Communications for this section will be published as space and priorities permit. The comments should not exceed 350 words in length, with a maximum of five references; one figure or table can be printed. Exceptions may occur under particular circumstances. Contributions may include comments on articles published in this periodical, or they may be reports of unique educational character. Please include a cover letter with a complete list of authors (including full first and last names and highest degree), corresponding author’s address, phone number, fax number, and email address (if applicable). An electronic version of the communication should be included on a 3.5-inch diskette. Specific permission to publish should be cited in the cover letter or appended as a postscript. CHEST reserves the right to edit letters for length and clarity.

Critically Ill Obstetric Patients Treated in an ICU

To the Editor:

We read with interest the article by Afessa and colleagues (October 2001) about obstetric patients needing critical care. We would like to comment on our experience after a retrospective study developed in a tertiary hospital, from 1991 to 1998. During this period, out of 49,717 delivery patients, we admitted 149 patients (0.3%) to the ICU.

Contrary to the Afessa study, where the prevalence of African American women was 64%, most of our patients (85%) were white women, and only 45% had long-term underlying medical conditions. This may explain the different reasons for admission to the ICU between the two studies. In the study by Afessa, the most common reasons for admission to the ICU were respiratory failure (45%) and hemodynamic instability (32%); in our series, however, obstetric conditions (70.4%), especially pregnancy hypertension (50.4%), were the most common reasons for ICU admission. Our most frequent complications were disseminated intravascular coagulation (38%), acute renal failure (19%), and ARDS (14%). The incidence of ARDS in our study was similar to that described by Afessa (15%).

We were surprised by the high incidence of pulmonary edema that developed in patients after emergent cesarean section, as described by Afessa (31%), because we have not found any case of this respiratory complication. We also found it curious that hypertension was listed as the cause for only 5% of the ICU admissions. In their description of complications, Afessa et al referred to as many as 33 cases of eclampsia, preeclampsia, and the syndrome of hemolytic anemia, elevated liver enzymes and low platelet count (HELLP).

In our study, only 35% of patients were treated with mechanical ventilation, fewer than the 45% referred by Afessa. This is probably related to our lower incidence of respiratory failure. Finally, when comparing fetal and maternal mortality rates, Afessa described a maternal mortality rate of only 2.7%, and eight perinatal deaths (11%); we experienced a maternal mortality rate of 7.5% and a fetal mortality rate of 13%. Although our study would seem to have had high mortality rates, they were quite low compared with those described by Plateau et al (21%), Collop and Sahn (20% and 35% respectively), Monaco et al (18% and 12% respectively), or Kilpatrick and Matthay (25%). This is why we were surprised by the even lower incidence found in the Afessa study.

In conclusion and after reading a recent article by Waterstone et al, we think future studies should try to estimate the predictor factors of severe obstetric morbidity, in order to improve prenatal care, perinatal management, and anesthetic procedures. With reliable predictor factors, we could reduce the number of obstetric patients who require critical care, and we could lower the rates of maternal and fetal morbidity and mortality.

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To the Editor:

In the experience related by Olarra et al, 0.3% of the obstetric patients were admitted to the ICU. This rate of ICU admission is similar to the rate reported in the literature. The authors mentioned some differences between their findings and ours. We are not surprised by these differences, because variations are likely to exist between our hospital and theirs with regard to patient mix, ICU admission criteria, and hospital settings. Similar to the observation by Olarra et al, others have reported hypertensive diseases of pregnancy to be the most common reason for ICU admission.2,3 Like the study by Lapinsky et al, respiratory failure and hemodynamic instability were responsible for ICU admission of 80% of the patients in our study.2 Eclampsia, preeclampsia, the syndrome of hemolytic anemia, elevated liver
enzymes, and low platelet count (HELLP) were present in 33 of our patients. Although these 33 patients may have had coexistent hypertension, that condition was the reason for ICU admission in only 5 patients. Our obstetric unit has equipment and staff to provide care to noncomplicated cases of hypertension without ICU admission, explaining this observation. Although there was no significant difference in the incidence of pulmonary edema between patients who did and did not undergo emergent cesarean section, pulmonary edema was the most common reason for ICU admission in our study. A large, earlier study had also shown that pulmonary edema is a common occurrence in obstetric patients admitted to ICU. We are surprised by the absence of any case of pulmonary edema in the study by Olarra et al.

The reported mortality rate of obstetric patients admitted to ICU ranges between 0% and 36%. The mortality rate reported by Olarra et al is within this range. Heterogeneity of the patient population and differences in disease severity may account for the differences in the reported mortality rates of critically ill obstetric patients. We agree with Olarra et al on the need for future studies to identify the risk factors for obstetric-associated critical illness, in order to decrease the associated morbidity and mortality.

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Strength of Evidence for Low-Molecular-Weight Heparin

To the Editor:

In the Sixth ACCP Consensus Conference on Antithrombotic Therapy, Geerts et al recommend (evidence 1A) the use of low-molecular-weight-heparin (LMWH) for prevention of venous thromboembolism in patients with ischemic stroke and impaired mobility. As reported, the recommendation is based on the results of three randomized control trials; one of those trials compared LMWH with unfractionated heparin and yielded no evidence on the disadvantages of nonprevention. The results of the two smaller-sized placebo-controlled trials are in disagreement. We think that this recommendation lacked a clear-cut connection to the evidence.

In support of our opinion, a meta-analysis by Bath et al assessed the efficacy and safety of treatment with LMWH in patients with acute ischemic stroke. They reported that treatment with LMWH reduced the risk of deep vein thrombosis (relative risk reduction [RRR], 45%; number needed to prevent [NNP] 40) and pulmonary embolism (RRR, 63%; NNP, 80), but it increased the risk of major extracranial bleeding (RRR, 53%; number needed to damage [NND], 80) and probably of intracranial hemorrhage (odds ratio, 1.77; confidence interval, 0.95–3.3), with no change in mortality. This study does not support the routine use of LMWH. However, it might be useful in patients with additional risk factors and greater benefit/risk ratios. Furthermore, most patients with stroke are receiving aspirin, and the baseline risk of venous thromboembolism is likely to be downgraded as a benefit of aspirin treatment. Thus, according to our present knowledge, treatment with LMWH can hardly be considered a routine, evidence-based recommendation for prevention of venous thromboembolism in patients with acute ischemic stroke.

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To the Editor:

Dr. Alonso and colleagues suggest that the use of low-molecular-weight heparin (LMWH) should not have been given a grade 1A recommendation as one of the thromboprophylaxis options in patients with ischemic stroke by the Sixth American College of Chest Physicians (ACCP) Consensus Conference on Antithrombotic Therapy. I certainly agree that more studies of thromboprophylaxis in stroke patients are needed. However, based on the available literature, our grade 1A recommendation for the use of LMWH (or low-dose heparin) stands.

Four randomized trials have compared prophylactic LMWH to either placebo or to low-dose unfractionated heparin (LDUH) in stroke patients. In the two LMWH vs placebo trials, prophylaxis with LMWH was associated with a 35% relative risk reduction for deep venous thrombosis (DVT). In the two LMWH vs LDUH studies, the use of LMWH was associated with a 37% relative risk reduction for DVT.

Alonso and colleagues use the meta-analysis by Bath et al to support their view. This study identified a DVT rate in stroke patients not receiving prophylaxis that is high enough to warrant prophylaxis (37%). Unfortunately, this review pooled studies of