

PEER REVIEW HISTORY

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ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Barriers and facilitators to adopting high value practices and de-adopting low value practices in Canadian Intensive Care Units: A multi method study |
| AUTHORS | Sauro, Khara; Bagshaw, Sean; Niven, Daniel; Soo, Andrea; Brundin-Mather, Rebecca; Parsons Leigh, Jeanna; Cook, Deborah; Stelfox, Henry |

VERSION 1 – REVIEW

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| REVIEWER | Dr. MMC van Mol Erasmus Medical Centre, Rotterdam, the Netherlands |
| REVIEW RETURNED | 31-May-2018 |

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| GENERAL COMMENTS | <p>Dear authors, Thank you for this interesting and well-written manuscript. The study adds valuable knowledge on facilitators and barriers in implementing new interventions or treatment options. I do not have any substantive comments; however, some textual suggestions:</p> <ul style="list-style-type: none">- page 6, rule 12, the abbreviations LMWH and VTE have been used for the first time without full description there. In line 33/34 it is the other way around. Please change this.- page 23, a sentence seemed not complete "Both adoption and de-adoption of the were less likely for patients with greater severity of illness and varied across institutions".- the description on figure 1 is not uniform in all places. <p>kind regards</p> |
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| REVIEWER | Peter Nydahl Nursing Research, University Hospital of Schleswig-Holstein, Campus Kiel, Germany |
| REVIEW RETURNED | 09-Jul-2018 |

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| GENERAL COMMENTS | <p>The authors conducted a multi method research about the implementation and de-implementation of two current practices in critical care. The manuscript (reads as if it had been through peer-review/s already, because it) is extremely well written, highly relevant for implementation research and should be considered for publication after revision of minor concerns.</p> <p>Abstract: fine</p> |
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| | <p>Strength and Limitation: the third limitation “A limitation of this study is related to the survey...” is a general limitation. Maybe the author would like to describe this limitation more specific?</p> <p>Section introduction is written well and clear. Usually, the aim/purpose of the study is described in the last paragraph and sentence of the introduction. Here it starts in section methods. I suggest to move this part above methods</p> <p>The authors asked participants for current use of strategies to encourage the adoption and de-adoption, and the results of this question are listed in figure 4. Were there any major differences between the nine ICUs? Please report.</p> <p>Survey/Table 1: were the items of the survey (LMWH, UFH etc) listed in the same order as in table1, or in a different order (maybe in a randomized order)? Please specify.</p> <p>Figures are doubled (p33-40)</p> <p>Discussion: detailed Conclusion: based on results Literature: fine Tables: fine Figures, fine, but doubled, wrong numbers</p> <p>As a note: this is one of the seldom manuscripts using the terms “multi-disciplinary and multi-professional” correctly. This is very much appreciated!</p> |
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| REVIEWER | George Anesi University of Pennsylvania, Philadelphia, PA, USA |
| REVIEW RETURNED | 27-Sep-2018 |

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| GENERAL COMMENTS | <p>This manuscript reports a mixed-methods study to assess prevalence of and barriers and facilitators to adoption of high-value practices and de-adoption of low-value practices in the ICU. The study looked at LMWH VTE prophylaxis (a high-value practice) and albumin for fluid resuscitation (a low-value practice). The two study approaches included a retrospective audit of practice patterns and a cross-sectional qualitative survey of ICU clinicians to assess knowledge of guiding evidence and barriers and facilitators to adoption/de-adoption. Notable findings include: (1) there was a perception that best practices were followed despite non-ideal adherence/non-adherence rates; (2) insufficient knowledge/understanding was the primary barrier to adoption; (3) clinician leader preference was the primary barrier to de-adoption; and (4) on-site education and order sets were the primary facilitators for changing practice (adoption and de-adoption). This study has a number of strengths. The study included 7000 ICU admissions from 9 ICUs in 2 large Canadian cities with a shared provincial health services provider and a common electronic health record. It importantly assessed clinician beliefs regarding barriers and facilitators. The authors importantly recognize that there may be differences in mechanisms and solutions to adoption versus de-adoption. The authors designed nuanced inclusion/exclusion criteria to create appropriate adoption and de-adoption cohorts. They also deployed rigorous survey development based on previous work and appropriately pilot tested resulting in high response rate for survey of clinicians.</p> |
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| | <p>The study has some important limitations. Most significantly, surveys in general are far more limited than semi-structured interviews or ethnography for understanding complex behaviors such as adoption/de-adoption of practices in a complex healthcare/ICU environment. The results should therefore be viewed as hypothesis generating rather than truly explanatory.</p> <p>Major Comments:</p> <ol style="list-style-type: none"> 1. As mentioned above, surveys are far more limited than semi-structured interviews or ethnography for understanding complex behaviors and associated results should therefore be viewed as hypothesis generating rather than truly explanatory. The authors propose follow-up implementation work from this study. Can the authors comment on whether there would be benefit in more explanatory semi-structured interviews/ethnography work either in addition to or before subsequent implementation studies? 2. Can the authors comment further on the poorer performance for patients with higher acuity? Does this reflect unintentional breakdown in standard of care during times of strain due to patient complexity? Or purposeful departures as clinicians deal with sequelae of severe critical illness and less clinical improvement/clinical deterioration (i.e., perceived bleeding risk, refractory third-spaced volume, etc.) not fully captured in their exclusions? <p>Minor Comments:</p> <ol style="list-style-type: none"> 1. Figure 2 is titled "Figure 1". 2. The shading/color scheme in Figure 2 is difficult to follow. 3. Is it possible to include either as part of Figure 2 or just in the text what percent of ICU days patients received any chemical VTE prophylaxis (i.e., LMWH or UFH)? 4. Re Figure 1: Stylistic, but might recommend that the exclusion boxes be placed between the "ICU admissions" box and the two cohort boxes, and that the direction of their arrows be reversed to be consistent with the direction of patient flow. |
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| REVIEWER | Nancy Morden Microsoft Healthcare NExT |
| REVIEW RETURNED | 16-Oct-2018 |

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| GENERAL COMMENTS | <p>Summary: This study aims to explore patterns of one high-value service and one low-value service, as well as the care team's perception of factors influencing patterns of adoption and de-adoption. This "multi-method observational study included: 1) a retrospective cohort study of patients admitted to ICUs to describe current VTE prophylaxis and fluid resuscitation practices, and 2) a cross-sectional survey of ICU healthcare providers to examine: knowledge of evidence underpinning these two practices, and perceived barriers and facilitators to adopt LMWH for VTE prophylaxis and de-adopt albumin for fluid resuscitation."</p> <p>The authors studied practice patterns across 9 ICUs in the two largest cities in Alberta, Canada. They describe this as a population study because they include all healthcare providers engaged in care in these study ICUs and patients admitted to the</p> |
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| | <p>ICUs in 2014, all without a VTE prophylaxis contraindication and all patients without an evidence-based indication for albumin fluid resuscitation.</p> <p>Main impression: This is an ambitious paper on an important and valuable topic. I would like to see this published as well as the follow-on studies examining the effect of interventions informed by this work. In its current state the paper suffers from a wealth of data and a shortage of focus and filtering. The authors have tried and likely can get this right with a revision.</p> <p>The authors have:</p> <ol style="list-style-type: none"> 1. Baseline rates of high- and low-value care overall and across 9 ICUs in the province. 2. Patient level predictors of low-value/high-value care modeled two ways (I believe): at the patient day level (LMWH) and at the patient level (Albumin). 3. Clinical team members' survey of: 1. practice perceptions (how good are we at getting this care right?) and 2. evidence-based practice barriers (what are the barriers to evidence-based practice?). <p>These important, broad and disparate data are presented collectively in the paper. The result is confusing despite the overall value of the work. As I note in my review, I find it a bit scattered and conclude this is at least, in part, a response to the diverse directions the paper attempts to take.</p> <p>I suggest a revision that clearly focuses on the survey and perhaps setting specific variation if insight can be gleaned from that. Patient-level factors associated with low- or high-value care, I believe are tangential to the team's main focus – barriers to adoption of high value care and de-adoption of low-value care.</p> <p>Section Review:</p> <p>Abstract: Good overall, except this phrase seems odd: "perfection was compromised to optimize the practicality of the survey." That jumps out at me as odd and suggests compromises that are not otherwise discussed.</p> <p>Introduction: The sentence about terminology is not linked to work or justified. It stands out as misplaced as currently written. Could add context about how people may know de-adoption by many other terms, but for purposes of this paper that is the term you're electing to use.</p> <p>Paragraph two of the introduction is long. Consider the overarching introduction structure: 1. What is the general problem? 2. What is the specific problem? 3. What do we add? (dividing the introduction into three paragraphs). (see Welch HG. Preparing manuscripts for submission to medical journals: the paper trail. <i>Effective clinical practice</i> : ECP. 1999;2(3):131-7.)</p> <p>Consider moving information on choice of measures (LWMH and Albumin) to methods.</p> <p>Methods: The authors used clinical data to identify ICU admitted patients without contraindication to LMWH and those without indication for Albumin. They collected a broad range of patient</p> |
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| | <p>level variables. As noted above, the modeling of predictors of low-value care and high-value care is interesting but feels like a separate study, and, in fact, the model output is relegated to supplemental material. I cannot quite figure out how to incorporate this unless it is being used to control for patient differences in the examination of variation across ICUs.</p> <p>The authors model outcomes two ways, patient day and patient. Why?</p> <p>Regarding survey methods. I am not a survey expert. It may be worth having such a person review this work.</p> <p>Results: The authors illuminate practice patterns, and document less adherence to evidence than teams perceive. They also reveal perceived barriers to evidence adoption/de-adoption, which these respondents attribute to both activities.</p> <p>It is not clear if the survey instrument itself may have led to similar factors being identified as influential in the two practices because the same plausible options were offered for each. It is like giving the same multiple-choice options on two questions and discovering similar answers. It is also not clear to me that the list of options reflects what we understand to be the range of barriers for adoption/de-adoption obstacles.</p> <p>More information might have emerged from open-ended questions, though that would require qualitative methods. What was revealed in the “other” option?</p> <p>A survey expert may be helpful here. That is area of not my expertise.</p> <p>The authors find “When controlling for demographic and site-level factors, there were no differences in adoption or de-adoption based on patient age, sex, or comorbidity (Supplemental Content 4).” Again this seems interesting but I struggle with how to include in a paper about opinions on practices.</p> <p>The authors present some information that seems of low relevance to the stated aim of the work (e.g., “The objective of this study was to describe illustrative example practices of the adoption of high value practices and the de-adoption of low value practices in the ICU.”) For example; Mortality rate and LOS. Was there a hypothesis around these measures?</p> <p>That said, the fact that only severity of illness reliably predicts low-value care could inform future interventions. I also find it interesting that, controlling for patient factors, use of LMWH and Albumin varied significantly across the ICUs. I think this point warrants a bit more discussion.</p> <p>Regarding tables and figures, I think some of the information in the supplement more valuable than its placement suggests, and, conversely some information in the main paper, less valuable than its placement suggests. This, of course, depends on the focus of the paper.</p> |
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| | <p>Table 1 effectively communicate the findings, but perhaps in more detail than is needed. I wonder about a table or chart that tells me % of each group identifying correctly the most effective treatment. Then details could be in supplementary material.</p> <p>Figure 1 (patient flow) seems tangential to the point of the paper. It adds little since the design is simple and well described in the text.</p> <p>Figure 1 (VTE proph. choice) seems of some value but is not key to the paper, especially as presented by day of stay, unless there is a hypothesis about LOS and treatment choice. It would be sufficient to report relationships between LOS and VTE prophylaxis product chosen if this seems important. A granular chart is not needed.</p> <p>Rather than seeing treatment by LOS, I would like to know how many patients crossed treatment groups. In my mind, that begins to let us understand more about the relationship between severity of illness and low-value care than is reported. I think there is something here – that desperate situations lead clinicians to deviate from evidence or simply try something else, perhaps. I wonder if sicker patients got other VTE prophylaxis or crystalloid followed by albumin (in response to lack of response to the former). I am not suggesting the authors explore this for this paper, but perhaps rephrase the conclusion about severity of illness prompting “conservative” decisions. I like the suggestion to consider severity of illness in study of decision making but the path to the observed care matters – did clinicians reach FIRST for non-evidence base care, or was non-evidence based care a response to failed improvement?</p> <p>The role-specific survey (MD, nurse, pharmacist) is interesting, as are the model results that communicate numerically the relationship between variables and treatment decisions. Currently no tables in the main paper communicate this and only OR is reported in the text.</p> <p>I would like to see Suppl. Table 5 in the manuscript or at least discussed a bit more.</p> <p>Inclusion of the method details and instrument in the supplemental materials is helpful.</p> <p>Smaller items</p> <ol style="list-style-type: none"> 1. The topic sentences in the discussion are a bit unwieldy, distracting from the thoughtful consideration of the study’s implications. For example: “Knowledge translation (KT) interventions; strategies to improve the synthesis, dissemination, exchange, and application of evidence to improve health, tailored to the specific barriers and facilitators of an innovation and the local context are more likely to effect change.” 2. I am not sure this sentence communicates what you intend. “Both observations suggest that clinicians may employ conservative decision-making when caring for sicker patients.” Does conservative mean evidence-ignoring? 3. The authors mention but do not explore order sets as a source of deviation from evidence and/or a source of variation across ICUs. They mention that LMWH use increased over the study |
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| | period, but we do not know why – this seems important to think about. A comment on order sets would be helpful if possible; it may not be. |
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| REVIEWER | Howard, David H. Emory University, Atlanta Georgia |
| REVIEW RETURNED | 16-Oct-2018 |

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| GENERAL COMMENTS | <p>The paper describes use of two practices, one evidenced-based the other not, in intensive care units in two Canadian cities and reports the results of a survey that assesses the factors that may promote or retard evidenced-based practice. The survey data are interesting. The data on the use of two practices are helpful for putting the survey results in context but are otherwise not particularly interesting. My main comment is that the paper should not use the terms “adoption” and “de-adoption” when describing a point-in-time analysis.</p> <p>The paper describes use of two practices at a point-in-time. I think it is misleading to say that the study describes adoption or de-adoption. There is an element of temporality implicit in these concepts.</p> <p>Before you quote the statistic that practice change can take up to 17 years, you might want to take a look at this paper: Howard DH. Evidenced-based claims about evidence. Medical Decision Making: Policy & Practice 2017;2(2):2381468317734527. In short: There isn't good evidence behind the 17 year statistic.</p> <p>In the last paragraph in the Introduction, one sentence states “were chosen by a network of medical-surgical ICUs...” and the last sentence states “Patient and family representatives, frontline providers, and decision-makers considered....chose low molecular”. These seem to conflict. Or at least belabor a point that could be made quickly and without fuss.</p> <p>Under Results, Audit of Current Practices, I didn't know how to interpret the statement that ICU mortality was 14.1% and hospital mortality was 21.0%. Does hospital mortality include deaths that occur in the hospital after a patient is discharged from the ICU?</p> <p>In Table 1, it would aid interpretation if you could indicate the right or preferred answer to each question (if that is possible).</p> |
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VERSION 1 – AUTHOR RESPONSE

RESPONSE TO REVIEWER 1

Thank you for this interesting and well-written manuscript. The study adds valuable knowledge on facilitators and barriers in implementing new interventions or treatment options.

Response: We thank the Reviewer for the time they took to review this manuscript.

I do not have any substantive comments; however, some textual suggestions:

1. Page 6, rule 12, the abbreviations LMWH and VTE have been used for the first time without full description there. In line 33/34 it is the other way around. Please change this.

Response: The paragraph in which this error occurred has been deleted in response to another Reviewer's comment. The first occurrence of LMWH, UFH and VTE is now on page 6, paragraph 1: "The objective of this study was to describe illustrative example practices of the adoption of a high value practice (use of low molecular weight heparin [LMWH] instead of unfractionated heparin [UFH] for venous thromboembolism prophylaxis [VTE] and the de-adoption of a low value practice (albumin for fluid resuscitation) in the ICU. The results of this study prompted a subsequent implementation study to improve these two practices. The audit data identified important opportunities to improve clinical care, and the perceived barriers and facilitators identified in the survey were used to inform the development of interventions."

2. Page 23, a sentence seemed not complete "Both adoption and de-adoption of the were less likely for patients with greater severity of illness and varied across institutions".

Response: This has been corrected.

Page 24, paragraph 2: "In conclusion, our study provides several insights into similarities and differences between adoption of high value practices and de-adoption of low value practices. Both adoption and de-adoption of the illustrative example practices did not reflect healthcare providers' knowledge of the evidence. The use of best practices for both illustrative examples practices were less likely for patients with greater severity of illness and varied across institutions. We found that perceived barriers and facilitators are more similar than different between adoption and de-adoption, which suggests existing behavior change frameworks for adopting high value practices may also be applicable for de-adopting low value practices."

3. The description on figure 1 is not uniform in all places.

Response: The description of figure 1 was likely inconsistent due to mislabeling Figure 2 as Figure 1 in the previous version. Both of these figures have been moved to supplemental content (supplemental content 4 and 5) and the description in the text is now consistent with the figures.

RESPONSE TO REVIEWER 2

The authors conducted a multi method research about the implementation and de-implementation of two current practices in critical care. The manuscript (reads as if it had been through peer-review/s already, because it) is extremely well written, highly relevant for implementation research and should be considered for publication after revision of minor concerns.

Abstract: fine

Response: We thank the Reviewer for taking the time to review our manuscript and providing feedback.

Strength and Limitation: the third limitation "A limitation of this study is related to the survey..." is a general limitation. Maybe the author would like to describe this limitation more specific?

Response: We have provided more clarity and detail around this limitation.

Page 3, third bullet in the Strengths & Limitations section: "The survey used to assess barriers and facilitators of the two illustrative practices was derived from a validated survey instrument. It was simple and designed to garner a representative perspective from all provider professions and therefore captured key concepts, but not granular data."

Section introduction is written well and clear. Usually, the aim/purpose of the study is described in the last paragraph and sentence of the introduction. Here it starts in section methods. I suggest to move this part above methods.

Response: We have revised the manuscript to be more in-line with convention. The objective: "The objective of this study was to describe illustrative example practices of the adoption of a high value practice (use of low molecular weight heparin [LMWH] instead of unfractionated heparin [UFH] for

venous thromboembolism prophylaxis [VTE] and the de-adoption of a low value practice (albumin for fluid resuscitation) in the ICU. The results of this study prompted a subsequent implementation study to improve these two practices. The audit data identified important opportunities to improve clinical care, and the perceived barriers and facilitators identified in the survey were used to inform the development of interventions.” is now on page 6, paragraph 1.

The authors asked participants for current use of strategies to encourage the adoption and de-adoption, and the results of this question are listed in figure 4. Were there any major differences between the nine ICUs? Please report.

Response: These data were difficult to analyze because of the number of sites and barriers and facilitators; as well, many healthcare providers work at multiple sites. When participants that worked at multiple sites were counted twice (once for each site where they worked) there were no major differences between the ICUs included in the study. The barriers and facilitators with the most variation related to computerized reminders and QI networks and teams for both adoption of high value practices and de-adoption of low value practices.

Survey/Table 1: were the items of the survey (LMWH, UFH etc) listed in the same order as in table1, or in a different order (maybe in a randomized order)? Please specify.

Response: The survey items were presented in the same order presented in Table 2 (previously table 1). A footnote indicating that the order of the survey items were the same as presented in Table 2 has been included with Table 2. The survey is also available in supplemental content 2.

Page 18, Table 2 footnote.1The order of the survey items are as presented in this table.

Figures are doubled (p33-40)

Response: This error has been corrected and the previous Figure 1 and 2 are now supplemental content 4 and 5.

Discussion: detailed; Conclusion: based on results; Literature: fine; Tables: fine

Figures, fine, but doubled, wrong numbers. As a note: this is one of the seldom manuscripts using the terms “multi-disciplinary and multi-professional” correctly.

This is very much appreciated!

Response: The figure numbers have been corrected (are now Supplemental Content 4 and 5).

RESPONSE TO REVIEWER 3

This manuscript reports a mixed-methods study to assess prevalence of and barriers and facilitators to adoption of high-value practices and de-adoption of low-value practices in the ICU. The study looked at LMWH VTE prophylaxis (a high-value practice) and albumin for fluid resuscitation (a low-value practice). The two study approaches included a retrospective audit of practice patterns and a cross-sectional qualitative survey of ICU clinicians to assess knowledge of guiding evidence and barriers and facilitators to adoption/de-adoption. Notable findings include: (1) there was a perception that best practices were followed despite non-ideal adherence/non-adherence rates; (2) insufficient knowledge/understanding was the primary barrier to adoption; (3) clinician leader preference was the primary barrier to de-adoption; and (4) on-site education and order sets were the primary facilitators for changing practice (adoption and de-adoption).

This study has a number of strengths. The study included 7000 ICU admissions from 9 ICUs in 2 large Canadian cities with a shared provincial health services provider and a common electronic health record. It importantly assessed clinician beliefs regarding barriers and facilitators. The authors importantly recognize that there may be differences in mechanisms and solutions to adoption versus de-adoption. The authors designed nuanced inclusion/exclusion criteria to create appropriate

adoption and de-adoption cohorts. They also deployed rigorous survey development based on previous work and appropriately pilot tested resulting in high response rate for survey of clinicians. Response: We thank the Reviewer for reviewing this manuscript and providing feedback to strengthen it.

The study has some important limitations. Most significantly, surveys in general are far more limited than semistructured interviews or ethnography for understanding complex behaviors such as adoption/de-adoption of practices in a complex healthcare/ICU environment. The results should therefore be viewed as hypothesis generating rather than truly explanatory.

Major Comments:

1. As mentioned above, surveys are far more limited than semi-structured interviews or ethnography for understanding complex behaviors and associated results should therefore be viewed as hypothesis generating rather than truly explanatory. The authors propose follow-up implementation work from this study. Can the authors comment on whether there would be benefit in more explanatory semi-structured interviews/ethnography work either in addition to or before subsequent implementation studies?

Response: We agree with the Reviewer that qualitative methods are a strong method for understanding barriers and facilitators, especially when combined with quantitative methods. We chose to use this quantitative survey for two reasons:

1) We modeled our survey on a survey previously used to successfully evaluate barriers and facilitators for the use of best VTE prophylaxis practices among critically ill patients with the objective of designing implementation interventions (Cook et al. J Crit Care, 2014).

2) Our second reason was more practical – we wanted to efficiently link implementation interventions to barriers and facilitators for the larger implementation study. While it may have been imperfect for garnering detailed information about the barriers and facilitators for adopting LMWH and de-adopting albumin it was a practical approach for designing implementation interventions.

This was the first phase of a larger study; in the next phase of the larger study we conducted a process evaluation, which used semi-structured interviews (informed by a quantitative survey similar to that used in this study) to gain a better understanding of the interventions and if they were successful in overcoming the barriers and leveraged the facilitators identified in this study.

In order to address this limitation, we have revised the limitation section in the discussion. Page 23, paragraph 3 to page 24, paragraph 1: “The survey was purposefully designed to be simple and accessible to garner a representative perspective from all provider professions and therefore captured key concepts, but not granular data. Nevertheless, the survey has been successfully used for a similar purpose by others;30 was reliable and reported to have good clinical sensibility. Alternative methodologies such as qualitative analyses of semi-structured interviews may have allowed for more in depth exploration of barriers and facilitators to adopting LMWH and de-adopting albumin.”

2. Can the authors comment further on the poorer performance for patients with higher acuity? Does this reflect unintentional breakdown in standard of care during times of strain due to patient complexity? Or purposeful departures as clinicians deal with sequelae of severe critical illness and less clinical improvement/clinical deterioration (i.e., perceived bleeding risk, refractory third-spaced volume, etc.) not fully captured in their exclusions?

Response: We agree with the Reviewer’s hypothesis regarding the underlying reason for our findings. Our study cannot determine whether more complex and acutely ill patients received less evidence-based care as a result of a conscious decision to use conservative treatment or an unintentional oversight. Both are reasonable hypotheses. We have expanded the discussion of this finding.

Page 21, paragraph 1 to page 22, paragraph 1: “Furthermore, confidence in applying new evidence in clinical practice may be particularly challenging in the care of severely ill patients. This hypothesis is supported by two of our findings: 1) the use of LMWH for VTE prophylaxis and not using albumin for fluid resuscitation was inversely associated with severity of patient illness and 2) the use of LMWH and not using albumin increased as the patient became more stable (over ICU stay). Potential hypotheses to explain these observations include that clinicians may employ conservative decision-making (use more familiar practices) or unintentionally neglect to use best practices when caring for sicker patients, but this need further exploration.”

Minor Comments:

1. Figure 2 is titled “Figure 1”.

Response: This error has been corrected, although Figure 1 and 2 are now supplemental content 4 and 5, respectively.

2. The shading/color scheme in Figure 2 is difficult to follow.

Response: The colours in Figure 2 have been revised to improve clarity. Figure 2 is now supplemental content 5.

3. Is it possible to include either as part of Figure 2 or just in the text what percent of ICU days patients received any chemical VTE prophylaxis (i.e., LMWH or UFH)?

Response: We strategically chose not to report the proportion of ICU days where either LMWH or UFH were given because the objective of the study was to examine the use of LMWH over UFH. We found that either LMWH or UFH were administered on 83.9% of eligible patient days. If the Editor or Reviewer feels that this information is important to include we will be happy to revise accordingly.

4. Re Figure 1: Stylistic, but might recommend that the exclusion boxes be placed between the “ICU admissions” box and the two cohort boxes, and that the direction of their arrows be reversed to be consistent with the direction of patient flow.

Response: We agree with the Reviewer and have made the suggested change to Figure 1, which is now supplemental content 4.

RESPONSE TO REVIEWER 4

Summary: This study aims to explore patterns of one high-value service and one low-value service, as well as the care team’s perception of factors influencing patterns of adoption and de-adoption. This “multi-method observational study included: 1) a retrospective cohort study of patients admitted to ICUs to describe current VTE prophylaxis and fluid resuscitation practices, and 2) a cross-sectional survey of ICU healthcare providers to examine: knowledge of evidence underpinning these two practices, and perceived barriers and facilitators to adopt LMWH for VTE prophylaxis and de-adopt albumin for fluid resuscitation.”

The authors studied practice patterns across 9 ICUs in the two largest cities in Alberta, Canada. They describe this as a population study because they include all healthcare providers engaged in care in these study ICUs and patients admitted to the ICUs in 2014, all without a VTE prophylaxis contraindication and all patients without an evidence based indication for albumin fluid resuscitation.

Main impression: This is an ambitious paper on an important and valuable topic. I would like to see this published as well as the follow-on studies examining the effect of interventions informed by this work. In its current state the paper suffers from a wealth of data and a shortage of focus and filtering. The authors have tried and likely can get this right with a revision.

The authors have:

1. Baseline rates of high- and low-value care overall and across 9 ICUs in the province.

2. Patient level predictors of low-value/high-value care modeled two ways (I believe): at the patient day level (LMWH) and at the patient level (Albumin).

3. Clinical team members' survey of: 1. practice perceptions (how good are we at getting this care right?) and 4. evidence-based practice barriers (what are the barriers to evidence-based practice?). Response: We thank the Reviewer for taking time to thoroughly review the manuscript and for providing suggestions for improving the manuscript.

These important, broad and disparate data are presented collectively in the paper. The result is confusing despite the overall value of the work. As I note in my review, I find it a bit scattered and conclude this is at least, in part, a response to the diverse directions the paper attempts to take. I suggest a revision that clearly focuses on the survey and perhaps setting specific variation if insight can be gleaned from that. Patient-level factors associated with low- or high-value care, I believe are tangential to the team's main focus – barriers to adoption of high value care and de-adoption of low-value care.

Response: The Reviewer raises concerns about the cohesion of data presented in the manuscript. As the Reviewer points out, there is a wealth of data and while we made a concerted effort to present the data as simply as possible, we appreciate that it may still have been too much.

We have revised the manuscript trying to keep the objective of our study (to describe two illustrative practices as the foundation for future studies examining the effect of implementation strategies to optimize VTE prophylaxis and fluid resuscitation) in the forefront. Based on our objective we believe that it is important to not only identify the perceived barriers and facilitators to the adoption of LMWH and de-adoption of albumin by healthcare providers but also describe the current clinical practices of these two illustrative practices to glean insight into their respective use (e.g., more acute patients are less likely to get evidence-based medicine). We also believe presenting the current clinical practice for VTE prophylaxis and fluid resuscitation adds useful context for the results of the survey. For these reasons we have elected to keep the audit of current practices but have made 3 important changes to address the Reviewer's concern.

1) We have moved what was previously Figure 2 (LMWH use) to the supplemental content and moved what was previously supplemental content 7 and 8 as a two-panel figure into the main manuscript (now Figure 2).

2) We streamlined the description of the findings of the audit within the results section.

Page 12, paragraph 2 to page 13, paragraph 2: "There were 6,946 ICU admissions during the study period, from 6,299 unique patients. Patient characteristics are presented in Supplemental Content 3.

The adoption cohort consisted of 4,931 admissions (71.0% of all admissions) without a contraindication to pharmacological VTE prophylaxis, and the de-adoption cohort consisted of 6,467 admissions (93.1%) without a potential indication for albumin (Supplemental Content 4).

During the ICU stay LMWH was given on 38.7% of ICU days, UFH on 45.3% of ICU days and mechanical prophylaxis (exclusive of pharmacological prophylaxis) on 7.7% of ICU days. The type of VTE prophylaxis administered varied throughout patients' ICU stay; administration of mechanical devices and UFH decreased over the course of the ICU stay while administration of LMWH increased (Supplemental Content 5).

6,804 units of albumin were administered to 20.0% of the 6,467 admissions without documented liver disease or receipt of plasma exchange. Among those receiving at least 1 unit of albumin, the median number of units per patient was 3 (IQR=1.0-6.0). Albumin was administered on 6.5% of ICU days.

When controlling for demographic and site-level factors, the odds of receiving LMWH for VTE prophylaxis and not receiving albumin for fluid resuscitation were significantly lower for those patients with higher severity of illness (APACHE II score). The odds of receiving LMWH for VTE prophylaxis

were significantly higher for patients with non-surgical admissions compared to those with elective surgical admissions (odds ratio = 1.34 (95% confidence interval 1.08-1.66); Table 1). There were significant differences in the odds of using LMWH for VTE prophylaxis, and not using albumin for fluid resuscitation across ICUs (Supplemental Content 6), and when controlling for patient-level factors some of these differences persisted especially with regards to the use of LMWH for VTE prophylaxis (Table 1)."

3) Revised the discussion so it is primarily focused on the survey results, except for findings that directly support the survey data (knowledge of best practices and potential patient-level factors that may determine the use of best practices).

Section Review:

Abstract: Good overall, except this phrase seems odd: "perfection was compromised to optimize the practicality of the survey." That jumps out at me as odd and suggests compromises that are not otherwise discussed.

Response: We agree that this was an odd phrase and have modified the paragraph to be more specific and clearer. This change can be found in the strengths and limitations section.

Page 3, third bullet in the Strengths & Limitations section: "The survey used to assess barriers and facilitators of the two illustrative practices was derived from a validated survey instrument. It was simple and designed to garner a representative perspective from all provider professions and therefore captured key concepts, but not granular data."

Introduction: The sentence about terminology is not linked to work or justified. It stands out as misplaced as currently written. Could add context about how people may know de-adoption by many other terms, but for purposes of this paper that is the term you're electing to use.

Response: We have revised this paragraph to improve the flow.

Page 5, paragraph 2: "A growing body of evidence has evaluated barriers and facilitators for adopting high value practices (effective at improving outcomes).³⁻⁶ Substantially less is known about the barriers and facilitators for de-adopting low value practices (ineffective at improving outcomes or harmful), and how they compare to those for adopting high value practices.^{7,8} De-adoption, also known by several other terms such as disinvestment and de-implementation,⁷ is the discontinuation of a practice that has been previously adopted.⁹ Some have suggested that the adoption of high value practices and de-adoption of low value practices involves similar processes and common facilitators and barriers;^{10,11} however, others suggest that the two are clearly distinct.^{8,12}"

Paragraph two of the introduction is long. Consider the overarching introduction structure: 1. What is the general problem? 2. What is the specific problem? 3. What do we add? (dividing the introduction into three paragraphs). (see Welch HG. Preparing manuscripts for submission to medical journals: the paper trail. *Effective clinical practice: ECP*. 1999;2(3):131-7.)

Response: We acknowledge that this was a longer paragraph and have revised accordingly.

Page 5, paragraph 2: Page 5, paragraph 2: "A growing body of evidence has evaluated barriers and facilitators for adopting high value practices (effective at improving outcomes).³⁻⁶ Substantially less is known about the barriers and facilitators for de-adopting low value practices (ineffective at improving outcomes or harmful), and how they compare to those for adopting high value practices.^{7,8} De-adoption, also known by several other terms such as disinvestment and de-implementation,⁷ is the discontinuation of a practice that has been previously adopted.⁹ Some have suggested that the adoption of high value practices and de-adoption of low value practices involves similar processes and common facilitators and barriers;^{10,11} however, others suggest that the two are clearly distinct.^{8,12}"

Consider moving information on choice of measures (LWMH and Albumin) to methods.

Response: Based on suggestions to streamline the manuscript we have deleted the paragraph in question. We believe the introduction is now more consistent with the structure outlined suggested by the Reviewer above.

Methods: The authors used clinical data to identify ICU admitted patients without contraindication to LMWH and those without indication for Albumin. They collected a broad range of patient level variables. As noted above, the modeling of predictors of low-value care and high-value care is interesting but feels like a separate study, and, in fact, the model output is relegated to supplemental material. I cannot quite figure out how to incorporate this unless it is being used to control for patient differences in the examination of variation across ICUs.

Response: The Reviewer raises a concern around the cohesion of the data presented in our study. Data on the use of the two illustrative best practices and patient level factors that may contribute to the use of best practice were collected and analyzed to support the perceived barriers and facilitators to the use of these two practices identified in the survey. As an example, we found sicker patients received less best care practices which was not identified in the survey but will need to be taken into consideration when designing implementation interventions to optimize evidence based care.

To be inclusive of recommendations provided by all Reviewers, we have streamlined the data presented (moved less relevant yet interesting findings of the audit to supplemental content and moved data displays of survey results to the main manuscript). Because of collective interest by the Reviewers and our interpretation of the importance of the modelling data it is now presented in the main manuscript as Table 1.

The authors model outcomes two ways, patient day and patient. Why?

Response: The unit of analysis for our outcome for the adoption cohort (LMWH use) was patient days because VTE prophylaxis is a routine clinical practice that should be performed on a daily basis. Conversely, the unit of analysis for our outcome for the de-adoption cohort (albumin use) was per patient because fluid resuscitation is a sporadic event that is not part of routine daily patient care. This explanation has been added to the data analysis section of the audit data on Page 8, paragraph 1.

Regarding survey methods. I am not a survey expert. It may be worth having such a person review this work.

Response: Our research team consists of members with considerable expertise in survey methodology (Stelfox et al. *Ann Surg.* 2012;256:163; Stelfox et al. *NEJM.* 1998; 8:101; Holodinsky et al. *PLoS One.* 2015;10:e0145408; Sauro et al. *PLoS One.* 2018;13:e0205280; Cook et al. *J Crit Care.* 2014;29:471.e1-9; and others) including publication of an evidence-based guide for the design and conduct of surveys (Burns et al. *CMAJ.* 2008; 179:245). The survey used in this study was modelled after an instrument successfully used and validated in a previous study, which was developed by a team member using well established survey methods (Cook et al. *J Crit Care.* 2014;29:471.e1). We made conscious trade-offs between strengths and limitations of different survey approaches which are outlined in the discussion.

Page 23, paragraph 3 to page 24, paragraph 1: "One limitation of this study is that the survey used was imperfect. The results of the self-reported survey reflect perceived modifiers of practice among providers rather than factors shown to influence practice patterns as identified in observational studies.⁴⁷ The survey was purposefully designed to be simple and accessible to garner a representative perspective from all provider professions and therefore captured key concepts, but not granular data. Nevertheless, the survey has been successfully used for a similar purpose by others;³³ was reliable and reported to have good clinical sensibility. Alternative methodologies such as qualitative analyses of semi-structured interviews may have allowed for more in depth exploration of barriers and facilitators to adopting LMWH and de-adopting albumin."

Results: The authors illuminate practice patterns, and document less adherence to evidence than teams perceive. They also reveal perceived barriers to evidence adoption/de-adoption, which these respondents attribute to both activities.

It is not clear if the survey instrument itself may have led to similar factors being identified as influential in the two practices because the same plausible options were offered for each. It is like giving the same multiple-choice options on two questions and discovering similar answers.

Response: The Reviewer proposes an interesting hypothesis that we cannot test with our data. The literature evaluating de-adoption is in its infancy and it is unknown how similar or different the barriers and facilitators for de-adoption are to those for adoption. However, we appreciate the Reviewer's comment and may test this hypothesis in future work.

It is also not clear to me that the list of options reflects what we understand to be the range of barriers for adoption/de-adoption obstacles. More information might have emerged from open-ended questions, though that would require qualitative methods.

What was revealed in the "other" option?

A survey expert may be helpful here. That is area of not my expertise.

Response: The survey response options were derived from a previously developed and validated instrument that was successfully used in a multicenter study (Cook et al. J Crit Care. 2014;29:471.e1-9). We acknowledge different methodologies have respective strengths and limitations and have revised the discussion to highlight this point.

Very few participants responded to open text fields and hence these data are not presented.

Page 23, paragraph 3 to page 24, paragraph 1: "Nevertheless, the survey has been successfully used for a similar purpose by others;30 was reliable and reported to have good clinical sensibility. Alternative methodologies such as qualitative analyses of semi-structured interviews may have allowed for more in depth exploration of barriers and facilitators to adopting LMWH and de-adopting albumin."

The authors find "When controlling for demographic and site-level factors, there were no differences in adoption or de-adoption based on patient age, sex, or comorbidity (Supplemental Content 4)."

Again this seems interesting but I struggle with how to include in a paper about opinions on practices.

The authors present some information that seems of low relevance to the stated aim of the work (e.g., "The objective of this study was to describe illustrative example practices of the adoption of high value practices and the de-adoption of low value practices in the ICU.") For example; Mortality rate and LOS. Was there a hypothesis around these measures?

That said, the fact that only severity of illness reliably predicts low-value care could inform future interventions.

Response: The Reviewer raises concerns related to best way to amalgamate the presentation of audit data and survey data. We acknowledge that while effectively presenting both sets of data is challenging, we believe it is important because they provide complementary information needed to inform future interventions (as the Reviewer highlights). We have extensively revised the manuscript in response to the Reviewer's comments to improve the presentation of these data.

1) We have moved data displays of less relevant or interesting audit results to supplemental content - what was previously Figure 1 (flow of patients for the audit) and Figure 2 (LMWH use) to the supplemental content (Supplemental content 4 and 5).

2) We have moved data displays of the results of the survey and key findings of the audit to the main manuscript - what was previously supplemental content 7 and 8 as a two-panel figure into the main manuscript (Figure 2) and formerly supplemental content 6 is now table 1.

3) We streamlined the description of the findings of the audit within the results section. Page 12, paragraph 2 to page 13, paragraph 2: "There were 6,946 ICU admissions during the study period, from 6,299 unique patients. Patient characteristics are presented in Supplemental Content 3.

The adoption cohort consisted of 4,931 admissions (71.0% of all admissions) without a contraindication to pharmacological VTE prophylaxis, and the de-adoption cohort consisted of 6,467 admissions (93.1%) without a potential indication for albumin (Supplemental Content 4).

During the ICU stay LMWH was given on 38.7% of ICU days, UFH on 45.3% of ICU days and mechanical prophylaxis (exclusive of pharmacological prophylaxis) on 7.7% of ICU days. The type of VTE prophylaxis administered varied throughout patients' ICU stay; administration of mechanical devices and UFH decreased over the course of the ICU stay while administration of LMWH increased (Supplemental Content 5).

6,804 units of albumin were administered to 20.0% of the 6,467 admissions without documented liver disease or receipt of plasma exchange. Among those receiving at least 1 unit of albumin, the median number of units per patient was 3 (IQR=1.0-6.0). Albumin was administered on 6.5% of ICU days.

When controlling for demographic and site-level factors, the odds of receiving LMWH for VTE prophylaxis and not receiving albumin for fluid resuscitation were significantly lower for those patients with higher severity of illness (APACHE II score). The odds of receiving LMWH for VTE prophylaxis were significantly higher for patients with non-surgical admissions compared to those with elective surgical admissions (odds ratio = 1.34 (95% confidence interval 1.08-1.66); Table 1). There were significant differences in the odds of using LMWH for VTE prophylaxis, and not using albumin for fluid resuscitation across ICUs (Supplemental Content 6), and when controlling for patient-level factors some of these differences persisted especially with regards to the use of LMWH for VTE prophylaxis (Table 1)."

4) Revised the discussion so it is primarily focused on the survey results, except for findings that directly support the survey data (knowledge of best practices and potential patient-level factors that may determine the use of best practices).

I also find it interesting that, controlling for patient factors, use of LMWH and Albumin varied significantly across the ICUs. I think this point warrants a bit more discussion.

Response: We have added discussion of this finding to the discussion.

Page 22, paragraph 2: "ICU culture and local clinical leader preferences were among the most commonly endorsed barriers to adopting high value practices and de-adopting low value practices in this study and in our study. This is highlighted by the variation in the use of LMWH between ICUs, even when patient level factors were taken into consideration. Interestingly, this finding was less pronounced for de-adoption, which has been previously reported.⁸"

Regarding tables and figures, I think some of the information in the supplement more valuable than its placement suggests, and, conversely some information in the main paper, less valuable than its placement suggests. This, of course, depends on the focus of the paper.

Response: We have elected to move Figure 1 and Figure 2 to supplemental content.

Table 1 effectively communicate the findings, but perhaps in more detail than is needed. I wonder about a table or chart that tells me % of each group identifying correctly the most effective treatment. Then details could be in supplementary material.

Response: We avoided classifying the answers as correct or incorrect, as the interpretation of research is designed to promote debate, which we discuss in the background and discussion. The footnotes in Table 2 (previously table 1) indicate what the evidence suggests is most correct.

Figure 1 (patient flow) seems tangential to the point of the paper. It adds little since the design is simple and well described in the text.

Response: We have moved figure 1 to supplemental content (supplemental content 4).

Figure 1 (VTE proph. choice) seems of some value but is not key to the paper, especially as presented by day of stay, unless there is a hypothesis about LOS and treatment choice. It would be sufficient to report relationships between LOS and VTE prophylaxis product chosen if this seems important. A granular chart is not needed.

Response: We have moved this figure to supplemental content (supplemental content 5).

Rather than seeing treatment by LOS, I would like to know how many patients crossed treatment groups. In my mind, that begins to let us understand more about the relationship between severity of illness and low-value care than is reported. I think there is something here – that desperate situations lead clinicians to deviate from evidence or simply try something else, perhaps.

Response: We agree with the reviewer. A total of 780 patients (15.8%) out of 4931 received both LMWH and UFH during their ICU stay, and there was an association between severity of illness as measured by APACHE II score type of pharmacological prophylaxis received. We have elected at present to not expand presentation of the audit data in the manuscript due to the Reviewer's earlier recommendations to primarily focus the manuscript on the survey data, but would be willing to do so if either the Editor or Reviewer feel it is important to include.

I wonder if sicker patients got other VTE prophylaxis or crystalloid followed by albumin (in response to lack of response to the former). I am not suggesting the authors explore this for this paper, but perhaps rephrase the conclusion about severity of illness prompting "conservative" decisions. I like the suggestion to consider severity of illness in study of decision making but the path to the observed care matters – did clinicians reach FIRST for non-evidence base care, or was non-evidence based care a response to failed improvement?

Response: We have revised the discussion of this issue.

Page 21, paragraph 1 to page 22, paragraph 1:" Furthermore, confidence in applying new evidence in clinical practice may be particularly challenging in the care of severely ill patients. This hypothesis is supported by two of our findings: 1) the use of LMWH for VTE prophylaxis and not using albumin for fluid resuscitation was inversely associated with severity of patient illness and 2) the use of LMWH and not using albumin increased as the patient became more stable (over ICU stay). Potential hypotheses to explain these observations include that clinicians may employ conservative decision-making (use more familiar practices) or unintentionally neglect to use best practices when caring for sicker patients, but this need further exploration."

The role-specific survey (MD, nurse, pharmacist) is interesting, as are the model results that communicate numerically the relationship between variables and treatment decisions. Currently no tables in the main paper communicate this and only OR is reported in the text. I would like to see Suppl. Table 5 in the manuscript or at least discussed a bit more.

Inclusion of the method details and instrument in the supplemental materials is helpful.

Response: We agree with the Reviewer that these are interesting findings. As such, we have decided to move the figures from the supplemental content to the main manuscript (now Figure 2).

We also agree that the relationship between patient-level factors and choice of treatment is interesting and valuable information in designing implementation interventions. As such, we have decided to move the table from supplemental content to the main manuscript (now Table 1).

Smaller items:

1. The topic sentences in the discussion are a bit unwieldy, distracting from the thoughtful consideration of the study's implications. For example: "Knowledge translation (KT) interventions; strategies to improve the synthesis, dissemination, exchange, and application of evidence to improve health, tailored to the specific barriers and facilitators of an innovation and the local context are more likely to effect change."

Response: We have revised this sentence.

Page 21, paragraph 1: "Knowledge translation (KT) interventions are strategies to improve the synthesis, dissemination, exchange, and application of evidence to improve health.⁴ KT interventions tailored to the specific barriers and facilitators of an innovation and the local context are more likely to effect change.^{4,5}"

We have also carefully reviewed and revised the discussion to improve clarity. For example:

Page 20, paragraph 2: "There is substantial literature describing the adoption of high value practices, but much less is known about de-adoption of low value practices.⁷"

Page 22, paragraph 2: "ICU culture and local clinical leader preferences were among the most commonly endorsed barriers to adopting high value practices and de-adopting low value practices in this study and in our study. This is highlighted by the variation in the use of LMWH between ICUs, even when patient level factors were taken into consideration. Interestingly, this finding was less pronounced for de-adoption, which has been previously reported.⁸"

2. I am not sure this sentence communicates what you intend. "Both observations suggest that clinicians may employ conservative decision-making when caring for sicker patients." Does conservative mean evidence-ignoring?

Response: We have revised this sentence to improve clarity.

Page 21, paragraph 1 to page 22, paragraph 1: "Furthermore, confidence in applying new evidence in clinical practice may be particularly challenging in the care of severely ill patients. This hypothesis is supported by two of our findings: 1) the use of LMWH for VTE prophylaxis and not using albumin for fluid resuscitation was inversely associated with severity of patient illness and 2) the use of LMWH and not using albumin increased as the patient became more stable (over ICU stay). Potential hypotheses to explain these observations include that clinicians may employ conservative decision-making (use more familiar practices) or unintentionally neglect to use best practices when caring for sicker patients, but this need further exploration."

3. The authors mention but do not explore order sets as a source of deviation from evidence and/or a source of variation across ICUs. They mention that LMWH use increased over the study period, but we do not know why – this seems important to think about. A comment on order sets would be helpful if possible; it may not be.

Response: We did not implement any changes in care practices during the study period. We did ask about the perceived value of order sets as a facilitator and report this data in the manuscript. We observed an increased use of LMWH over individual patients' ICU stay but not over the course of the study period.

RESPONSE TO REVIEWER 5

The paper describes use of two practices, one evidenced-based the other not, in intensive care units in two Canadian cities and reports the results of a survey that assesses the factors that may promote or retard evidenced-based practice. The survey data are interesting.

Response: We thank the Reviewer for taking the time to review our manuscript and for providing thoughtful feedback.

The data on the use of two practices are helpful for putting the survey results in context but are otherwise not particularly interesting.

Response: We appreciate the Reviewer's enthusiasm for the survey data presented. We believe, as the Reviewer points out, that the patient care audit data complements this information and both sets of data will be valuable in developing implementation interventions to improve care. We also believe these data provide some insight into factors that contribute to the use of best practices. For example, severity of illness was associated with receiving best practices and the use of best practices improved as patients become more stable during their ICU stay.

My main comment is that the paper should not use the terms "adoption" and "de-adoption" when describing a point-in-time analysis. The paper describes use of two practices at a point-in-time. I think it is misleading to say that the study describes adoption or de-adoption. There is an element of temporality implicit in these concepts.

Response: We agree with the Reviewer that the use of the terms "adopt" and "de-adopt" implies temporality and could be misleading when describing a point-in-time analysis. We have accordingly carefully revised the manuscript to ensure accuracy of our terminology. We continue to use the terms "adopt" and "de-adopt" when discussing the barriers and facilitators to the adoption and de-adoption because these do refer to processes that will occur over time.

Some examples of where the terminology has been made more precise include:

Page 13, paragraph 2: "When controlling for demographic and site-level factors, the odds of receiving LMWH for VTE prophylaxis and not receiving albumin for fluid resuscitation were significantly lower for those patients with higher severity of illness (APACHE II score). The odds of receiving LMWH for VTE prophylaxis were significantly higher for patients with non-surgical admissions compared to those with elective surgical admissions (odds ratio = 1.34 (95% confidence interval 1.08-1.66); Table 1). There were significant differences in the odds of using LMWH for VTE prophylaxis, and not using albumin for fluid resuscitation across ICUs (Supplemental Content 6), and when controlling for patient-level factors some of these differences persisted especially with regards to the use of LMWH for VTE prophylaxis (Table 1)."

And

Page 20, paragraph 1: "The use of the best practice for these two illustrative examples were less likely for patients with greater severity of illness and varied across institutions. The perceived barriers and facilitators to adoption and de-adoption were broadly similar."

Before you quote the statistic that practice change can take up to 17 years, you might want to take a look at this paper: Howard DH. Evidenced-based claims about evidence. *Medical Decision Making: Policy & Practice* 2017;2(2):2381468317734527. In short: There isn't good evidence behind the 17 year statistic.

Response: We thank the Reviewer for providing this citation. We have revised this statement.

Page 5, paragraph 1: "Optimizing the quality of care¹ is of particular importance in the intensive care unit (ICU) due to the acuity of patient illness and substantial resources required to care for these patients. However, practice change (adopting high value practices or de-adopting low value practices) is slow with some evidence suggesting it can take well over a decade.² To minimize the latency for change, it is important find ways to improve the implementation of best practices."

In the last paragraph in the Introduction, one sentence states "were chosen by a network of medical-surgical ICUs..." and the last sentence states "Patient and family representatives, frontline providers, and decision-makers considered....chose low molecular". These seem to conflict. Or at least belabor a point that could be made quickly and without fuss.

Response: We agree with the Reviewer. In response to another Reviewer's concern about the length and structure of the introduction this paragraph has been deleted.

Under Results, Audit of Current Practices, I didn't know how to interpret the statement that ICU mortality was 14.1% and hospital mortality was 21.0%. Does hospital mortality include deaths that occur in the hospital after a patient is discharged from the ICU?

Response: To address one of the other concerns of the Reviewer (presentation of the audit results) this sentence has been removed, but these findings are still presented in Supplemental content 3. Hospital mortality captures deaths that occurred anywhere during the patients' hospital stay – the ICU or other units.

In Table 1, it would aid interpretation if you could indicate the right or preferred answer to each question (if that is possible).

Response: We avoided classifying the answers as correct or incorrect, as scientific evidence is continually evolving and the interpretation of research is designed to promote debate, which we discuss in the background and discussion. The footnotes in Table 2 (previously table 1) indicate what the evidence suggests is currently most correct.

VERSION 2 – REVIEW

| | |
|-------------------------|---|
| REVIEWER | Peter Nydahl Nursing Research, University Hospital of Schleswig-Holstein, Kiel; Department of Anesthesiology and Intensive Care Medicine, Germany |
| REVIEW RETURNED | 09-Dec-2018 |
| GENERAL COMMENTS | The reviewer thanks the authors for their hard work and submission of a revised manuscript. The authors answered all concerns of the reviewer, or argued very reasonable. The authors improved their manuscript and finally re-submitted a very fine paper, which makes important contributions to the field. The reviewer suggests to publish this manuscript soon. Personal note: it is not easy to answer to all concerns of so many reviewers, but you did it very well! |
| REVIEWER | George Anesi University of Pennsylvania Perelman School of Medicine; Philadelphia, PA, USA |
| REVIEW RETURNED | 05-Dec-2018 |
| GENERAL COMMENTS | The authors have thoroughly addressed my comments and I have no further revisions to suggest. |
| REVIEWER | David Howard Emory University, USA |
| REVIEW RETURNED | 19-Dec-2018 |
| GENERAL COMMENTS | I appreciate that you dropped a direct reference to the 17-year statistic (reference 2), but you still cite the paper, which is problematic because the paper is problematic. And the claim that "...practice change (adopting high value practices or deadopting low value practices) is slow with some evidence suggesting it can take well over a decade." is incorrect. There are some (many?) |

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| | <p>cases where evidence has been rapidly adopted into practice. If you want to make the point that adoption can be slow, I think it is better to qualify the statement and cite some specific examples. For example, you could write: It can take many years for evidence to affect practice, particularly when clinicians are asked to de-adopt existing treatments. (cite examples, like the influence of the OAT trial). I think the statement would still be a little misleading, but it would be an improvement on what is there now.</p> |
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VERSION 2 – AUTHOR RESPONSE

RESPONSE TO REVIEWER 2

1. The reviewer thanks the authors for their hard work and submission of a revised manuscript. The authors answered all concerns of the reviewer, or argued very reasonable. The authors improved their manuscript and finally resubmitted a very fine paper, which makes important contributions to the field. The reviewer suggests to publish this manuscript soon.

Personal note: it is not easy to answer to all concerns of so many reviewers, but you did it very well!

Response: We thank the Reviewer for reviewing the revised manuscript and for their kind feedback.

RESPONSE TO REVIEWER 3

1. The authors have thoroughly addressed my comments and I have no further revisions to suggest.

Response: We thank the Reviewer for taking the time to review the revised manuscript.

RESPONSE TO REVIEWER 5

1. I appreciate that you dropped a direct reference to the 17-year statistic (reference 2), but you still cite the paper, which is problematic because the paper is problematic. And the claim that "...practice change (adopting high value practices or de-adopting low value practices) is slow with some evidence suggesting it can take well over a decade." Is incorrect. There are some (many?) cases where evidence has been rapidly adopted into practice. If you want to make the point that adoption can be slow, I think it is better to qualify the statement and cite some specific examples. For example, you could write: It can take many years for evidence to affect practice, particularly when clinicians are asked to de-adopt existing treatments. (cite examples, like the influence of the OAT trial). I think the statement would still be a little misleading, but it would be an improvement on what is there now.

Response: We acknowledge the Reviewer's concern with the accuracy of this statement and the cited manuscript. We have addressed the Reviewer's concern by revising the sentence for clarity and replacing the reference with two other references (McGlynn et al. NEJM. 2003, which shows that recommended care is only delivered about half the time, and Niven et al. JAMA IM. 2015 which, shows that there was a rapid adoption of evidence for tight glycemic control but de-adoption of this practice once new high-quality evidence was published was slower).

Page 5, paragraph 1: “Optimizing the quality of care[1] is of particular importance in the intensive care unit (ICU) due to the acuity of patient illness and substantial resources required to care for these patients. However, practice change (adopting high value practices or de-adopting low value practices) can lag behind the publication of evidence hindering delivery of evidence-based practices and may be different when adopting or de-adopting practices.[2, 3] To minimize the latency for change, it is important to find ways to improve the implementation of evidence-based practices.”