

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	A before/After Intervention study to determine impact on life cycle carbon footprint of converting from single-use to reusable sharps containers in 40 United Kingdom NHS Trusts
AUTHORS	Grimmond, Terry; Bright, Anna; Cadman, June; Dixon, James; Ludditt, Sally; Robinson, Clive; Topping, Clare

VERSION 1 – REVIEW

REVIEWER	Chantelle Rizan Brighton and Sussex University Hospitals NHS Trust
REVIEW RETURNED	07-Jan-2021

GENERAL COMMENTS	<p>General comments</p> <p>This article addresses an important topic and provides evidence that the carbon footprint of reusable sterilisation containers are lower than single use equivalents. Unfortunately this paper is of limited additional value to the prior publications on this topic by the first author which show similar findings but in US settings (referenced 25&26 within the study). This paper does however provide evidence supporting use of reusable SC in a UK setting, and also uses more representative travel assumptions due to data from multiple hospitals. A more valuable piece of future research might be to conduct scenario analysis in which the conditions and thresholds are determined to identify parameters and settings in which the carbon footprint of RSC<SSC (e.g. vehicle type, distance, electricity source).</p> <p>It appears that analysis was based on real activity using the SSC vs RSC system. A more helpful analysis might be to compare the system for the same functional unit (e.g. holding Xg of sharps) as there are multiple confounding factors which may have effected the number of sharps generated (beyond workload accounted for).</p> <p>It would be helpful to specify the primary data collected on energy & water use for decanting & decontamination of the RSC, within supplementary data. The contributions for the reusable item from reprocessing seem low as compared with other LCAs evaluating reusable items. This may be due to lower sterility standards needing to be achieved. It would be worth exploring this and commenting in results.</p> <p>Abstract</p> <p>Please amend typo- this should read 95% CI not 'CL'</p>
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	<p>Please amend abstract in line with recommendations to main text below</p> <p>Intro</p> <p>Please use the latest figures for NHS carbon footprint for proportion of national emissions (i.e. 4% from 'Delivering a Net Zero NHS' 2020 report)</p> <p>Consider including other studies examining carbon footprint of single-use vs reusable items within intro/ discussion e.g.</p> <ul style="list-style-type: none"> • Ibbotson S, Dettmer T, Kara S, et al. Eco-efficiency of disposable and reusable surgical instruments - a scissors case. <i>Int J Life Cycle Assess.</i> 2013;18: 1137–1148. • Overcash M. A comparison of reusable and disposable perioperative textiles: sustainability state-of-the-art 2012. <i>Anesth Analg.</i> 2012;114:1055–1066. • 44. McGain F, McAlister S, McGavin A, et al. The financial and environmental costs of reusable and single-use plastic anaesthetic drug trays. <i>Anaesth Intensive Care.</i> 2010;38:538–544. • 45. Eckelman M, Mosher M, Gonzalez A, et al. Comparative life cycle assessment of disposable and reusable laryngeal mask airways. <i>Anesth Analg.</i> 2012;114:1067–1072. • 46. Sherman JD, Raibley LA, Eckelman MJ. Life cycle assessment and costing methods for device procurement: comparing reusable and single-use disposable laryngoscopes. <i>Anesth Analg.</i> 2018;127:434–443. • 47. Davis NF, McGrath S, Quinlan M, et al. Carbon footprint in flexible ureteroscopy: a comparative study on the environmental impact of reusable and single-use ureteroscopes. <i>J Endourol.</i> 2018;32:214–217. <p>Methods</p> <p>The sources of GHG conversion factors should be summarised within the methods.</p> <p>It is unclear whether primary data was collected on manufacturing inputs (e.g. electricity, water, fuel).</p> <p>Regarding work-load normalised results, was this calculated for the trusts involved rather than national figures (i.e. were the results divided by total number of finished admission episodes performed annually across the 47 trusts?) Please specify this within methods.</p> <p>The adjustment for change in NHS workload should be included in the methods.</p> <p>I would recommend consulting a statistician, but to my mind the statistics used here do not seem appropriate. My understanding is that you have compared two single point measures of carbon footprints. Use of chi squared is therefore not valid as this relies on comparing the distribution of a discontinuous variable between 2+ independent samples. A rate ratio has also been performed and again does not seem appropriate here. Most carbon footprinting studies comparing two systems would not use p values unless</p>
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	<p>making multiple measures of slightly different scenarios, or following running Monte-Carlo analysis. If this was done this should be made clear.</p> <p>Results</p> <p>Please specify what 'container exchanges' refers to within the table legend.</p> <p>The method suggests 3 measures of workload were taken into account but only one appears to have been reported within the results. Please adjust method or results accordingly.</p> <p>It would be helpful to determine the contribution of the SSC for special purposes to the RSC scenario.</p> <p>Discussion</p> <p>How was the reduction in labour calculated?</p> <p>The sensitivity analysis and parameters for this should be detailed in the methods section, and results of the sensitivity analysis within the results section rather than discussion. Please also specify the direction of difference within the transport sensitivity analysis.</p> <p>The extent of recycling of 80% of reusable containers at the end of life is unlikely to be complete, and this should be discussed as a study limitation, or alternatively contribution of disposal (e.g. via domestic waste or other) accounted for.</p> <p>Supplementary data Please spell out all acronyms within table legend.</p>
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REVIEWER	Matthew Eckelman
REVIEW RETURNED	12-Jan-2021

GENERAL COMMENTS	<p>This study is timely and will be useful to the healthcare sustainability community, especially in the UK. I have one major suggestion to perform a quantitative uncertainty analysis for the entire model, otherwise only minor comments, below:</p> <ul style="list-style-type: none"> - In addition to the HCWH piece, for global context of healthcare emissions, the following publications may also be of interest – similar findings to the HCWH report but peer-reviewed: Pichler et al. https://iopscience.iop.org/article/10.1088/1748-9326/ab19e1 Watts et al. https://www.thelancet.com/journals/lancet/article/PIIS0140-6736(20)32290-X/fulltext Lenzen et al. https://www.thelancet.com/journals/lanplh/article/PIIS2542-5196(20)30121-2/fulltext - Access to actual procurement data is a major strength of the work, and the coverage of 75% of trusts using RSCs means that the work is broadly representative. - Introduction, "clinical waste containers are among the top 20 contributors to the UK NHS supply chain carbon footprint" This
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	<p>sentence needs more context, that this list covers only medical devices, food and catering, paper, and mfg fuels and chemicals, but excludes services and other product types such as pharmaceuticals.</p> <ul style="list-style-type: none"> - Introduction, please clarify the statement "peer-reviewed, quantitative studies are required." Required by whom, for what purpose? - Abbreviate GHG after it appears the first time spelled out. - Please cite exactly which GWP100 values were used, from AR5, with/without indirect effects, etc. - Study overview jumps right into study design and noting past results. I think it would help readers also to have a short description of the product system, including the design specification differences between the two containers - the method for adjusting procurement requirements for increases in FAEs makes sense. - "calculation model" is vague, if this was a spreadsheet model constructed in MSEXcel, please note that. - perhaps the citations have gotten off, it appears that citation 30 should be 26, for PAS2050? - the PAS2050:2011 standard was used, which has been aligned with the GHG Protocol since the original version, but does it now also use the Scopes framework? If not, please use the categories set forth in the PAS2050 standard, and please use PAS2050 terminology throughout. Also, could clarify that Scope 3 emissions include those from waste management and final disposal, they are not just about upstream supply chain. - Details on waste management for the RSCs at end of life should be described in the same place as for the SSC. - GHG Allocation, presumably upstream mass-based allocation for petrochemical refining and the like was not conducted by the authors...? Please cite here in the Data Sources section what LCI data sets were used (i.e., PlasticsEurope) and differentiate between allocation done in the background data sets versus the foreground model. In this section, please detail exactly what the assumed sources of avoided emissions were (i.e., UK grid average electricity). - please keep the units consistent as kg CO₂e, and not KgCo₂e. - Most of the Results section appears to be Methods details and should be moved to that section. Results can focus more on the contributions to GHG emissions for each product system shown in Figure 2. - Figure 1, 'Non-GHG Outputs' should be changed to 'Non-GHG emissions' or environmental burdens, following PAS2050. - In all results, figures, and tables, please use a consistent number of significant figures. - There is a quantitative uncertainty analysis done related to the procurement data but not the emissions data, if I am understanding it. It would be preferable to have a quantitative uncertainty analysis that applies to the full model and can be used to add error bars to Figure 2, for example. - Good extrapolation of results to the total NHS to provide context about the relative importance of this measure of switching to RSCs. - The sensitivity analysis section on mfg focuses on electricity and the role of geography, but this is not the only energy type used in petrochemicals and polymers mfg, please clarify. - In the final sections of the paper, the authors use the word 'significantly' several times. Is this meant in the statistical sense, or do they simply mean that the quantities are large. Even if it is the
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	<p>latter, pls try to give readers as sense of the threshold of significance. Is a 20% reduction significant? What about a 5% reduction?</p> <p>- The authors do a nice job of tying the work to the Greener NHS guidance and reports.</p>
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REVIEWER	Peter-Paul Pichler Potsdam Institute for Climate Impact Research, Social Metabolism & Impacts
REVIEW RETURNED	18-Jan-2021

GENERAL COMMENTS	<p>The study investigates whether a switch from single-use sharps containers to reusable containers in hospitals leads to a reduction in the associated greenhouse gas emissions over the entire life cycle. The topic of the study is relevant and although containers represent only a small part of the total emissions in the healthcare system, such detailed studies are important to help hospitals make concrete purchasing decisions.</p> <p>The manuscript is clear and well written, but in its current form it contains weaknesses that I believe need to be addressed before publication. I only list the three major concerns I have with the present manuscript that I think need to be addressed before potential publication.</p> <p>My most serious criticism is that the main conclusion „Reusable devices achieve significant GHG reductions over single-use plastic devices (p12)“ is not supported in this generality by the results of the study. The major limitation of generality, in my view, arises partly from the specificity of the supply chain studied and partly from the large differences in container size and its effect on production and transportation effort. A more appropriate (admittedly slightly exaggerated) conclusion would be that „Michigan-produced reusable devices of a volume of 18.8l result in significant reductions over the use of smaller disposable containers (6.7l) when the plastic is emissions-intensively manufactured in Saudi Arabia.“</p> <p>The sensitivity analysis could be used to discuss both of those issues. At the moment, the influence of the carbon intensity of energy production in the country of production is mentioned, but its influence on the results and the consequences for the conclusions is not quantified. The size of the containers, which seems to me most important for the magnitude of CO2 savings, is not considered at all in the sensitivity analysis. There may be reasons for both, but they are not mentioned and I am therefore skeptical about the actual significance of these figures.</p> <p>My second concern regards the description of the methods which is quite vague or not available (I did not have access to a methods appendix, apologies if one was provided). For example, for the sensitivity analysis above, it is simply stated „A sensitivity analysis revealed“ with little more information how that was done, what parameter ranges were tested or how plausible those are. To allow for the results to be reproduced, a methods appendix should be provided, ideally including the software code and the data in machine readable form to allow computational reproducibility. I have never understood what a good reason could be for providing data and information about a published study only „on request“.</p>
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	<p>Finally, I think the authors should make much greater effort to situate the study in the existing scientific literature. Currently, of the 52 references in the manuscript, by my count only 6 (!) are to peer-reviewed scientific studies.</p> <p>Important typo/error: in the data table in row 13 (GHG of electricity for SSC mfg (UK)) it should say „Saudi Arabia“ instead of UK.</p> <p>Overall, I think this study can be a relevant and interesting addition to the literature if those concerns are addressed, even if the conclusions need to be much more qualified and even if probably some of the limitations I raised cannot be addressed on a quantitative level, but only discussed more thoroughly.</p>
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VERSION 1 – AUTHOR RESPONSE

	Page/ Line	Reviewer Comment	Author Action	Author comment
		REVIEWER 1		
#1		It appears that analysis was based on real activity using the SSC vs RSC system. A more helpful analysis might be to compare the system for the same functional unit (e.g. holding Xg of sharps) as there are multiple confounding factors which may have effected the number of sharps generated (beyond workload accounted for).	Agreed	Result also now expressed as kg CO2eq/1000 Fill-line Litres for each containment system - see in Abstract line 27; Methods lines 211-2; and Results last row of Table 1.
#2		It would be helpful to specify the primary data collected on energy & water use for decanting & decontamination of the RSC, within supplement. data.	Agreed	Stated in BMJOpen online Supplementary data (now included in

				submission)
#3		The contributions for the reusable item from reprocessing seem low as compared with other LCAs evaluating reusable items. This may be due to lower sterility standards needing to be achieved. It would be worth exploring this and commenting in results.	Agreed	Explanation added Lines 275-6. NB. RSC require thorough cleaning/disinfection between uses (not sterilization).
#4		Please amend typo- this should read 95% CI not 'CL'	Agreed	Now deleted
#5		Please amend abstract in line with recommendations to main text below	Agreed	Abstract now matches Body
#6		Intro. Please use the latest figures for NHS carbon footprint for proportion of national emissions (i.e. 4% from 'Delivering a Net Zero NHS' 2020 report)	Agreed	Updated Line 75
#7		<ul style="list-style-type: none"> • Intro/Discussion. Consider including other studies examining carbon footprint of single-use vs reusable items within intro/ discussion e.g. <ul style="list-style-type: none"> • Ibbotson S, Dettmer T, Kara S, et al. Eco-efficiency of disposable and reusable surgical instruments - a scissors case. Int J Life Cycle Assess. 2013;18: 1137–1148. • Overcash M. A comparison of reusable and disposable perioperative textiles: sustainability state-of-the-art 2012. Anesth Analg. 2012;114:1055–1066. 	Agreed	6 peer-reviewed studies suggested now included in Introduction at lines 66, 67, 73, 88.

	<ul style="list-style-type: none"> • McGain F, McAlister S, McGavin A, et al. The financial and environmental costs of reusable and single-use plastic anaesthetic drug trays. <i>Anaesth Intensive Care</i>. 2010;38:538–544. • Eckelman M, Mosher M, Gonzalez A, et al. Comparative life cycle assessment of disposable and reusable laryngeal mask airways. <i>Anesth Analg</i>. 2012;114:1067–1072. • Sherman JD, Raibley LA, Eckelman MJ. Life cycle assessment and costing methods for device procurement: comparing reusable and single-use disposable laryngoscopes. <i>Anesth Analg</i>. 2018;127:434–443. • Davis NF, McGrath S, Quinlan M, et al. Carbon footprint in flexible ureteroscopy: a comparative study on the environmental impact of reusable and single-use ureteroscopes. <i>J Endourol</i>. 2018;32:214–217. 		
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#8	Methods. The sources of GHG conversion factors should be summarised within the methods.	Agreed	Stated in BMJOpen online Supplementary data (now included in submission)
#9	Methods. It is unclear whether primary data was collected on manufacturing inputs (e.g. electricity, water, fuel).	Agreed	Clarified in lines 168
#10	Methods. Regarding work-load normalised results, was this calculated for the trusts involved rather than national figures (i.e. were the results divided by total number of finished admission episodes performed annually across the 47 trusts?) Please specify this within methods.	Agreed	Clarified in lines 187 and 188
#11	Methods. The adjustment for change in NHS workload should be included in the methods.	Agreed	Included in “Baseline Data” section of Materials and Methods at lines 200-3
#12	Methods. I would recommend consulting a statistician, but to my mind the statistics used here do not seem appropriate. My understanding is that you have compared two single point measures of carbon footprints. Use of chi squared is therefore not valid as this relies on comparing the distribution of a discontinuous variable between 2+ independent samples. A rate ratio has also been performed and	Agreed	Statistical comparison using p values removed.

	again does not seem appropriate here. Most carbon footprinting studies comparing two systems would not use p values unless making multiple measures of slightly different scenarios, or following running Monte-Carlo analysis. If this was done this should be made clear.		
#13	Results. Please specify what 'container exchanges' refers to within the table legend.	Agreed	Defined in legend at line 289
#14	Results. The method suggests 3 measures of workload were taken into account but only one appears to have been reported within the results. Please adjust method or results accordingly.	Agreed	Sentence clarified in Line 277
#15	Results. It would be helpful to determine the contribution of the SSC for special purposes to the RSC scenario.	Agreed	Contribution of retained SSC was 3% - see lines 262. Also captured in Table 1.
#16	Discussion. How was the reduction in labour calculated?	Agreed	Clarified at lines 282-285
#17	Discussion. The sensitivity analysis and parameters for this should be detailed in the methods section, and results of the sensitivity analysis within the results section rather than discussion.	Agreed	Sensitivity methods and results moved to their relevant sections.
#18	Discussion. Please also specify the direction of difference within the transport sensitivity analysis.	Agreed	Deleted. Now incorporated in new Table 2

#19	Discussion. The extent of recycling of 80% of reusable containers at the end of life is unlikely to be complete, and this should be discussed as a study limitation, or alternatively contribution of disposal (e.g. via domestic waste or other) accounted for.	Agreed	Sentence included in Limitations at lines 331-2
#20	Supplementary data. Please spell out all acronyms within table legend.	Agreed	Legend acronyms expanded – see lines 289-302
	Reviewer 2		
#21	I have one major suggestion to perform a quantitative uncertainty analysis for the entire model,	Agreed	New Table 2 depicts quantitation of lowest impact alternatives.
#22	In addition to the HCWH piece, for global context of healthcare emissions, the following publications may also be of interest – similar findings to the HCWH report but peer-reviewed: <ul style="list-style-type: none"> • Pichler et al. https://iopscience.iop.org/article/10.1088/1748-9326/ab19e1 • Watts et al. https://www.thelancet.com/journals/lancet/article/PIIS0140-6736(20)32290-X/fulltext • Lenzen et al. 	Agreed	9 suggested Peer-reviewed studies now included in manuscript- see Intro first 3 paras.

		https://www.thelancet.com/journals/lanplh/article/PIIS2542-5196(20)30121-2/fulltext		
#23		Introduction. “clinical waste containers are among the top 20 contributors to the UK NHS supply chain carbon footprint” This sentence needs more context, that this list covers only medical devices, food and catering, paper, and mfg fuels and chemicals, but excludes services and other product types such as pharmaceuticals.	Agreed	Corrected line 89
#24		Introduction. please clarify the statement “peer-reviewed, quantitative studies are required.” Required by whom, for what purpose?	Agreed	Reworded with references line 93-4
#25		Abbreviate GHG after it appears the first time spelled out.	Agreed	Corrected NB. Other uses of full terms are necessitated by : • Start of sentence • Reviewer 1 requirement in Table Legend (Task 20).
#26		Please cite exactly which GWP100 values were used, from AR5, with/without indirect effects, etc.	Agreed	Clarified at Lines 133-7
#27		Study overview jumps right into study design and noting past results. I think it would help readers also to have a short description of the product system,	Agreed	Overview of container types and handling moved from later in Methods to

		including the design specification differences between the two containers		beginning – see lines 101-111
#28		“calculation model” is vague, if this was a spreadsheet model constructed in MSExcel, please note that.	Agreed	Corrected in line 129-30
#29		perhaps the citations have gotten off, it appears that citation 30 should be 26, for PAS2050?	Agreed	Corrected
#30		the PAS2050:2011 standard was used, which has been aligned with the GHG Protocol since the original version, but does it now also use the Scopes framework? If not, please use the categories set forth in the PAS2050 standard, and please use PAS2050 terminology throughout.	Agreed	Reworded at Lines 146-52
#31		Also, could clarify that Scope 3 emissions include those from waste management and final disposal, they are not just about upstream supply chain.	Agreed	Clarified lines 149-50
#32		Details on waste management for the RSCs at end of life should be described in the same place as for the SSC.	Agreed	Now included in same para.
#33		GHG Allocation, presumably upstream mass-based allocation for petrochemical refining and the like was not conducted by the authors...?	Agreed	GHG allocation for fuel is obtained from DEFRA database (well-to-wheel) as specified in Supplementary data now attached as

				BMJO online document.
#34		Please cite here in the Data Sources section what LCI data sets were used (i.e., PlasticsEurope)	Agreed	Specified in Supplementary data now attached as BMJO online document.
#35		differentiate between allocation done in the background data sets versus the foreground model. In this section, please detail exactly what the assumed sources of avoided emissions were (i.e., UK grid average electricity).	Agreed	Secondary databases made no mention of allocations in foreground or background, nor mentioned avoided emissions. Have now stated this in Limitations.
#36		please keep the units consistent as kg CO ₂ e, and not KgCo ₂ e.	Agreed	Changed to kg CO ₂ e throughout document.
\$37		Most of the Results section appears to be Methods details and should be moved to that section. Results can focus more on the contributions to GHG emissions for each product system shown in Figure 2.	Agreed	Several sentences/paras in Results now moved to Methods.
#38		Figure 1, 'Non-GHG Outputs' should be changed to 'Non-GHG emissions' or environmental burdens, following PAS2050.	Agreed	Fig 1 Exclusions box corrected
#39		In all results, figures, and tables, please use a consistent number of significant figures.	Agreed	All figures now to one decimal pint

#40	There is a quantitative uncertainty analysis done related to the procurement data but not the emissions data, if I am understanding it. It would be preferable to have a quantitative uncertainty analysis that applies to the full model and can be used to add error bars to Figure 2, for example	Agreed	New Table 2 depicts quantitation of lowest impact alternatives.
#41	The sensitivity analysis section on mfg focuses on electricity and the role of geography, but this is not the only energy type used in petrochemicals and polymers mfg, please clarify.	Agreed	Deleted. Replaced by quantitative sensitivity Analysis Table 2
#42	In the final sections of the paper, the authors use the word 'significantly' several times. Is this meant in the statistical sense, or do they simply mean that the quantities are large. Even if it is the latter, pls try to give readers as sense of the threshold of significance. Is a 20% reduction significant? What about a 5% reduction?	Agreed	Deleted or changed to "markedly". P statistics deleted from paper.
	Reviewer 3		
#43	My most serious criticism is that the main conclusion „Reusable devices achieve significant GHG reductions over single-use plastic devices (p12)“ is not supported in this generality by the results of the study. The major limitation of generality, in my view, arises partly from the specificity of the supply chain studied and partly from the large differences in	Agreed	Conclusion 1 corrected – see lines 435-437.

	<p>container size and its effect on production and transportation effort. A more appropriate (admittedly slightly exaggerated) conclusion would be that „Michigan-produced reusable devices of a volume of 18.8l result in significant reductions over the use of smaller disposable containers (6.7l) when the plastic is emissions-intensively manufactured in Saudi Arabia.“</p>		
#44			
#45	<p>The size of the containers, which seems to me most important for the magnitude of CO2 savings, is not considered at all in the sensitivity analysis. There may be reasons for both, but they are not mentioned and I am therefore skeptical about the actual significance of these figures</p>	Agreed	<p>Sensitivity Analysis expanded to include new Table 2 which includes a scenario of identical SSC size.</p>
#46	<p>The sensitivity analysis could be used to discuss both of those issues. At the moment, the influence of the carbon intensity of energy production in the country of production is mentioned, but its influence on the results and the consequences for the conclusions is not quantified.</p>	Agreed	<p>New Table 2 depicts quantitation of lowest impact alternatives of 4 major contributing processes.</p>

#47	<p>My second concern regards the description of the methods which is quite vague or not available (I did not have access to a methods appendix, apologies if one was provided). For example, for the sensitivity analysis above, it is simply stated „A sensitivity analysis revealed“ with little more information how that was done, what parameter ranges were tested or how plausible those are. To allow for the results to be reproduced, a methods appendix should be provided, ideally including the software code and the data in machine readable form to allow computational reproducibility. I have never understood what a good reason could be for providing data and information about a published study only „on request“.</p>	Agreed	<p>Quantitative</p> <ol style="list-style-type: none"> 1. sensitivity analysis now includes new Table 2. 2. Full method data now available in Supplementary Data table now available as BMJOpen online file.
#48	<p>Finally, I think the authors should make much greater effort to situate the study in the existing scientific literature. Currently, of the 52 references in the manuscript, by my count only 6 (!) are to peer-reviewed scientific studies.</p>	Agreed	<p>Nine peer-reviewed studies added in Intro paras 1-3 as per Reviewer 1 and 3 suggestions.</p>
#49	<p>Important typo/error: in the data table in row 13 (GHG of electricity for SSC mfg (UK)) it should say “Saudi Arabia“ instead of UK</p>	Agreed	<p>Clarified in Col 1 Row 13.</p> <p>Note.</p> <ul style="list-style-type: none"> • SSC bins are mfged in UK;

				SSC polymer is • mfged in Saudi Arabia.
#50		even if the conclusions need to be much more qualified and even if probably some of the limitations I raised cannot be addressed on a quantitative level, but only discussed more thoroughly.	Agreed	Conclusion now qualified. Quantitation used in Table 2.

VERSION 2 – REVIEW

REVIEWER	Matthew Eckelman
REVIEW RETURNED	29-Jun-2021

GENERAL COMMENTS	<p>The authors have responded to each of the comments raised by reviewers, and made corresponding revisions in the manuscript in most cases. I am still supportive of publication, but have a few remaining suggestions.</p> <ul style="list-style-type: none"> - On the sensitivity analysis, the documentation about exactly how each of the major assumptions was changed quantitatively occurs much later in the manuscript – ideally this would be presented with Table 2 or even earlier in the Methods. - The authors emphasize the advantages of their study in taking data from multiple trusts, but not much is done to take advantage of this. For example, it would be very interesting to see not just the total over all 47 trusts, but also to see variation among trusts, normalized by an indicator like finished admission episode, which the authors also mention but do not appear to use anywhere. The authors can also use the distribution of results to properly create a confidence interval when scaling up results to the total NHS. - Since the UK footprint has some results per FAE, the authors could compare these results for sharps bins to the total in order to answer the important question of how much sharps bins matter in the overall scheme. The last sentence of before the Conclusion section also attempts this, but there is a problem. The text reads that switching will provide an annual benefit, but this cannot be compared to an annual reduction where the baseline shifts each year, as the same interventions cannot be counted more than once. Providing some sort of context for the importance of sharps bins is helpful for readers, if it is possible to do here. Just presenting the absolute totals for 47 trusts will not mean much to most readers. - The discussion is full of results with a high degree of precision. Please keep an appropriate number of significant digits (I would think no more than 4, and 2 for percentages). - The best parts of the Discussion are when the narrative moves away from presenting all of the numerical results and comments on how the results should be interpreted. One aspect I would have liked to see is on what makes sharps containers similar or different to other medical devices that have been compared for reusable vs disposable options. For example, many reusable devices can be cleaned on-site – would that ever make sense for sharps containers, cutting down on transport?
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	- The RSC sensitivity analysis considers increasing lifespan, but I imagine it is much more likely that bins are switched before they reach 18 years, and moving in this direction should have much more influence on the relative results than lifetime extension. Actually, it would be helpful to include a calculation of break-even here – how many years of use of an RSC are necessary in order to make the switch worth it in terms of carbon emissions? I do not agree with the general Conclusion as written: “RSC lifespan has minimal effect on carbon footprint of RSC LCCF.” What if the reusable bin is only used once?
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REVIEWER	Peter-Paul Pichler Potsdam Institute for Climate Impact Research, Social Metabolism & Impacts
REVIEW RETURNED	05-Jul-2021

GENERAL COMMENTS	<p>I think the authors have addressed the most critical issues and the manuscript is much clearer overall. The results of the additional sensitivity analysis are very helpful.</p> <p>There is some issue with the introduction which I would recommend the authors go over again. In line 85/86 there seems to be something missing (maybe "countries"?). Also, the sentence starting in line 87 (In 2010...) seems redundant (and slightly conflicting) with the first paragraph above.</p> <p>I feel a little bit bad about the first sentence of the conclusion (l 363) which is very close to my somewhat exaggerated suggestion to qualify the conclusion in my initial review. It is correct, so if the authors are happy with that sentence I have no objections. However, if it is phrased this way mostly for my benefit, I would be happy to tone down the qualifications a little now that the sensitivity analysis already provides this additional context.</p> <p>I think it would be entirely justified to write that the switch from SSC to RSC investigated here led to substantial emissions reductions.</p> <p>I think this is now a tidy and well written article that provides a novel and relevant contribution to the literature. I recommend publication of the article.</p> <p>Outside of the formal review, I do have a final complaint though, which I simply want to state without expecting the authors to address before publication (!). It regards the publication of data and methods which I understand is handled very differently across disciplines (and sometimes generations of scientists). I do not at all understand the reasons for not publishing the computer code or EXCEL models that were used to arrive at the results of a scientific study. Keeping this from the scientific community reduces reproducibility and makes incremental self-correction basically impossible. All downside, without, as far as I can see, any upside at all. Also, data and result tables should be published in a machine readable format (CSV or Excel or whatever) to be useful to other scientists rather than as pdf tables.</p>
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VERSION 2 – AUTHOR RESPONSE

	Reviewer Comment	Author reply	Author action
	REVIEWER 2		
#1	On the sensitivity analysis, the documentation about exactly how each of the major assumptions was changed quantitatively occurs much later in the manuscript – ideally this would be presented with Table 2 or even earlier in the Methods.	Agreed	Results of sensitivity Analysis moved to “Results”. See Lines 260-288
#2	The authors emphasize the advantages of their study in taking data from multiple trusts, but not much is done to take advantage of this. For example, it would be very interesting to see not just the total over all 47 trusts, but also to see variation among trusts, normalized by an indicator like finished admission episode, which the authors also mention but do not appear to use anywhere. The authors can also use the distribution of results to properly create a confidence interval when scaling up results to the total NHS.	Agreed	Individual trust activity episodes (FAE, AEA, OPA) now included for all three indicators for all trusts to establish mean and 95% Confidence intervals for scaling to national NHS activity savings. See Lines 189-92; 234-38
#3	Since the UK footprint has some results per FAE, the authors could compare these results for sharps bins to the total in order to answer the important question of how much sharps bins matter in the overall scheme. The last sentence of before the Conclusion section also attempts this, but there is a problem. The text reads that switching will provide an annual benefit, but this cannot be compared to an annual reduction where the baseline shifts each year, as the same interventions cannot be counted more than once. Providing some sort of context for the importance of sharps bins is helpful for readers, if it is possible to do here. Just presenting the absolute totals for 47 trusts will not mean much to most readers.	Agreed	While it is not possible to accord a comparison between NHS standard Activity Episodes and the “proportion” of activities that sharps container exchanges represent, we have addressed the Reviewer’s request in three ways: <ol style="list-style-type: none"> 1. The impact of national adoption of RSC is apportioned (3.6%) to NHS annual GHG reduction targets for each of next 10 years. See Lines 398-402. 2. Using the three Activity Episodes (FAE, AEA, OPA), a mean kgCO₂e per 1000 AE was calculated so that all trusts can determine their own GWP potential savings/year against their own activity levels. See Lines 235-8 3. Using Total Fill Line Litres of sharps containers (an easily-determined figure) we calculated kgCO₂e per 1000 FLL – which again enables trusts to determine their own GWP potential savings/year against their own sharps container usage levels. See Lines 234-5

#4	The discussion is full of results with a high degree of precision. Please keep an appropriate number of significant digits (I would think no more than 4, and 2 for percentages).	Agreed	Amended as follows: <ul style="list-style-type: none"> • All impact factors <10 in Supplementary data now given at 3 significant digits. • We believe percentages at 2 decimal places impede ease of reading and have no effect on outcome as Excel automatically uses >4 decimal places in all calculations. • All figures in body now at 1 decimal point (unless references state whole %). We will defer to Editorial policy on this matter.
#5	The best parts of the Discussion are when the narrative moves away from presenting all of the numerical results and comments on how the results should be interpreted. One aspect I would have liked to see is on what makes sharps containers similar or different to other medical devices that have been compared for reusable vs disposable options. For example, many reusable devices can be cleaned on-site – would that ever make sense for sharps containers, cutting down on transport?	Agreed	Differences in patient medical devices and sharps container safety devices, are explained, as well as clarifying the reasons why SSC cannot be used as RSC, and why RSC processing is not feasible onsite by trusts. See Lines 348-60.
#6	The RSC sensitivity analysis considers increasing lifespan, but I imagine it is much more likely that bins are switched before they reach 18 years, and moving in this direction should have much more influence on the relative results than lifetime extension. Actually, it would be helpful to include a calculation of break-even here – how many years of use of an RSC are necessary in order to make the switch worth it in terms of carbon emissions? I do not agree with the general Conclusion as written: “RSC lifespan has minimal effect on carbon footprint of RSC LCCF.” What if the reusable bin is only used once?	Agreed	The average age of RSC has never been <18 years, but if it were to occur, we agree it would impact results. To amend, we: <ul style="list-style-type: none"> • recalculated RSC GWP at a 1yr lifespan and at breakpoint – see lines 283-88 and Table 2 last row. • Reworded conclusion on lifespan – see Lines 283-88 and 308-11.
REVIEWER 3			
#1	In line 85/86 there seems to be something missing (maybe "countries"?). Also, the sentence starting in line 87 (In 2010...) seems redundant (and slightly conflicting) with the first paragraph above.	Agreed	<ul style="list-style-type: none"> • “countries” inserted Line 12. • Intro para1 pertains to UK gov, 2nd para to NHS. Have clarified Line 13-15 starting “In 2010...”
#2	I feel a little bit bad about the first sentence of the conclusion (I 363) which is very close to my somewhat exaggerated suggestion to qualify the conclusion in my initial review. It is correct, so if the authors are happy with that sentence I have no objections. However, if it is phrased this way mostly for my benefit, I would be happy to tone down the qualifications a little now that the	Agreed	First conclusion amended. See Lines 407-8.

	<p>sensitivity analysis already provides this additional context. I think it would be entirely justified to write that the switch from SSC to RSC investigated here led to substantial emissions reductions.</p>		
#3	<p>(Reviewer post-comment only – no action required) Outside of the formal review, I do have a final complaint though, which I simply want to state without expecting the authors to address before publication (!). It regards the publication of data and methods which I understand is handled very differently across disciplines (and sometimes generations of scientists). I do not at all understand the reasons for not publishing the computer code or EXCEL models that were used to arrive at the results of a scientific study. Keeping this from the scientific community reduces reproducibility and makes incremental self-correction basically impossible. All downside, without, as far as I can see, any upside at all. Also, data and result tables should be published in a machine readable format (CSV or Excel or whatever) to be useful to other scientists rather than as pdf tables.</p>	Agreed	<p>For transparency, every unit process, and associated impact factor value and result, is listed in Supplementary Data doc.</p> <p>We have a higher level of transparency than most recent papers – which used commercial sources and did not reveal all values.</p> <p>Also, our Excel workbook took 18 months to purpose-build and is our IP – it enables alternative scenarios for Sensitivity Analysis to be achieved more easily – but all values and sources are tabled in Supp Data doc.</p>

VERSION 3 – REVIEW

REVIEWER	Matthew Eckelman
REVIEW RETURNED	16-Aug-2021

GENERAL COMMENTS	<p>The authors have addressed all of the comments from earlier rounds, including normalizing the results by FAE and computing confidence intervals. There are still results that are presented with many significant digits that may give a false sense of precision, but this choice is up to the authors. Other than this minor point, I am supportive of publication.</p>
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