in an updated examination of the national data during the MELD era. However, the inference that waitlist time alone is the factor contributing to the reduced survival in the MGH-migrated cohort has yet to be validated. As we mentioned in our discussion, the lack of complete data from the secondary center, including date of evaluation and listing, was a limitation of the study. The latter data, as well as other contributing factors potentially involved (donor type, operative course, immunosuppression, transplant center/provider practices, fragmentation of care, etc) all warrant further investigation to help ascertain the observed increased mortality in the MGH-migrated group. A mirror analysis by Dr Lee and his colleagues may help better define the complete picture.

Third, we agree that a further understanding of the waitlisted population is important. Indeed, our practice now focuses on identifying those waitlisted candidates who fit the demographics of the migrated candidate for discussions regarding the aggressive pursuit of expanded criteria liver allografts or living donor liver transplantation in order to achieve transplant without relocating. In addition, we believe that we must broaden the discussion to consider the at-large population with end-stage liver disease (ESLD). Are there low MELD candidates within our region not referred for evaluation given the high MELD scores required to obtain a transplant? One cannot assume that the entire ESLD population consists only of those referred or listed for liver transplantation. Finally, we disagree with Dr Lee and his colleagues on their interpretation of our data. The geographic disparity that continues to persist across regional lines is irrespective of exception point listing. Indeed, the match MELD score for the MGH-transplanted cohort with HCC exception points was 28, and for non-HCC patients it was 32. In contrast, in a recent report of 966 liver transplants performed at Mayo Clinic, Jacksonville, FL from 2005 to 2010, 96% of the recipients had a median MELD score of 20.3 We believe our data demonstrate that a privileged few who are unable to achieve transplant in Region 1, due to low MELD scores, migrate in order to receive a liver transplant, but do so at the cost of a reduced 5-year overall survival. Furthermore, although we agree with Dr Lee and his colleagues that the MGH-migrated cohort, with a match MELD of 19.6, have the potential to benefit from liver transplantation, we do not believe that they should have to travel across the United States to obtain that benefit.

A geographic inequity for the distribution of livers continues to exist. We applaud the efforts put forth by the Organ Procurement and Transplantation Network/United Network for Organ Sharing Liver and Intestinal Organ Transplantation Committee in seeking to address these issues, and look forward to adjustments that will allow access to liver transplantation to proceed based on need, rather than an individual’s ZIP code or ability to migrate.

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No Evidence to Support Ambulation for Reducing Postoperative Venous Thromboembolism

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We read with great interest the recent article, “Reducing postoperative venous thromboembolism complications with a standardized risk-stratified prophylaxis protocol and mobilization program” by Cassidy and colleagues.1 As the authors of the chapter on practices to prevent venous thromboembolism (VTE), in the “Making Health Care Safer II” report from the Agency for Healthcare Research and Quality (AHRQ), we appreciate the authors’ interest in prevention of VTE and standardization of VTE prophylaxis practices. In fact, AHRQ identified “interventions to improve prophylaxis for VTE” as one of the top 10 patient safety practices.2 In our opinion, the authors make strong claims about the benefit of ambulation as prophylaxis against VTE without any evidence to support their claim.

First, the authors attribute a reduction in VTE events to implementation of a mobilization program alone. However, they neither accounted for nor offered possible explanations for, the dramatic downward trend in VTE
events observed in the preimplementation period. A more appropriate analysis to compare results across these intervention periods would include an evaluation of the difference-in-differences of the outcomes, which has been used by previous studies when evaluating quality improvement interventions. This analysis would describe the downward temporal trend, which is clearly evident (Figs. 2, 3, and 4 in their article) before the start of their intervention. Compared with the preintervention trend line, there is no change in the slope of the line due to the mobilization program.

Second, the authors included “sitting in a chair” as an example of optimal practice ambulation, which we do not believe one could reasonably expect would have a beneficial impact on VTE risk, even if ambulation alone does. The fact that ambulation was observed only in a small sample of patients, and that no attempt was made to quantify the amount of ambulation (eg, frequency, distance, time, steps, speed) by patients gives little credence to the value of ambulation in this population.

Third, the authors provide no quantitative process measure data for either mechanical or pharmacologic practices at baseline. Instead, they state “pneumatic compression boots and subcutaneous heparin were frequently used,” “but outpatient VTE chemoprophylaxis was rarely, if ever, used” and “patients generally remained in bed more than desired.” Without a quantitative measure of practice change for all types of VTE prevention, it is impossible to assess what is responsible for the observed improvement in outcomes. Using our informatics expertise, we have been able to show the ability to link these critical VTE process measures (from the same Sunrise Acute Care, Allscripts system) to our institutional NSQIP data.

Fourth, bundled interventions have been shown to be effective at reducing patient harm and complications. The prime example is the case for the bundle of care aimed to reduce central line-associated bloodstream infection in the ICU, in which constituent components contribute to overall harm reduction. Many times it is this overall bundle of care that results in better outcomes, rather than constituent parts, making it difficult to ascertain individual pieces. Yet creation of the bundles most often starts with an evaluation of the evidence for benefit of each individual component. In the case of VTE prevention, thousands of studies have demonstrated the benefit of pharmacologic VTE prophylaxis. Yet none has demonstrated the effectiveness of ambulation alone, nor the incremental added benefit of ambulation with concurrent pharmacologic VTE prophylaxis.

We are pleased that the authors were able to reproduce our intervention with a mandatory computerized VTE risk assessment and clinical decision support-enabled prophylaxis recommendation tool and report similar findings, thereby validating our previous work. The authors have noted several key features, including the mandatory risk assessment, in which the provider must perform the risk assessment or orders cannot be completed.

The authors highlight early ambulation as an important component of successful VTE prevention. In light of the scant data provided to support this recommendation, we do not believe this is an appropriate message. We have been fighting this common misconception, which we have found leads to a significant number of missed doses of VTE prophylaxis. Although we agree that early ambulation of hospitalized patients is likely of overall benefit in general, its value in VTE prevention remains unclear. Until these critical data are known, we caution readers against relying on early ambulation as a component in any VTE prevention program.

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Reducing Postoperative Venous Thromboembolism Complications

In Reply to Lau and colleagues

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We thank Mr Lau and his colleagues for their interest in our recent article, “Reducing postoperative venous thromboembolism complications with a standardized risk-stratified prophylaxis protocol and mobilization program.” We agree that venous thromboembolism (VTE) is a leading cause of preventable morbidity and mortality, and that the key to optimization of VTE prevention will be standardized, evidence-based prophylaxis practices.

In their letter, Mr Lau and coauthors express concern that we have overstated the benefit of postoperative mobilization for the prevention of VTE. Our article describes a mandatory electronic standardized risk stratification and pharmacologic prophylaxis protocol combined with a standardized postoperative mobilization program that achieved a significant reduction in VTE events among our patients. Mr Lau and colleagues’ letter states that we have attributed the reduction in VTE events to our ambulation program alone. A close reading of the manuscript reveals that this is not the case. The message of the article is that the mandatory VTE risk stratification and risk-based pharmacologic prophylaxis together with mobilization led to the dramatic reduction in VTE events. Although we noticed that VTE complications began to decline as the mobilization aspect of the program was introduced, even before the automated risk-stratified prophylaxis was fully integrated into the computerized physicians’ order sets, our article clearly states in the Discussion section that we cannot demonstrate the efficacy of either intervention alone, and that our intent was to analyze the effect of a standardized bundled program encompassing both interventions. The language in our article does not overstate the contribution of mobilization alone, nor do we claim that mobilization alone can account for the outcomes. It would be simple to attribute the sharp decline in postoperative VTE to the formal implementation of the Caprini system, as that had been its intent. However, the structured mobilization program had been introduced for pulmonary purposes just 6 months earlier. It is impossible to measure the relative contributions of these 2 elements of care, but it would be dishonest to disregard a major change in clinical practice that had been installed nearly simultaneously with the risk-based prophylaxis.

The authors of the letter recognized a downward trend in VTE events during the preimplementation phase of our study, and they are correct in that observation. However, they comment that we have provided no explanation for that trend. In fact, referring to page 1100 of our published article, we explained that our faculty were aware of the high incidence of VTE events at our institution and we were involved in the deliberations as we planned and formalized VTE prevention efforts. As stated in the article, we believe that changes in practice naturally occurred as a result of these deliberations, even before full electronic implementation.

Mr Lau and his coauthors are correct that we did not attempt to strictly quantify ambulation in our experimental design. We performed observational audits of practice, and we demonstrated a significant change in the frequency of mobilization after the intervention. The optimal “quantity” of mobilization that may provide a benefit to postoperative patients is unknown and requires further study. The issue is complex and is influenced by a number of factors, including patient comorbidities, baseline functional status, the operation performed, and local resources. Although the question of how much ambulation will be beneficial is interesting, it was not a question we intended to answer with our study. Rather, our goal was to demonstrate the feasibility of a qualitative change in postoperative practice, while tracking outcomes over time.

The authors have also indicated that we did not provide quantitative analysis of our VTE process measures before implementation of our program. Although this fact contributed to the limitations of our study, we aimed to compare a system in which no standardized VTE prevention practices existed to a new system based on formal standardization. Our comparison should be interpreted...