Shifting the Focus of ICU Staffing Research to the Community

To the Editor:

In an issue of CHEST (May 2012), I read with great interest the article on 24-h intensivist staffing in teaching hospitals by Kerlin and Halpern1 because it was published at the same time as a large retrospective study that did not reveal a mortality benefit with night intensivist staffing at high-intensity ICUs.2 Kerlin and Halpern1 make strong arguments in support of prospective randomized trials on the topic of ICU staffing. As well, I recently questioned the need for 24-h intensivist coverage on the basis of my anecdotal experience in the community setting.3

In the article, the authors focused on the risks and benefits of 24-h attending staff as these relate to medical education and the care of patients in an academic setting. They also briefly discussed the potential for the unintended consequences of decreased staffing and compromising care at regional hospitals, widening existing chasms of care. I emphasize that this is not a theoretical problem—it is real and ever growing in the community, where the shortage of trained intensivists is already painfully felt. For this reason, this topic should be examined in earnest, as advocated by the authors, and the focus should shift to the community setting. I believe that there is a pressing need to explore the value of night intensivists to community ICUs, more so than studying that of a night ICU attending physician working with trainees vs trainees alone to academic ICUs.

Most community hospitals in my metropolitan region have shifted from a 12- or 16-h workday to 24-h coverage. As Kerlin and Halpern1 mentioned, the benefits of this move seemingly have strong face validity and the support of observational studies. However, a significant consequence of this shift is that a small regional pool of community intensivists is now spread thin to cover nights. This, in turn, has led to lean and fractured day coverage and exacerbated a provider tug-of-war between facilities where full-time equivalent (FTE) and moonlighting physicians recruited to fill voids created by night staffing at one facility leave voids at another. For example, we recently recruited an FTE physician from one facility, leaving that facility, which had recently transitioned to 24-h coverage, more short staffed. A nearby hospital just transitioned to 24-h staffing and recruited one of our moonlighting physicians, necessitating our FTE physicians to work more nights. These voids are challenging to fill given the ongoing shortage of trained intensivists and may offset the purported benefits of 24-h coverage.

Nonacademic ICUs account for more than one-half of the ICUs in the United States.2 Despite this, most ICU staffing research to date has involved academic centers, leaving a paucity of evidence to guide community ICU staffing. Research on staffing in academia has focused on the question of whether 24-h attending physician coverage is necessary. This has little bearing on community ICUs because these typically are staffed with one intensivist, not a team of trainees led by an attending physician. The main question for community ICUs is whether the presence of a night intensivist is at all necessary if there is high-intensity staffing during the day. Alternatives to 24-h intensivist staffing include night intensivist phone coverage, remote video monitoring, physician extender coverage, expanded hospitalist coverage, or any combination of these. These alternatives need to be explored rigorously because the demand for trained intensivists continues to outstrip supply. If viable, these alternatives may help to alleviate the increasing staffing strain in the community.

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References

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Management of Recurrent Malignant Pleural Effusions

An Ever-Recurring Issue?

To the Editor:

We read with great interest the CHEST Point/Counterpoint Editorials by Lee1 and Light2 (July 2012) on thoracoscopic talc pleurodesis (TTP) vs an indwelling pleural catheter and would like to make several comments. The value of thoracoscopic assessment of the pleural cavity during talc poudrage has not been carefully studied. Thoracoscopic circumvents the limitations of a blind talc application through a chest tube, which as pointed out by Dr Lee, can only be successful with adequate apposition of the visceral and parietal pleura. Thoracoscopy, which is commonly done with local anesthesia or conscious sedation in the outpatient setting, allows for debridement, lysis of adhesions, and optimal talc pleurodesis.

Regarding the study by Dresler et al,3 we agree that the post hoc subset analysis showing a better efficacy of TTP over talc slurry is, at best, hypothesis generating. However, we would like to point out that the complications reported in the TTP arm were staggering, with blood transfusion, atelectasis, pneumonia, respiratory failure, and postoperative death in 4.5%, 1.3%, 9.3%, 8.1%, and 8.4% of patients, respectively. These results do not reflect those described by pulmonologists around the world. It is difficult to argue against the large body of safety data for talc pleurodesis in light of two multicenter prospective European studies.4,5 To answer Dr Light’s concern about the study by Janssen et al,6 one of the authors (P.A.) was involved in both studies and can attest to the extreme caution used in determining the causes of respiratory manifestation observed after talc pleurodesis.

Regional differences in health-care delivery rather than data on efficacy and safety often dictate management. In that context, the data provided by the only cost-effectiveness analysis available, as quoted by both authors,1,2 is of questionable relevance on efficacy and safety often dictate management. In that context, the data provided by the only cost-effectiveness analysis available, as quoted by both authors,1,2 is of questionable relevance, considering the calibrrated French talc used in the majority of countries (but not in the United States) has a proven record of short- and long-term safety and efficacy, we anticipate that talc pleurodesis will remain the standard of care chosen by most pulmonologists around the world.

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Response

To the Editor:

I commend Drs Maldonado and Astoul for their points in support of thoracoscopic talc poudrage (TTP) as the first choice for malignant pleural effusion.1 Light2 based his rebuttal on the study by Dresler et al,3 where patients with malignant pleural effusions had to demonstrate >90% lung expansion before they received talc slurry via chest tube or TTP. Thirty-two percent and 27% of patients randomized to talc slurry and TTP groups, respectively, were excluded, and they could have trapped lungs from pleural loculations and extensive tumor load for which thoracoscopic adhesiolysis would be beneficial by promoting fluid drainage and lung expansion. Complications associated with TTP performed by surgeons in these patients with good Eastern Cooperative Oncology Group status (0–2) were higher than those in the published literature,7 particularly blood transfusion (4.5%), respiratory failure (8.1%), and postoperative death (8.4%). The type of talc used was also unclear, because small-particle talc (<15 μm) could have caused respiratory failure.3 A trial involving 13 centers in Europe and one center in South Africa demonstrated the safety of TTP with large-particle talc (mean size, 24.5 μm). TTP was performed by pulmonologists in 558 patients with Karnofsky scores >50 or Eastern Cooperative Oncology Group <4. Thirty-day mortality was markedly lower, at 1.97%. Only one patient (0.17%) developed respiratory failure from contralateral pneumothorax, and no patient required surgical bailout or blood transfusion.8 In his

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