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Reviewer A

This important and timely submission characterizes the importance of an intensivist-led closed intensive care unit. The authors have demonstrated improvements in various metrics following the closure of the ICU, and these conclusions are in keeping with opinions within the critical care medicine space.

Nevertheless, the absence of granular detail makes the manuscript less persuasive. For example, within the exclusionary criteria, did the authors account for emergency cases, cases that may have required mechanical support or cases of high lethality such as aortic dissection? The differences may just as plausibly have resulted from any one possible alteration in selection, criteria, or candidacy. Each of these may serve as a confounder and should be evaluated within the methods or, at the very least, be mentioned as a limitation.

Author Response:

We would like to thank the reviewer for pointing this out and are happy to provide more detail regarding emergent cases. Three emergent cases were included in the OPEN cohort, two emergent cases were included in the CLOSED cohort and we have added these numbers to our Table 1. We have added the following to the Methods section “Following IRB approval, data was sourced from the Arkansas Data Repository for patients who underwent **cardiac and ascending aortic surgery by median sternotomy, including emergent cases**, and were subsequently admitted to the SICU one year prior and one year after implementation of a closed SICU model on August 1, 2017”. In addition, we have addressed the impact that selection, criteria and candidacy may have had on our results with the following limitation statement in the Discussion section “**There are also patient selection**, institutional policy, individual surgeon, and procedure variables that affect outcomes that cannot be fully appreciated in a retrospective review, **and may serve as confounders**”.

The premise of the manuscript is in keeping with the future of intensive care, and the authors should be applauded for seeking to quantify the changes in objective terms. Nevertheless, the small sample size and the need for increased rigor suggest one or two things. Either:

- 1. the authors should accrue a greater sample size and more rigorous methodology OR**
- 2. they should summarize their findings into a case report.**

Author Response:

We would like to thank the reviewer for their feedback. While we agree that a greater sample size would strengthen our findings and acknowledge this in our limitations statement, we do think that some valuable conclusions can be drawn from this study as a retrospective cohort pre-post comparative group analysis. We acknowledge the need for additional larger scale

studies to clarify the impact of a closed SICU on post-operative complications as well as hospital charges in our Discussion section.

Reviewer B

Congrat on your nice work in this paper, small sample size in this series though, you could show us the better improvement of LOS & readmission rate of the cardiac patients with closed SICU model.

Author Response:

We appreciate this reviewer's feedback.

Reviewer C

This is a retrospective review of pre and post conversion of a general ICU from an open model to a closed model in patients undergoing open heart surgery. The paper is well written and addresses a valuable question that would be of interest to readers. I have the following comments which should be addressed prior to consideration for acceptance:

Methods:

Which comorbidities were included? Would be useful to specifically state how many patients had pulmonary dysfunction and renal dysfunction pre-operatively since these are stronger predictors of ICU LOS than more benign comorbidities (HTN, DM, etc).

Author Response:

All pre-admission comorbidities were included in the comorbidity number calculation. We performed targeted review for pulmonary and renal comorbidities and have added these results to Table 1 and the Results section. "The OPEN and CLOSED cohorts did not vary significantly in age, race, number of comorbid conditions on admission, or **pre-operative renal/pulmonary dysfunction**"

Why were patients with incomplete chart information excluded? How many patients would this have resulted in excluding? If just one variable was missing were patients excluded? Would this introduce any selection bias as these registries tend to have inconsistencies in data acquisition over time?

Author Response:

We thank the reviewer for pointing this out, and are happy to provide clarification regarding the "incomplete chart information" exclusion criteria. Patients were only excluded if they lacked the chart variables necessary to calculate data needed for our outcomes. The most common incomplete information was comorbidities prior to admission, which we feel is necessary for our outcomes as more comorbidities could impact ICU LOS, mortality, etc... Of course it is possible that inconsistencies in data acquisition over time *could* introduce some selection bias, though we have no reason to believe that there was any change in data acquisition in the two years that data was gathered for this study. In fact, we were intentional to clinically define our cohorts with equal time frames in the OPEN and CLOSED periods to reduce selection bias. In addition, the

number of patients excluded for incomplete chart information was very similar, 27 in the OPEN cohort vs. 19 in the CLOSED cohort. Regardless, we have added the following acknowledgement to our Discussion section “There is possibility for the introduction of selection bias due to inconsistencies in data collection over the study period, though there were no hospital data acquisition policy changes during this time.”

Results:

Please included exactly how many patients were excluded per each exclusion criteria (ie how many excluded for incomplete records)

Author Response:

We have added the following breakdown of exclusion criteria and number of patients excluded to the Results section “Three patients were excluded from the OPEN cohort because they were imprisoned (3 OPEN) and 1 patient was excluded from the OPEN cohort because they were pregnant (1 OPEN). Five patients were excluded because their admission overlapped the cohort periods (2 OPEN, 3 CLOSED), 46 patients were excluded for incomplete chart information (27 OPEN, 19 CLOSED), and 40 patients were excluded because they were not admitted to the SICU at any time (23 OPEN, 17 CLOSED). ”.

Subgroup analysis of only CABG/valve patients, or excluding the “other” patients should be performed. Other operations were much more common in the closed group and may include relatively simple operations. This also was not included in the model and might be contributing to the improved outcomes in the CLOSED group. Sternal debridement should have a fairly short ICU LOS and might be contributing to the results.

Author Response:

We appreciate the reviewers feedback, and have performed sub-group analysis of the patients who received CABG and valve procedures. The results of this analysis are summarized in Table 2, and have been added to our Results section. We see similar trends in the bivariate analysis with significant hospital readmission rates.

The in-hospital mortality of 4.8% and 8.8% seems high, and the percentage for hospital readmission is incorrect. If 4 patients in the open group died in-hospital and 6 patients in the closed group died in hospital, your hospital readmission rates are 25/64 (39%) and 12/78 (15%), respectively. Additionally, if any of these deaths occurred in SICU these patients should be excluded in you SICU LOS outcome variables (if somebody died within 2 hours of arrival to SICU they will have a short SICU length of stay).

Author Response:

We are thankful to the reviewer for pointing this out. The Results section and Table 2 have been edited to reflect the correct percentages for hospital readmission rates. Patients with mortality in the SICU were indeed excluded from calculation of the ICU LOS variable as well as the SICU

charges variable. We have added the following statement to the Methods section “Patients with mortality in the ICU were excluded from the LOS and SICU charges analysis.”

Discussion:

“Regarding the trend toward significant difference in SICU charges”, this sentence is misleading because there was no significant difference. Please rewrite to just state results and follow with discussion.

Author Response:

We appreciate the reviewers feedback and have removed the “trend”. The sentence now reads “There was no significant difference in SICU charges between the two groups.”

This study appears to be being performed as a low-medium volume center. Please address if your findings are generalizable to large volume centers

Author Response:

Our results are likely not generalizable to large institutions with dedicated cardiac ICUs. However, for many academic medium sized hospitals, especially in rural states with large catchment areas, we hope our results at least provide a point of discussion about the benefits of the closed model.

Please discuss the in-hospital mortality rate and if this is in keeping with the literature for this patient population.

Author Response:

We are happy to provide more information regarding in-hospital mortality rate in cardiac surgery patients. In-hospital mortality ranges from 3-4.4% according to several large cardiac surgery database reviews and we have added those sources into our references. We have added the following statement to the Discussion section “Though not statistically significant, the in-hospital mortality rate in the CLOSED SICU group is more similar to that reported in the literature for this patient population of between 3-4.4% [9,10],”

The rate of ICU LOS >48 hours of 89% and 71% seems high. Are there other factors in the hospital that may play a role in this? Availability of floor beds? Is this rate in keeping with the literature for this patient population?

Author Response:

We are thankful for this observation by the reviewer, and are happy to provide more information regarding the data on ICU LOS in cardiac surgery patients. The definition of prolonged ICU LOS after cardiac surgery does vary in the literature as much as 24 hours to 7 days depending on the source. Our institution faces similar struggles as most hospitals our size regarding availability of step-down units and floor beds and we have added the following statement to the Discussion section to acknowledge this “The definition of “prolonged” ICU LOS after cardiac surgery is widely variable in the literature (24 hours to 7 days), which presents difficulty in asserting if our

reported ICU LOS is a good representation of this population [13]. Our institution faces similar struggles as most medium sized hospitals regarding availability of step-down units and floor beds, which may have impacted the ICU LOS outcome.”

Were there any other changes that may have affected the hospital readmission rate during the time period?

Author Response:

We appreciate the reviewer pointing this out. As far as the authors are aware there were no major management or policy changes that occurred that may have affected the hospital readmission rate during our study period. However, we cannot rule out that some confounders may have played a role, and so the following statement has been added to the Discussion section **“There are also patient selection, institutional policy, individual surgeon, and procedure variables that affect outcomes that cannot be fully appreciated in a retrospective review, and may serve as confounders”**.