adjusted for age and gender. But it seems plausible that once again, those unobserved factors associated with obesity might also affect wages, so that wages for the obese may not be the same as those for the non-obese. Interestingly, evidence seems to suggest that the effect can work either way, though the balance of evidence seems to indicate a wage penalty.

Having raised these issues, is there any way in which this endogeneity can be addressed? In truth there is no easy way out of this, and I am not suggesting that the authors need to adopt elaborate statistical approaches to try to correct for this endogeneity, especially as such approaches, with non-experimental data, are of dubious efficacy. But I would like to see a discussion and acknowledgement of the issue in the paper.

The authors indicate that they are taking a societal perspective. I am not quite sure what they mean by this, but I infer that they are looking at social and not private costs. Thus if there was a health insurance premium associated with obesity then this would constitute a private and not a social cost. They seem to be implying also that all productivity losses are borne by the firm and/or other workers. This may well be the case in many workplaces, but there are also situations where it will not be the case. If I am self-employed, or I am employee working on a piece rate, then if I am less productive owing to an obesity related condition, this is a private, not a social cost. But the authors do not consider this at all. Perhaps in some of the data available they may have information on the self-employed, in which case perhaps some adjustment might be needed to the costs associated with productivity losses.

Amongst the costs which the authors include are losses owing to premature mortality. I note that these costs seem to be calculated via what could be called the human capital approach, rather than a valuation of a statistical life (VOSL) approach. I guess this is consistent with only including social costs though my own preference would be for a VOSL approach or at least some acknowledgement that if someone dies prematurely there are costs to others in society, even if is only close friends or relatives.

However, going back once again to the counterfactual, if obesity could be eliminated and we had lower mortality owing to its absence, then while health costs such as the ones mentioned here would be avoided (or at least reduced), other health costs would ultimately be incurred. I am thinking of costs in the form of elderly care or other diseases which the person might suffer from, given that they are not

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	killed off by obesity (check that this is the case). Having people not suffer from obesity and living longer is certainly a good thing and will give rise to health savings, but it will also give rise to some extra health costs. The balance is surely positive, otherwise why would we try to save lives, but if you are going to be consistent in terms of inclusion and exclusion of health related costs, then these extra costs arising from extra longevity must also be acknowledged. So once again, I am not asking for these costs to be calculated and included in the study, that would constitute another study in itself. But I believe the authors should at least refer to them. I suppose the last couple of points I am making are that I believe that the counterfactual which the authors are implicitly considering should be spelt out more clearly. Or to express it in a way which economists might be more familiar with, this study takes very much a partial equilibrium approach, concentrating solely on obesity related conditions. But eliminating obesity will have second round effects and give rise to other costs. In the case of smoking it is believed that these costs can be quite substantial (there is work by Kenneth Warner on this). I will now provide some specific comments. Page 4: The authors make reference to studies linking obesity with various conditions etc. They do not mention the work of Katherine Flagel which has been published by the Journal of the American Medical Association. Flagel's work suggests that over certain ranges of BMI higher BMI can have a protective effect. I confess to not being sure whether Flagel's work has been refuted or not, but it might be worth checking. Page 6: The authors state that a societal perspective is being used, maybe it would be useful to spell out more clearly what this implies in terms of what is and is not being included. Table 1 does not include the source of BMI for N Ireland as far as I can see. The RRs upon which the PAFs are based are calculated from Slan – are these calculated as RRs from logit regressions with controls or are they just simple prevalence rates? It does not seem clear to me. Page 8-9: It would be useful if the authors could give reassurance that the figures upon which the costs are based are not sensitive to the stage of the economic cycle, given that the figures are based on data sources from 2007 to about 2011 which incorporated quite a severe economic downturn. Page 13: what are the confidence intervals used around which the RRs (and hence PAFs) are adjusted for the Republic (95%?) Why is a figure of 1/3 used for N Ireland – why not use the CI from the RRs for N Ireland? Page 14: the estimated losses from productivity using the friction costs method are very similar North and South. But not so using the human capital approach – why is this?

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VERSION 1 – AUTHOR RESPONSE

Reviewer Name Marie Kruse
Institution and Country University of Southern Denmark
Denmark

This is a traditional cost-of-illness/burden of disease study that provides some useful figures for the societal costs of overweight and obesity in two jurisdictions: the republic of Ireland and Northern Ireland.
The main justification of this study is the very large cost figures, pointing to the urgency of political action towards the incidence (and prevalence?) of obesity in the two jurisdictions. The authors explicitly state in the introduction, that burden of illness estimates can provide useful information for priority setting, health technology assessment, etc. The overall challenge of the paper is that it doesn't quite provide any estimates that are useful for e.g. health technology assessments. A HTA would generally include an economic evaluation, where cost estimates and effect measures (e.g. QALY's or LYG) for two interventions are compared, enabling decision makers to choose the right intervention against obesity.

If this study had produced figures for the marginal costs of a new case of obesity, it could be applied in the decision making process. Such a figure should compare the cost of a new case of obesity with the cost of a normal-weight person. And it should be incidence based. If one case of obesity is prevented, the society will save $X. Or: if one obese person becomes normal-weight (and remains so), the society will save $Y.
The overall critique of this study is that it provides figures that are not useful for the decision making process because they are not marginal (per person), not attributable (i.e. costs of normal weight people are not subtracted), and they are not incidence based. It may very well be, that 1.1 and 0.5 billion EURO are numbers that are useful in a political debate, however beyond that debate there's not much you can do with it.

Response:
This study was not designed to produce unit costs, that would be an altogether different type of study. In respect of comments relating to marginal costs we have interpreted this to mean incremental costs. We think the reviewer has not properly understood the methods used and have sought to clarify our methods section here.

As noted we used both a top down and bottom up approaches in our estimation of costs. In the top down approach Population Attributable Fractions incorporate relative risks in the estimation of costs and as such capture incremental which the reviewer correctly identifies as those of particular interest. Similarly, where we have used a bottom up approach (in respect of GP services) our analyses were based on regression analyses in which the incremental cost of overweight and obesity were captured controlling for other variables. In short our analyses reflect the incremental cost of overweight and obesity which the reviewer correctly highlights is the pertinent focus of interest in studies of this type.

To help clarify this we have added the following text to the methods section:

"Bottom up estimates were based on regression analyses in which a range of health and socio-demographic covariates featured in the estimated functions. The resultant cost estimates are based on conditional probabilities of adiposity and in consequence are to be interpreted as incremental costs of adiposity. Similarly, in respect of the top down estimates of cost, PAFs are based on relative and not absolute risks of morbidity conditional on adiposity and can again be interpreted as incremental costs of adiposity. The two approaches are different, a fact we return to in our limitations section."

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In the limitations section at the paragraph beginning “Lastly this study was constrained…”
We have added:

“Fourth, with respect to those elements where cost estimates were provided a number of simplifying assumptions were employed any of which are open to debate. For example, data constraints obliged the use different methods – bottom up and top-down – that are not directly comparable. In the top down approach simplifying assumptions were employed in respect, for example, of average earnings at time of death, of the value of lost productivity among the retired, of the duration of absenteeism in Northern Ireland, of the interval over which friction costs were calculated of discount rates used etc. The use of lost output when valuing premature death as opposed, for example, to the value of a statistical life in similarly open to question. Similarly, in the bottom up approaches, alternative regression models and survey data may have resulted in alternative estimates of costs. In respect of both unobserved characteristics correlated with adiposity – for example preferences for health or attitudes to risk – that we cannot control for will complicate the estimation of incremental costs. While the precision of our estimates should in consequence be treated with some caution in the spirit of lighting a candle rather than simply cursing the dark, however, we contend these estimates at a minimum help spark a debate in respect of costs and hopefully help to provoke a policy response.”

I suggest that the authors produce a whole new set of results, per person and distinguishing between incidence and prevalence. They should also distinguish between overweight and obesity because several studies find that overweight does not cause excess health problems compared to normal-weight.

Response:
For a condition of insidious onset and lifelong duration, prevalence (which reflects incidence and duration of disease) is a more appropriate measure of disease occurrence and disease burden than incidence. The latter (incidence) is poorly defined by age and gender in Ireland as in most populations. It should also be noted that from a biological or clinical perspective, the burden of morbidity and mortality due to overweight and obesity are primarily due to the duration of disease (the major factor in prevalence) as opposed to incidence.

They should also update their terminology to current uses in health economics. I.e. ‘health care costs’ instead of ‘direct costs’, and ‘productivity costs’ instead of ‘indirect costs’ or ‘productivity loss costs’.

Response:
We thank the reviewer for highlighting our use of language in respect of direct and indirect costs and have amended this throughout the document.

My final suggestion in this round would be that the authors are more critical towards their own use of top-down estimates. Such estimates are average and not marginal and therefore (and for other reasons) often over-estimates.

Response:
We have added the following:

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Lastly, this study was constrained by lack of suitable longitudinal or cross sectional data to allow for a reliable bottom up estimate of costs. The top down approach using PAFs may have resulted in some overestimation, as these costs are average costs, and also such calculations cannot rule out double counting of cases, where multiple co-morbidities co-exist.

Reviewer Name David Madden
Institution and Country School of Economics, University College Dublin, Ireland.

Please leave your comments for the authors below I have a number of general comments, followed by some specific comments. The first general comment is: why an all-Ireland approach? My guess is because this was the condition laid down by the funder, but in terms of the academic merits of the paper it makes little sense. While the demographic and cultural profiles of the two jurisdictions are likely to be similar, the method of healthcare delivery is not. So this complicates comparison (a point which is acknowledged but not really developed in the text). If the authors really wanted to exploit the North-South differences then surely a preferable approach would be to look at differences in various measures of healthcare costs which they assign to obesity, and then decompose these differences. So, it should be possible to see what element of the difference arises because of scale of population, what element arises owing to different prevalence and PAFs, and what element arises owing to actual prices of procedures or drugs. This would enable the construction of a counter-factual along the lines of: what would be the obesity related cost of (for example) stroke in Northern Ireland if it has the same demographic and prevalence profile as the Republic. Of course, this does ignore an element of endogeneity in that the mode of healthcare delivery may well influence prevalence of disease, but I feel it would go some way towards exploiting the North-South dimension in a way that the current paper does not. While I am not insisting that such analysis should be included in this paper, it gives some idea of what form of analysis a North-South comparison might take. As it stands, the comparative analysis is imposed on the data without really doing anything. The authors should consider dropping it, or at least finding some way of exploiting it properly.

Response:
In respect of the North/South comparison, we concur with the reviewer that a decomposition of differences in cost between North and South would be provide a valuable and interesting investigation. We have chosen not to develop this within the current paper as we think we do not have sufficient space to do justice to this and explain and discuss our existing cost estimates as well as this additional avenue of investigation.

To recognize this fact we have added the following text [insert at comment 2]

“Decomposing differences in cost on the two parts of the island would provide an interesting and useful exercise, in for example, investigating the impact on health care costs differences in access to public services might have. As this was not the focus of this paper and mindful of constraints on space we have chosen not to provide such an analysis here.”

The second main point I wish to make concerns the type of counter-factual implied in the analysis (and indeed implied in all analysis of this nature). The exercise is designed to arrive at some estimate of the costs of obesity in Ireland. So presumably the authors are trying to discover how much lower healthcare costs would be in the absence of obesity. And they do this by calculating risk ratios for

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various conditions for those with obesity compared to those without (are these raw risk ratios, or are other covariates controlled for? – this is not entirely clear). And then they calculate PAFs etc. I think this approach would be fine if obesity was randomly allocated across the population. But my guess is that obesity is not randomly allocated across the population and that there are unobserved factors associated with obesity which are also associated with the various obesity-related conditions listed here. It also seems more plausible to me that these unobserved factors which are correlated with obesity will also be correlated with these other conditions. Thus if we were to wave a magic wand and eliminate obesity, we would still observe higher RRs for the formerly-obese, higher RRs which arise owing to these unobserved factors. All of which implies that the costs presented here as arising from obesity are probably overestimates of the costs arising from obesity per se.

Response:
In respect of the counterfactual we agree entirely with the reviewer that modeling this is in a convincing manner is not straightforward. Both the relative risks used in the top down approach and the regression functions used in the bottom up approach have endeavored to control for pertinent observable characteristics – for example age and gender in the former, age, gender, marital status and education in the latter. With respect to unobserved characteristics such as risk perception or preferences for health we acknowledge these as constraints.

We have added additional text to our methods section in respect of our modeling as noted and have added additional discussion in our limitations section to highlight these issues.

Endogeneity also raises its head in another of the costs which are estimated, that of lost productivity. The authors cost the lost hours arising from obesity related conditions at the average wage costs adjusted for age and gender. But it seems plausible that once again, those unobserved factors associated with obesity might also affect wages, so that wages for the obese may not be the same as those for the non-obese. Interestingly, evidence seems to suggest that the effect can work either way, though the balance of evidence seems to indicate a wage penalty.
Having raised these issues, is there any way in which this endogeneity can be addressed? In truth there is no easy way out of this, and I am not suggesting that the authors need to adopt elaborate statistical approaches to try to correct for this endogeneity, especially as such approaches, with non-experimental data, are of dubious efficacy. But I would like to see a discussion and acknowledgement of the issue in the paper.

Response:
With respect to average wages in our calculation of lost productivity we thank the reviewer for drawing our attention to this. Rather than speculate as to the magnitude of the wage penalty we have noted in the text that this may be the case and cautioned as to its implications for our estimates. We have inserted the following text:

“The use of average earnings to estimate lost productivity is open to question. Those who are overweight or obese may experience an earnings penalty related to unobserved characteristics and/or discrimination in the workplace. Rather than speculate as to the magnitude of such effects (and there differences between jurisdictions) we have used the average wage figure but caution as to the interpretation of results.”

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The authors indicate that they are taking a societal perspective. I am not quite sure what they mean by this, but I infer that they are looking at social and not private costs. Thus if there was a health insurance premium associated with obesity then this would constitute a private and not a social cost. They seem to be implying also that all productivity losses are borne by the firm and/or other workers. This may well be the case in many workplaces, but there are also situations where it will not be the case. If I am self-employed, or I am employee working on a piece rate, then if I am less productive owing to an obesity related condition, this is a private, not a social cost. But the authors do not consider this at all. Perhaps in some of the data available they may have information on the self-employed, in which case perhaps some adjustment might be needed to the costs associated with productivity losses.

In respect of the "societal perspective" our terminology reflects that commonly used in the literature. In the interests of clarity we have included in parenthesis at its introduction a definition of the term. "(i.e. that includes both healthcare costs and lost productivity)"

Amongst the costs which the authors include are losses owing to premature mortality. I note that these costs seem to be calculated via what could be called the human capital approach, rather than a valuation of a statistical life (VOSL) approach. I guess this is consistent with only including social costs though my own preference would be for a VOSL approach or at least some acknowledgement that if someone dies prematurely there are costs to others in society, even if is only close friends or relatives.

However, going back once again to the counterfactual, if obesity could be eliminated and we had lower mortality owing to its absence, then while health costs such as the ones mentioned here would be avoided (or at least reduced), other health costs would ultimately be incurred. I am thinking of costs in the form of elderly care or other diseases which the person might suffer from, given that they are not killed off by obesity (check that this is the case). Having people not suffer from obesity and living longer is certainly a good thing and will give rise to health savings, but it will also give rise to some extra health costs. The balance is surely positive, otherwise why would we try to save lives, but if you are going to be consistent in terms of inclusion and exclusion of health related costs, then these extra costs arising from extra longevity must also be acknowledged. So once again, I am not asking for these costs to be calculated and included in the study, that would constitute another study in itself. But I believe the authors should at least refer to them.

I suppose the last couple of points I am making are that I believe that the counterfactual which the authors are implicitly considering should be spelt out more clearly. Or to express it in a way which economists might be more familiar with, this study takes very much a partial equilibrium approach, concentrating solely on obesity related conditions. But eliminating obesity will have second round effects and give rise to other costs. In the case of smoking it is believed that these costs can be quite substantial (there is work by Kenneth Warner on this).

Response:
We thank the reviewer for these comments and have attempted to address them in the text.

I will now provide some specific comments.

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Page 4: The authors make reference to studies linking obesity with various conditions etc. They do not mention the work of Katherine Flagel which has been published by the Journal of the American Medical Association. Flagel's work suggests that over certain ranges of BMI higher BMI can have a protective effect. I confess to not being sure whether Flagel's work has been refuted or not, but it might be worth checking.

A minority of studies have shown a protective effect of overweight (not obesity) in middle aged men. We make reference in our text to the better predictive nature of waist circumference measurement than BMI, but in the interests of brevity, we have not expanded on this point.

Page 6: The authors state that a societal perspective is being used, maybe it would be useful to spell out more clearly what this implies in terms of what is and is not being included.

(i.e. that includes both healthcare costs and lost productivity) has been included in the text

Table 1 does not include the source of BMI for N Ireland as far as I can see.

Corrected

The RRs upon which the PAFs are based are calculated from Slan – are these calculated as RRs from logit regressions with controls or are they just simple prevalence rates? It does not seem clear to me.

The RRs were taken from international literature, as has been made explicit in the methods section now.

Page 8-9: It would be useful if the authors could give reassurance that the figures upon which the costs are based are not sensitive to the stage of the economic cycle, given that the figures are based on data sources from 2007 to about 2011 which incorporated quite a severe economic downturn.

This study used data from 2009, before the first round of pay cuts as a result of the downturn.

Page 13: what are the confidence intervals used around which the RRs (and hence PAFs) are adjusted for the Republic (95%?) Why is a figure of 1/3 used for N Ireland – why not use the CI from the RRs for N Ireland?
Thank you for pointing this out. This is in fact an error and the sensitivity analysis was performed using the same method as for the Republic. This has been corrected in the text.

Page 14: the estimated losses from productivity using the friction costs method are very similar North and South. But not so using the human capital approach – why is this?

I think that this reflects the differences in the amount and type of data available in both jurisdictions, which was magnified when the Human capital approach was used.

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VERSION 2 – REVIEW

REVIEWER	David Madden School of Economics, University College Dublin, Ireland.
REVIEW RETURNED	19-Jan-2015

GENERAL COMMENTS	This version is certainly an improvement over the original and a number of the issues which I raised have been addressed. However, not all issues have been covered and in some cases the authors do not address the issue adequately. Thus the issue of unobserved factors associated with obesity is briefly mentioned on page 17, and rather causally dealt with by the comment: In respect of both unobserved characteristics correlated with adiposity – for example preferences for health or attitudes to risk – that we cannot control for will complicate the estimation of incremental costs. While the precision of our estimates should in consequence be treated with some caution in the spirit of lighting a candle rather than simply cursing the dark, however, we contend these estimates at a minimum help spark a debate in respect of costs and hopefully help to provoke a policy response. I think the authors could do better here. The bias involved in not taking account of these factors almost certainly leads to an overestimate of the costs of obesity and this should be acknowledged. I am sympathetic to the authors' view that we have to come up with some numbers and cannot be paralysed into inaction by fears that our estimates are not 100% accurate, but where it is fairly clear in which direction a particular omission is likely to work, this should be recognised. The authors also fail to address the point regarding the reductions in health care costs which arise owing to the presence of obesity. Premature mortality, however tragic we may view it, will lead to lower future costs in terms of elderly care etc. It goes back to the counterfactual you are looking at: the authors claim that in the absence of obesity various healthcare costs would be lower, and allowing for the uncertainty involved in these estimates, I agree with them. But other costs would be higher, and while I am not looking for estimates of these costs, they should be acknowledged. The authors also fail to mention that for some (e.g. self-employed), the costs of lost output arising from obesity are private not social. Some other (minor) issues: The authors should define their terms early on. Does "Ireland" refer to the Republic of Ireland or the whole island? In the Introduction It is not always clear, so a definition at the start would be helpful especially for non-Irish (all-island!) readers. The authors should consider the sensitivity of some of their estimate to the choice of discount rate. It is understandable that they use the "official" rate of 4%. But we are now living in a possibly new era of lower interest rates and already there have been court judgements in the Republic reflecting the view that official discount rates should perhaps be lower. It would be interesting (and trivial for the authors

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	to calculate) to see what their results would look like with alternative discount rates. Table 3 should give total costs in the last row for each jurisdiction. The first paragraph of the discussion mentions that care in Northern Ireland is delivered free of care for everyone, while this is only true of medical card patients in the Republic and speculates that the higher price at point of demand in the Republic might reduce demand. Thus if the Republic had free care at point of use for everyone, then demand would be higher. Equivalently if Northern Ireland moved to a mixed system like that in the Republic, then demand would presumably be lower in the North. They could develop this point. The likelihood seems to be greater that the Republic will move towards free care at point of use for all than that the North will move in the opposite direction. The conventional wisdom is that healthcare is inelastically demanded (i.e. price elasticity of demand is less than one in absolute terms). Thus if price to the consumer goes down, there will be a rise in demand in the Republic, but it will be proportionally less than the fall in consumer price. Of course the GP will still have to be reimbursed by the state so there is still a cost, but the rise in cost might not be great as initially presumed.

REVIEWER	Marie Kruse University of Southern Denmark Denmark
REVIEW RETURNED	22-Jan-2015

GENERAL COMMENTS	I just read through my previous review. It appears that the authors haven't really changed a lot. If they wish to publish in an international journal, they should make an effort to make their results relevant to people from other countries. It is still not clear why they pool those two jurisdictions. What is the value-added for the analysis? I enclose my old review. This is a traditional cost-of-illness/burden of disease study that provides some useful figures for the societal costs of overweight and obesity in two jurisdictions: the republic of Ireland and Northern Ireland. The main justification of this study is the very large cost figures, pointing to the urgency of political action towards the incidence (and prevalence?) of obesity in the two jurisdictions. The authors explicitly state in the introduction, that burden of illness estimates can provide useful information for priority setting, health technology assessment, etc. The overall challenge of the paper is that it doesn't quite provide any estimates that are useful for e.g. health technology assessments. A HTA would generally include an economic evaluation, where cost estimates and effect measures (e.g. QALY's or LYG) for two interventions are compared, enabling decision makers to choose the right intervention against obesity.

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	If this study had produced figures for the marginal costs of a new case of obesity, it could be applied in the decision making process. Such a figure should compare the cost of a new case of obesity with the cost of a normal-weight person. And it should be incidence based. If one case of obesity is prevented, the society will save $X. Or: if one obese person becomes normal-weight (and remains so), the society will save $Y. The overall critique of this study is that it provides figures that are not useful for the decision making process because they are not marginal (per person), not attributable (i.e. costs of normal weight people are not subtracted), and they are not incidence based. It may very well be, that 1.1 and 0.5 billion EURO are numbers that are useful in a political debate, however beyond that debate there's not much you can do with it. I suggest that the authors produce a whole new set of results, per person and distinguishing between incidence and prevalence. They should also distinguish between overweight and obesity because several studies find that overweight does not cause excess health problems compared to normal-weight. They should also update their terminology to current uses in health economics. I.e. 'health care costs' instead of 'direct costs', and 'productivity costs' instead of 'indirect costs' or 'productivity loss costs'. My final suggestion in this round would be that the authors are more critical towards their own use of top-down estimates. Such estimates are average and not marginal and therefore (and for other reasons) often over-estimates.

VERSION 2 – AUTHOR RESPONSE

Reviewer Name David Madden
Institution and Country School of Economics, University College Dublin, Ireland Please state any competing interests or state 'None declared': None.

Please leave your comments for the authors below

This version is
certainly an improvement over the original and a number of the issues which I raised have been addressed. However, not all issues have been covered and in some cases the authors do not address the issue adequately.

Thus the issue of unobserved factors associated with obesity is briefly mentioned on page 17, and rather causally dealt with by the comment:

In respect of both unobserved characteristics correlated with adiposity - for example preferences for health or attitudes to risk - that we cannot control for will complicate the estimation of incremental costs. While the precision of our estimates should in consequence be treated

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with some caution in the spirit of lighting a candle rather than simply cursing the dark, however, we contend these estimates at a minimum help spark a debate in respect of costs and hopefully help to provoke a policy response.

I think the authors could do better here. The bias involved in not taking account of these factors almost certainly leads to an overestimate of the costs of obesity and this should be acknowledged. I am sympathetic to the authors' view that we have to come up with some numbers and cannot be paralysed into inaction by fears that our estimates are not 100% accurate, but where it is fairly clear in which direction a particular omission is likely to work, this should be recognised.

The authors have added the following text to the paper:

In respect of both unobserved characteristics correlated with adiposity – for example preferences for health or attitudes to risk –we acknowledge that we have not been able to take these into account in this study, and that the bias from such factors may well lead to an overestimate of the costs of obesity.

The authors also fail to address the point regarding the reductions in health care costs which arise owing to the presence of obesity. Premature mortality, however tragic we may view it, will lead to lower future costs in terms of elderly care etc. It goes back to the counterfactual you are looking at: the authors claim that in the absence of obesity various healthcare costs would be lower, and allowing for the uncertainty involved in these estimates, I agree with them. But other costs would be higher, and while I am not looking for estimates of these costs, they should be acknowledged.

While the authors acknowledge that this is often considered to be the case, we consider that there has been sufficient research in the area of morbidity compression, showing that it can reduce health care costs. Premature mortality is not necessarily cost saving nor longevity cost increasing. In respect of healthcare the issue is one of morbidity compression people can live longer and not cost more – e.g. O'Neill C. Groom L, Avery A, Thornhill K.
"Age and Proximity to Death as Determinants of Health Care Costs: Results from Study of Nursing Home Patients." Health Economics Vol. 9(8) pp 733-738. 2000.

The authors also fail to mention that for some (e.g. self-employed), the costs of lost output arising from obesity are private not social.

I'm not sure what he means here. We can say that while they will fall on the individual they will also be felt in GDP and thus broader societal measures also.

The authors have added the following to the text:

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Also excluded are the costs of lost production in the self-employed, which are private rather than societal costs.

Some other (minor) issues:

The authors should define their terms early on. Does "Ireland" refer to the Republic of Ireland or the whole island? In the Introduction It is not always clear, so a definition at the start would be helpful especially for non-Irish (all-island!) readers.

We have added the line: (Ireland refers to the Republic of Ireland, while mentions Northern Ireland refers to the Northern part of Ireland which is part of the UK, and the Island of Ireland refers to the whole) to the introduction.

The authors should consider the sensitivity of some of their estimate to the choice of discount rate. It is understandable that they use the "official" rate of 4%. But we are now living in a possibly new era of lower interest rates and already there have been court judgements in the Republic reflecting the view that official discount rates should perhaps be lower. It would be interesting (and trivial for the authors to calculate) to see what their results would look like with alternative discount rates.

A sensitivity analysis was done, and we have now included it as follows:

The basic analysis used undiscounted years of life and weights these years of life using undiscounted income. As part of the sensitivity analysis, the effect of using different discount rates, 2%, 4%, 6%, 8% and 10% was investigated. As expected both the absolute number of years of life lost and the corresponding costs fall sharply. The figures are shown in Table 5 below.

	Northern Ireland	Republic of Ireland
Discount rate	Age to 75	Age to 75
0.00%	€210,763,565.53	€8,208,337,242
2.00%	€173,995,104.01	€7,487,852,062
4.00%	€147,417,113.39	€6,878,312,466
6.00%	€127,567,162.23	€6,358,103,699
8.00%	€112,326,333.97	€5,910,531,821
10.00%	€100,345,294.30	€5,522,568,832

Table 3 should give total costs in the last row for each jurisdiction.

We have done this. In doing this, an error was noted in the section on hospital inpatient and day case costs, and this has been corrected in tables 3 and 4, now highlighted in yellow.

The first paragraph of the discussion mentions that care in Northern Ireland is delivered free of care (sic. cost) for everyone, while this is only true of medical card patients in the Republic and speculates that the higher

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price at point of demand in the Republic might reduce demand. Thus if the Republic had free care at point of use for everyone, then demand would be higher. Equivalently if Northern Ireland moved to a mixed system like that in the Republic, then demand would presumably be lower in the North. They could develop this point. The likelihood seems to be greater that the Republic will move towards free care at point of use for all than that the North will move in the opposite direction. The conventional wisdom is that healthcare is inelastically demanded (i.e. price elasticity of demand is less than one in absolute terms). Thus if price to the consumer goes down, there will be a rise in demand in the Republic, but it will be proportionally less than the fall in consumer price. Of course the GP will still have to be reimbursed by the state so there is still a cost, but the rise in cost might not be great as initially presumed.

We acknowledge that the reviewer is correct, and we have added the following text:

. If the systems were equivalent, i.e. if healthcare was also free at the point of use in the Republic of Ireland, one would expect a rise in demand, with increased costs, although the fact that healthcare is relatively price inelastic means that the increase in cost would be disproportionately less than the rise in demand. While the different systems might in part explain some of the differences in cost across the two jurisdictions

Reviewer Name Marie Kruse
Institution and Country University of Southern Denmark Denmark Please state any competing interests or state 'None declared': None declared

Please leave your comments for the authors below I just read through my previous review. It appears that the authors haven't really changed a lot. If they wish to publish in an international journal, they should make an effort to make their results relevant to people from other countries.

It is still not clear why they pool those two jurisdictions. What is the value-added for the analysis?

We have added the following text: The pooling of two jurisdictions (as per the funding requirements) for this study provides useful comparisons across the jurisdictions, especially as there are differing healthcare systems.

I enclose my old review.

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Correction

Dee A, Callnan A, Doherty E, *et al*. Overweight and obesity on the island of Ireland: an estimation of costs. *BMJ Open* 2015;5:e006189. The correct spelling of the second author's name is Aoife Callan.

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