Author's response to reviews

Title: Effectiveness of a novel and scalable clinical decision support intervention to improve venous thromboembolism prophylaxis: a quasi-experimental study.

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Author's response to reviews: see over
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Dear Editor Aldcroft,

Thank you for inviting us to resubmit our manuscript “Effectiveness of a novel and scalable clinical decision support intervention to improve venous thromboembolism prophylaxis: a longitudinal study” for your consideration for publication in *BMC Medical Informatics and Decision Making*.

We have uploaded a revised manuscript file, a second copy with changed sections marked in “tracked changes”, and have provided detailed responses to the reviewers’ comments below. We appreciate the excellent feedback offered by you and the reviewers assigned to our manuscript, and believe it has improved our work greatly.

On behalf of all of my co-authors, we look forward to your reply.

Yours sincerely,

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Reviewer 1:

General comments:
This work provides useful insights on how to increase VTE prophylaxis rates using a CDS intervention across a health-system. However, several issues require clarification prior to publication.

MAJOR COMPULSORY
1. Abstract/Results. The abstract indicates the total number of patients included in the study but this was not stated in the body of the paper (or the tables). Please include the total number of patients in the results section of the paper (or a study flow diagram) in addition to including this information in the abstract. Major

We added the total number of patients to the results.

2. Background. A paragraph on risk stratification is needed. In the methods section, the authors indicate that eleven risk factors were used to make risk stratification decisions. Since risk stratification is a critical part of the decision support that a CDS system is intended to provide, it should be made clear to readers what the major risk factor for VTE are. A clinician with limited knowledge of VTE or an administrator who is reading this paper may not be as familiar with these risk factors. Major

Risk factors for VTE and contraindications to pharmacologic prophylaxis that were listed in the CDS were added to the methods section of the manuscript.

3. Background, First paragraph, line 3, you mention VTE rates from 40% - 60%. These rates are for asymptomatic VTE (found on venography) in patients who received no prophylaxis. The rates of symptomatic VTE events are much, much lower. This should be clearly stated and the rate of symptomatic events should also be included in the background as you report only symptomatic VTE events in your paper. Major

We clarified that the estimates reported in the Background were for asymptomatic DVT, and included estimates for symptomatic events as well.

4. Methods. Please state the experimental design used for this study.

The familiar term “interrupted time series” was made more explicit in the data analysis section of the methods and throughout the manuscript.

5. Methods, Page 5, paragraph 2, line 4: The authors mention eleven VTE risk factors. What are these eleven risk factors? Does the CDS auto-populate some or all these risk factors based on information in the patient’s medical record? Or does the clinician have to check off these risk factors as part of the ordering process? It should be clearly stated how (procedurally) the information is gathered and used to do risk stratification. Major
We added to the methods section of the manuscript the risk factors for VTE and contraindications to pharmacologic prophylaxis that were listed in the CDS. Providers were expected to order prophylaxis unless their patients were low risk as defined by the absence of all eleven listed risk factors. Because it was rare for a patient to have none of the eleven listed risk factors, we did not ask providers to check off risk factors as part of the ordering process, nor did the CDS auto-populate risk factors based on the medical record. We added these details to the methods section of the manuscript.

6. Methods, Page 5, paragraph 2, line 7: The authors mention the second iteration of the CDS being launched but don’t address why a second iteration was deemed necessary. Please address, perhaps in the discussion section, and any effect it may have had on your results. Major

We note in the results that although unadjusted rates of our three prophylaxis measures increased between periods 2 and 3, the adjusted rates using our time series analyses showed no difference. However, when we examined the type of prophylaxis administered stratified by how the provider initially responded to the CDS intervention (i.e., a “yes” versus “no” response to the question of whether or not the provider would order VTE prophylaxis), we found that most providers who indicated they would order prophylaxis did order it, and that proportion increased over our two follow-up periods, from 89.0% to 93.8% in periods 2 and 3, respectively (p < 0.01). Those who said they wouldn’t order prophylaxis often did not (63.7% ordered no prophylaxis in period 2 vs. 74.1% in period 3, p < 0.01). Given these results, we note in the first paragraph of our discussion that “our revision to the CDS intervention where we linked the risk assessment to the prophylaxis order set grid improved the concordance between the users’ risk assessments (i.e. what they said they would order) and the prophylaxis actually ordered.” We added in the first paragraph of the discussion that our intended goal with the revision was to improve the concordance between what the users said they would order and what they actually ordered.

7. Methods, Page 5, paragraph 2, line 11: When VTE prophylaxis was not ordered, you indicate that the clinician was required to give a specific reason. Was this done using check boxes (pre-defined reasons), a free text field, or both? Was this requirement true for both the first and second iterations of the CDS? Or just the second iteration? Please clarify. Major

A drop down menu with pre-defined reasons (e.g. patient has none of the risk factors, patient is on therapeutic anticoagulation) and a free text option was available in both iterations. This was clarified in the manuscript.

8. Methods. Please discuss how providers were educated regarding this intervention and the CDS. Did it just appear one day? Or did each institution have a major VTE educational series that informed clinicians about VTE prophylaxis, its importance to the institution, how to use the CDS, etc? Was there a communication plan (e.g. newsletters, posters, email) about this initiative? In your discussion please talk about how practitioner education and communications outside the CDS may have impacted the
results. Major

Before launching the intervention across UPHS, pharmacists increased their stock of VTE prophylaxis, and nurses increased the supply of mechanical VTE prophylaxis on the wards. Nurses also implemented a nursing education program about the intervention and the availability of LMWH as a VTE prophylaxis option. In addition, an email communication about the intervention was distributed to all faculty and staff. Lastly, a point-of-use educational video was developed and implemented to educate ordering providers about the intervention. These details were added to the methods. We also added text in the Discussion section about the potential impact of increased staff awareness on VTE prophylaxis and event rates.

9. Results, Page 9, paragraph 3. These data and the analysis were not described in your methods. Please describe how this data was collected and analyzed. Major

A description of this data was added to the “Data sources and measures” section of the Methods, and a description of the analyses was added to the “Data analysis” section of the Methods.

10. Discussion. Given that most hospital-associated (HA) VTE events occur after hospital discharge, it is critically important to mention to reader that your methods/results only captured a fraction of HA VTE events. Major

This was added to the limitations section of our discussion.

11. Table 2. Error? Why does the baseline rate of recommended VTE prophylaxis use change at HUP in sections A (31.5) vs. B (30.9). Since the baseline (period 1) is the same in both A and B sections of the table, the baseline rate should be same. Similarly, the baseline rate for the entire system changed slightly (27.1 vs. 27.2). Moreover, given that the baseline rate when DOWN at HUP (A vs. B) and baseline rates remain unchanged at PMC and PAH, why would the system-wide rate go UP (A vs. B)? Please correct or clarify. Major

The percent values of the 'baseline' and 'increase' are estimated averages generated by a regression equation. The 'baseline' value is the mid-point of a straight line regression plotted through time period 1. It is not a percent derived by dividing a numerator by a denominator for the entire period. Since the 'baseline' and 'increase' values are estimates derived from an equation, any change to the input data of the equation will produce different estimates. In this case, the change of data input was the inclusion of time period 3 in the second analysis. The change had an effect on all metrics from all the hospitals but was imperceptible at PAH and PPMC given the number of significant figures utilized. We placed a footnote in table 2 to clarify this: “The above percentages are estimates, derived from a time series regression model.”

MINOR ESSENTIAL
1. Abstract, Methods section, Line 3: Consider revision, as the word limit allows, to
indicate there were three period of analysis in the study. Suggested wording “Time series analyses were used to examine the impact of a VTE prophylaxis CDS intervention during time period one and time period two compared to baseline and a simple pre-post design examined the impact on VTE events.” Minor

The abstract was modified to clarify that there were three periods of analysis in the study.

2. Background, First paragraph. How effective is VTE prophylaxis? What is the rate of asymptomatic and symptomatic VTE when appropriate VTE prophylaxis methods are use? Are there any benchmark data available? This would allow comparison of benchmark data with your results. Minor

We included estimates of the effectiveness of prophylaxis in the Background. We also included estimates of the rate of symptomatic DVT and PE in the context of appropriate prophylaxis in the Discussion, and compared the rates in our study to these expected rates.

3. Background, Last paragraph, last sentence. The purpose of the study is not clearly stated. There are really two main outcomes you talk about in the discussion: increased prophylaxis rates and VTE events. It appears the study was designed to primarily evaluate changes in prophylaxis rates after implementation of CDS and not VTE events (although you do capture this data). Minor

The purpose of the study was clarified in the last sentence of the background.

4. Methods. The authors mention the different hospitals in the health-system. We recommend including a brief description the patient populations and services provided at each hospital. What subspecialties are offered? How many admissions are there at each hospital per year? If I am thinking about implanting a similar system at my healthsystem, I’d like to know if I have similar (or very different) patients. Minor

We included in the first paragraph of the methods more details about each of the hospitals. We also deidentified the hospitals throughout the manuscript.

5. Methods. We recommend expanding the description of the experimental design to include whether the study was conducted in a prospective or retrospective manner. Minor

The study was retrospective. This was added to the second section of the Methods.

6. Methods, Page 6, paragraph 2, line 8: Please clarify who was excluded. It is not clear why patients discharged from these services were not included in the analysis. What percentage of patients admitted to each hospital was excluded from the analysis? Please include the number of patients included/excluded in the results section (e.g. a study flow diagram would be helpful). Minor
The only adults admitted to an acute care inpatient setting at UPHS who were excluded were those without a listed discharging service. Because we used patients’ discharging service to define their “recommended prophylaxis”, we excluded those without a “discharging service” listed. This accounted for 1546 encounters out of 224,608 encounters in our dataset, or 0.70% of our encounters. These details were added to the methods and results as appropriate.

7. Methods, Page 7: The authors list the process measures in this order: any prophylaxis, pharmacologic, then recommended … but then discuss them in this order: recommended, pharmacologic, then any. This is confusing to the reader. Please discuss them in the order they are listed. Minor

We described the results in the abstract and the body of the manuscript in the order of recommended, any, and pharmacologic prophylaxis. Thus, we changed the order of the process measures listed in the methods to reflect the order used in the abstract and results section of the manuscript.

8. Methods, Page 7, paragraph 1, line 12: Please list the mechanical prophylaxis methods available. Minor

Mechanical prophylaxis was defined as an order for intermittent pneumatic compression devices. This was clarified in the Methods section.

9. Results, Page 8, paragraph 1: “Importantly” there was a decrease in “unknown” health insurance status. Why is this observation important? Do you believe it impacted the results in some way? We recommend to address this in your discussion section, or if this is not important, to exclude the statement from your manuscript. Minor

The statement was excluded from the manuscript.

10. Discussion. You bleeding event rates were very low likely due of the method used to capture these events (using E codes representing medical or drug errors plus a secondary bleeding CPT code). This needs to be clearly pointed out to readers. Minor

The full test characteristics of the bleed codes we used as well as the implications of these test characteristics were included in the discussion.

11. Table 2. Header labels. Consider changing the “baseline” header to “Period 1” to make this clearer to readers. Minor

Done.

DISCRETIONARY

1. The title accurately reflects the content of the work. The authors could consider
substituting “a quasi-experimental study” for “a longitudinal study” to more accurately reflect the experimental design. **Discretionary**

*Done.*

2. Methods, Page 7, paragraph 1, line 13: How was CrCl calculated by the CDS? Was the patient’s actual body weight, adjusted body weight, or ideal body weight used when calculating creatine clearance with the Cockcroft-Gault equation? Where was data regarding the patient’s weight obtained (e.g. patient’s EHR or clinician input)? **Discretionary**

*The Cockcroft-Gault equation was used to estimate creatinine clearance. All of the values used in this equation were automatically imported from the patients EHR, without any additional input by the provider. The most recent values for age, actual body weight, gender and creatinine were used, whether they were from the index admission or the last values of the last admission.*

3. Discussion. Page 11, paragraph 1, line 5: Rates of line-associated VTEs. Are these rates similar to other institutions? How do these rates compare to what’s been reported in the literature? **Discretionary**

*These rates reflect the proportion of inpatients with PICCs or midlines, not the proportion of patients with PICCs or midlines who have VTE. We clarified this in the text.*

4. Table 2. Why do you combine Periods 2 and 3 in section B? Why not compare Period 2 to Period 3? **Discretionary**

*Time periods 2 and 3 were combined because they both included the CDS intervention, as compared to time period 1, which was the baseline control. When we compared time period 2 to time period 3, the prophylaxis rates were not significantly different. This was clarified in paragraph 3 of the results.*

**Reviewer 2:**

*Whereas it is an interesting clinical article assessing the effectiveness of a computerized clinical decision support intervention on VTE prophylaxis and events in a big series of hospitalized patients in Pennsylvania, the follow-up is too short (just the hospitalization period) and the clinical relevance of the intervention should be emphasized, in order to draw practical conclusions.*

*We noted the short follow-up of our study in our limitations section in the discussion: “…our study did not examine the effect of our CDS intervention on VTE events after hospital discharge, where many such events may occur.”*
We believe that the amended conclusion in our abstract and manuscript summarize the clinical relevance of our study. We would also be happy to consider emphasizing other findings which the reviewer deems most important.

Major comments

The study fails to show a significant benefit beyond a surgical setting which are those receiving more appropriate thromboprophylaxis.

We added more detail to the results as well as three new tables to demonstrate that the intervention did have a benefit in improving VTE prophylaxis rates across all of our services and hospitals. In addition, the intervention was associated with a reduction in VTE across a prespecified surgical subset of our sample as defined by the AHRQ PSI for public reporting purposes.

They have to discuss deeperly the higher incidence of PE despite intervention and some of the observed results focused on medical inpatients, which are those in which thromboprophylaxis is generally underused.

We added to our Discussion section a paragraph regarding the demonstrated higher incidence of PE despite our CDS intervention. We also included more detailed information in our results and discussion regarding the observed changes in VTE prophylaxis across our services, including the medicine service.

Minor comments

-See additional references reporting both clinical and economical effectiveness of an electronic alert system in the prevention of VTE should be incorporated and discussed (Lecumberri et al Thromb Haemost 2008 and J Thromb Haemost 2011)

We thank the reviewer for these suggestions. We reviewed both papers, and added the most relevant study by Lecumberri and colleagues published in Thromb Haemost in 2008 to the background of our manuscript.

-Thrombosis in keywords

Corrected.

Reviewer 3:

Discretionary Revisions

Suffice to say on page 5 that the iteration was launched in 4/08; on page 6, much space again expended clarifying the difference of one day, which the reader does not need to dwell on. No need to repeat the details about Sunrise.

This information was removed.
**Minor Essential Revisions**

Page 2: consider: VTE causeS morbidity… systems to prevent IT

*Changed.*

Page 3: spelling in the keyword “throbosis”

*Corrected.*

General: is the punctuation not before the reference #, like in this question? 1,2,3

This is how the format occurs on, say, page 11, but not on some earlier pages.

*We reviewed the references in our manuscript, and in the majority of cases, we cite our references at the end of sentences after the period. In rare circumstances, we have cited references directly after the citable information. This most commonly occurs in sentences with multiple clauses, where the references are relevant to one clause of the sentence, but not another. We would be happy to make any further modifications as the reviewer and editor sees fit.*

Misplaced comma on page 11.

*Corrected.*

**Major Compulsory Revisions**

Page 2:
Should there not be a comment on bleeding analysis in the methods if they are reported in the conclusions?

*This was added to the abstract.*

Recommended prophy went up about 7-8% and yet it also increased from 27 to 52%? This is confusing.

*Our unadjusted analysis suggested recommended prophylaxis increased from 27 to 52%. When we adjusted our analysis using the interrupted time series model, the increase in recommended prophylaxis was 8%. This was clarified in the abstract and the manuscript.*

Page 3:
I would exclude the outdated discussion of asymptomatic VTE events and substitute some estimate of symptomatic VTE; note the movement away from surrogate endpoints in ACCP and ACP publications.

*We clarified that our estimates were for asymptomatic DVT, and included estimates for symptomatic events as well.*
Might want to quote the primary source for material in paragraph 1; consider removing the 8th ACCP source now that the 9th is out (although there is less useful overview material).

We appreciate the reviewer’s suggestion, but we decided to preserve our citation to the 8th version of the ACCP guideline given that it was our primary reference for this information. We did note in our limitations section however that our work was based on the 2008 guideline, which was recently replaced by the 2012 version.

Actually, reference 2 says in 2008 that CMS is considering adding DVT to its list for nonpayment, in 2009—and isn’t it orthopedic post op DVT?

This was corrected in the Background section.

Missing from the list of published CDS efforts is a non-popup alert based effort which increased adequate prophylaxis to >95% with a reduction in overall VTE: J Hosp Med. 2010 Jan;5(1):10-8. Optimizing prevention of hospital-acquired venous thromboembolism (VTE): prospective validation of a VTE risk assessment model. Maynard GA. This effort was since translated into Epic, a widely used commercial EMR. Or other successful projects; McKeaJ Hosp Med 2009 lists a few.

We thank the reviewer for these suggestions. We reviewed the papers cited, and added all studies not previously included that examined the impact of a CDS intervention designed to increase VTE prophylaxis and that were published in a peer-reviewed journal, including the study by Maynard and colleagues.

Page 6:
Would list the exclusion criteria adjacent to the inclusion criteria, not separate them by the time periods.

Done.

Page 7:
Duplication of prophyl advice—why not say we recommend x,y, or z on all patients except a,b,c on ortho trauma?

The duplication was corrected.

Are there no other services, eg neuro?

The “medicine” service included neurology, as well as these other services: allergy, cardiology, endocrinology/diabetes, family medicine, gastroenterology, general internal medicine, geriatrics, hematolgy/oncology, hospitalists, infectious diseases, neurology, pulmonary, renal/metabolic and rheumatology. The “other surgery” service included: anesthesia, cardiac surgery, colorectal surgery, obstetrics, gynecological
oncology, gastrointestinal surgery, maternal fetal medicine, neurosurgery, otolaryngology, plastic surgery, general surgery, surgical oncology, thoracic surgery, transplant surgery, urology, oral/maxillofacial, and vascular surgery. These details were added to the methods section.

Would list definitions in the order they’re originally listed (any prophy mentioned first and defined last) OR just list the terms once followed by definitions.

Done.

The comment on creatinine clearance could be moved to the list of measured items as a parenthetical “a,b,c, creatinine clearance (measured by the C-G equation), etc etc” back on page 6; seems out of place at the end.

Done.

Also, was pharm prophy truly “recommended” for everyone, including patients with bleeding, or low risk? No contraindications are mentioned.

**Recommendations for prophylaxis were based on locally adapted national guidelines. Based on these guidelines, we recommended pharmacologic prophylaxis for most inpatients. Notable exceptions included patients admitted to the inpatient services following routine transurethral procedures, inpatient vascular surgery patients in the first 48-72 hours post-op, and low risk neurosurgical patients in the inpatient setting. The CDS intervention included a list of contraindications, and a description of this list was added to the third paragraph of the methods section.**

If only prophy heparin doses count for the medicine services, what about those who arrived on therapeutic Coumadin?

*Therapeutic Coumadin is on the list of contraindications to additional pharmacologic prophylaxis.*

Can the authors be certain that discharges with VTE as the primary dx are always POA VTE? What if PE became the primary problem, might a coder possibly end up listing that as a primary diagnosis even though it was a HA-VTE?

*We confirmed with the administrative lead for our coders at Penn that if VTE is a primary diagnosis it is automatically assigned a “POA yes”, and if the VTE is hospital-acquired during the admission then it is assigned a “POA no” and is listed as a secondary diagnosis. This is consistent with standard coding manuals as well as the AHRQ PSI definition of hospital acquired VTE.*

Following reference 16 for the AHRQ PSI subset is a frustrating experience. The website does not list the patient inclusion criteria for this subset in an easy to find manner. I tried a bunch of searches and still don’t know what subset we’re
discussing. A direct link would be better. (I see the comments about surgery patients later on 12).


Page 8:
The bleeding analysis is fundamentally limited. It relies on a coder recognizing that a bleeding event was linked to a prophylactic dose of AC or physicians documenting the same. This is practically impossible since people bleed with and without VTE prophy. A better measure would have been rates of overall bleeding, but even that is quite limited and one would need bleeding criteria as defined in major trials, measured via EMR or chart by chart—perhaps not feasible, but represents a limitation.

We expanded on this limitation in the limitations section of the discussion.

What other confounders did the statistics adjust for?

No other confounders were adjusted for in the interrupted time series analyses.

Here is a major discrepancy with the abstract and page 9. Abstract says pharm prophy did not go up. Here it says it did.

This was corrected and clarified in the abstract and the manuscript. Pharmacologic prophylaxis increased in the unadjusted analyses, but not in the adjusted analyses.

It is not clear how recommended prophy (which was defined as UFH TID enox 40 or dalte 5000) went to 52%, while pharm prophy went to 54% (was the difference just some UFH BID?).

VTE prophylaxis using enoxaparin 40mg daily or UFH BID or TID on the orthopedic/trauma services, or UFH BID on the medicine and other surgical services would qualify as “pharmacologic” prophylaxis but not “recommended” prophylaxis on these services. This distinction explains how “pharmacologic” prophylaxis could increase to rates higher than “recommended” prophylaxis.

Most importantly seems that (again) the “recommended” prophy takes no account of contraindications, which is an odd definition.

The CDS intervention included a list of contraindications, and a description of this list was added to the third paragraph of the methods section.

Page 9:
Any thoughts about why PE went up when DVT went down, since they are part of the same disease?

We added to our Discussion section a paragraph regarding the demonstrated higher incidence of PE despite our CDS intervention, and our thoughts about potential causes.

What about percent prophy in the AHRQ subset? If we are to focus on this subset (and why?) should we not hear how VTE prophy changed in this group? Otherwise, why assume the whole project would be worth duplicating? VTE could have fallen for other reasons, like it went up for other reasons overall.

In our unadjusted analyses, "recommended" prophylaxis (32.3% vs. 50.7% vs. 60.0%; p<0.01), “any” prophylaxis (62.8% vs. 82.0% vs. 85.7%; p<0.01) and “pharmacologic” prophylaxis (47.9% vs. 56.3% vs. 63.3%; p<0.01) significantly increased in the AHRQ subset across our three time periods. This information was added to the abstract and body of our manuscript.

Page 10:
I think the authors overcall their use of a default treatment. It’s not as if the prophy order was prechecked and users had to cancel it if not wanted; for the first section of the intervention, they had to go to another module to find the “default” order.

This sentence was removed.

If the desirability of this product is its reach of all hospitalized patients, what are we to make of the lack of VTE impact except in a subset of the data? Note: other work HAS targeted whole hospital populations, and the authors should avoid implying their steps are groundbreaking in this way.

We further expanded paragraphs three and four of our Discussion to address the potential reasons for lack of VTE impact in our overall population. In addition, although we stand by our statement that many studies investigating CDS interventions to improve VTE prophylaxis do target select populations, and that a potential strength of our study is that it addresses all inpatients, we tempered our statements regarding this topic in both the introduction and the discussion, and emphasized that one of the key strengths of our study is that we examined the impact of a CDS intervention to improve VTE prophylaxis across all inpatients and hospitals in a multi-hospital academic health system.

Page 11:
True that another limitation of the data is lack of information about whether any meaningful prophy occurred. Many sites struggle with prophy resumption after transfer or procedures. Here, a single dose counts as “sufficient” – I would mention this point in the limitations as well, also, you don’t need changes in the frequency of missed prophylaxis to account for unchanged VTE rates; they might
not change because the prophy remained underdosed throughout.

*These changes were made to the limitations.*

I’m not sure refs 26 and 27, apply fully. The ACP review notes the risk benefit calculus, but you should be able to predict a reduction in VTE based on your N of patients and their VTE per 1000 patient data--the real point is benefits are small. And the mortality study is irrelevant to your DVT prevention study; mortality has never been reduced in a trial or a meta analysis nor did you assess it.

*The sentences and associated references were removed.*

Was the subset analysis prespecified or posthoc?

*Prespecified. This detail was added to the abstract as well as the body of the manuscript.*

Page 12:

Other limitations to consider mentioning:

Why even look at patients who had VTE present on admission? We are only interested in HA-VTE, UNLESS you were able to identify “POA” VTE as HA-VTE related to recent hospitalizations. But the methods makes it clear that you did not have POA data for earlier admissions. This is a significant limitation. Because period 2 started at 4/7/08 for PMC, and POA data was unavailable until 4/1/08 at PMC, for example, you missed an entire time period. A causal reader would not notice this.

*This point was added to the limitations sections in the Discussion.*

The poor linkage of providers stating they would not use prophy but many going on to order it suggests “blow through” of the module—people zooming through it without using it properly because it felt intrusive into their work flow.

*Providers could have declined prophylaxis at the time of admission for a valid reason such as “no risk factors” or “therapeutic anticoagulation”, but then subsequently ordered it if the clinical situation changed (e.g. the patient undergoes surgery, or therapeutic anticoagulation is stopped).*

The number of patients deemed a bleed risk seems low—I admit far more than 2% of patients with severe ESLD or outright bleeding, never mind low platelets, PUD, or other issues.

*The estimates listed in the results for “reasons why providers decline VTE prophylaxis” are limited by self report. Two percent reflects the percent of providers declining prophylaxis who listed “bleeding risk” as the primary reason for not ordering prophylaxis. It is likely not a valid estimate of the percentage of patients who are admitted who have*
bleeding risks. For example, a provider could decline VTE prophylaxis for a patient with “no risk factors” who is at risk of bleeding, and list “no risk factors” as the primary reason for not ordering prophylaxis. Alternatively, a provider could admit a patient for a lower GI bleed who will undergo a colectomy, and list “peri-procedure” as the primary reason for declining prophylaxis. This limitation was added to the Discussion section.

The limitations discussion on admin data misses the fundamental point. It’s nice that VTE or bleeding codes have reasonable sensitivity or PPV—but if this were a robust method, you wouldn’t have had to do other analyses to show that VTE was stable not worse, and maybe went down in a subset, but some elements of the same disease were more frequent as others dropped.

The limitations regarding the unavailability of POA data for VTE in our primary analyses were emphasized in the limitations.

The bleeding data performance measure I would most want would be sensitivity, which you do not mention. Maybe you caught only 10% of the bleeding events from your therapy, but those coded had high PPV—not that useful.

The sensitivity data was added to the discussion.

For conclusions:
Pharmacologic prophylaxis, or a measure of adequate prophy (pharm, or mech if pharm is contraindicated) would be the best measure to rate, since mech prophy is iffy. The issue with pharmacologic prophy (did it go up or not) is of major importance here as a result.

Our measure of “recommended prophylaxis” attempts to capture the use of “adequate pharmacologic prophylaxis” for those services where pharmacologic prophylaxis is indicated, or the use of mechanical prophylaxis for those services where pharmacologic prophylaxis is not indicated.

Pharmacologic prophylaxis did increase in our unadjusted analysis, but not in our adjusted analysis.

The prespecified findings of the study should be the primary conclusion reported—if you searched for all VTE events, then your conclusion is that your efforts did not decrease all VTE (rather, it increased). You can mention a posthoc analysis on POA events and patient subsets AFTER that.

We agree, and amended the conclusion of our abstract and manuscript accordingly.

Some things worth considering that are not mentioned:
1) successful programs require multiple interventions, a lot of effort, education, and the transmission of quality culture. Look at Pronovosts work in michigan’s CLABSI data—they didn’t just put out an orderset, they had to change the way
doctors and nurses spoke to each other. For a VTE example, look at the multimodal recommendations in AHRQ’s guide to improvement, or the SHM VTE resource room. Throwing a protocol out there doesn’t work well, and the authors say not a word about how this change was announced or if anything else was done in their project—I assume not. 52% “recommended” prophylaxis is not sufficient, even if an improvement, and thus there should be some “lessons learned” rather than just the suggestion other sites can adopt this partially effective orderset.

We agree with the reviewer, and appreciate the reviewer’s comments. More detailed information describing the development and implementation of the CDS intervention was added to the first part of the methods section.

2) Is the orderset available to other sunrise users, anyway? If not, then this is not any more scalable than ANY orderset someone would have to reprogram to their institution. If so, where do they get it?

To raise awareness of the clinical decision support (CDS), we have shared the information in this manuscript with clinical leadership at Allscripts, and presented the CDS intervention at an Allscripts regional users group meeting as well as a prior Society of Hospital Medicine national meeting. We also hope to increase the awareness of the CDS through publication of our findings. The code for the CDS intervention is available from our data architect at the University of Pennsylvania.

Declaration of competing interests:
I have no financial COI. I have worked on similar projects myself and have worked in SHM’s VTE quality improvements mentorship program. We believe based on experience with > 100 hospitals that simply tossing an orderset out there without a QI framework and high reliability design measures is insufficient to expect such a project to achieve success.

We agree with the reviewer, and appreciate the reviewer’s comments.