Editorial

Weekend versus weekday admission mortality

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In their article, Crowley and colleagues1 address an issue that is of interest not only from the neurosurgical point of view but also from a more general sociological point of view. Stimulated by a series of studies that have shown that the short-term mortality of patients admitted with some specific diagnoses is higher when they are admitted during the weekend, the authors queried the Nationwide Inpatient Sample (NIS) database for patients admitted with subarachnoid hemorrhage (SAH) during the year 2004. Using appropriate statistical analysis, they concluded that there was no difference in the short-term mortality regardless of whether the patients were admitted Monday through Friday or during the weekend (Saturday or Sunday).

Is it important to know that, while patients admitted with a variety of conditions that require acute care such as myocardial infarction, pulmonary embolism, ruptured abdominal aortic aneurysm, stroke, and acute epiglottitis have a greater short-term mortality when admitted during the weekend, patients with SAH do not. The authors have addressed this issue well, and I will add some comments.

First of all, I have little to add to the authors’ discussion of the limitations of this type of database. The obvious advantage is the large number of cases that can be analyzed and the fact that they include a very broad spectrum of types of hospitals, geographic locations, patient demographics, and so forth. The obvious weaknesses include an inability to determine the severity of the condition at admission and more specific information about outcome, beyond the straightforward parameter of mortality and indirect indices of morbidity, such as whether the patient was discharged home or to a continuing care facility. In this particular instance, the authors chose to utilize a clear-cut outcome measure, short-term mortality, which, of course, is a very incomplete indicator of quality of care. In this respect, I would like to emphasize the obvious fact that short-term mortality is a much more important indicator of the quality of acute care in conditions such as myocardial infarction, pulmonary embolism, and, particularly, ruptured abdominal aortic aneurysm and acute epiglottitis. Just to take the last of these conditions, whether one performs an emergency tracheotomy when necessary determines whether the patient lives or dies.

What does “short-term” mortality after SAH indicate in terms of quality of care, which is really the important issue that I believe the authors are trying to address? In other words, how can we influence, with our care, short-term mortality from SAH? First of all, it should be noted that although the authors looked at mortality at 7 days, 14 days, and 30 days, in fact, most of the deaths occurred within the first 7 days; 19% of the patients were dead within 7 days and 24% at 30 days. What were most of the deaths that occurred within 7 days due to? Although this database does not permit this analysis, it is fair to speculate that most of these patients died as an immediate result of the severity of the initial SAH. The fact that our care has little influence on this aspect of the overall morbidity and mortality of SAH does not need extensive discussion. Of course we could delay or even prevent death, but only to increase long-term severe disability in these patients who present with a devastating SAH. Our treatment of vasospasm is unlikely to have any significant impact on early mortality, but of course it can have a major impact on long-term morbidity and possibly even long-term mortality. Likewise, the quality of intensive care in terms of dealing with associated morbidity, such as pulmonary problems, infections, and electrolyte imbalance, should have little impact on early mortality. Granted, the ability to diagnose and appropriately treat acute hydrocephalus may be the difference between living and dying for some patients, but this group is small. We are left then with aneurysmal rebleeding, which, of course, is still an important cause of early mortality. Through early treatment of the aneurysm, we can indeed influence early mortality as a result of rebleeding. Therefore, I agree with the implication of the authors that what their data indicates is that in 2004, patients with ruptured aneurysms were as likely to have their aneurysm treated early after their SAH when they were admitted during the weekend as they were if they were admitted during a weekday. Patients who went initially to an emergency room and were directly transferred to another hospital did not count as admissions to the initial hospital. We can reasonably speculate that the reason for the transfer in most of those cases was the unavailability of a team capable of treating the aneurysm in an expedient fashion. The authors go further in their interpretation of their data to speculate that they also indicate that there has been increased “regionalization” of care for patients with SAH. In other words, that increasingly these patients are referred to major centers where they are cared for in neurological intensive care units and where full capabilities exist not only for urgent open microsurgical treatment of the aneurysm but also for endovascular treatment.

Do the authors’ findings mean that we neurosur-
I congratulate the authors on this important study, which indicates, as they concluded, that “the specialty of neurosurgery” is achieving the goal of treating patients with SAH in the same manner regardless of whether they present on a weekday or during the weekend. I would only add that, as stated above, nowadays we must share this credit with other specialists involved in the care of these patients. Importantly, this study does not tell us anything about the quality of that care; it concretely tells us that early mortality, most of which occurs within the first 7 days after SAH, is similar whether the patient is admitted during the week or during the weekend. The rest of the reasonable implications that the authors derive from these data, and with which I agree by and large, are no more than speculations.

Response

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We sincerely appreciate Dr. Heros’ thoughtful discussion of our article. We agree with all of the points that he raised; however, we would like to specifically comment on 2 of these points.

The first of these is regarding the statement: “the health care providers who treat patients with stroke are often the same providers who treat those patients with SAH.” We agree completely with Dr. Heros’ comments that neurosurgeons are the primary physicians charged with the care of patients with SAH, and we apologize if our comments suggested anything other than that. At our institution, as we know is the case in many other centers that treat aneurysms, the neurosurgeon is the primary physician for every patient in the hospital with SAH. What the above statement was referring to was the large number of other health care providers who help treat patients with stroke and SAH—nurses, patient care technicians, and respiratory therapists, among others. In this regard, patients with ischemic stroke often have the same, if not a similar treatment team as those with SAH, albeit with a different “captain” of the team. Much of the impetus for our study came from concern that developed after reading the studies demonstrating that patients with stroke have increased short-term mortality rates if admitted on the weekends,