muscle groups responding can be palpated by the surgeon or an assisting physical therapist).

Our study was not the place to discuss in detail our opinions of the original classification scheme described by Fasano, et al., or the modifications and quantification of that scheme made by Phillips and Park. Nevertheless, our impressions were in complete agreement with Peacock and colleagues that sustained responses within the muscle segment being stimulated might indeed be "normal." We noted that several other centers had reported at least this degree of "abnormal" response from patients without spasticity. To better deal with these issues, we have recently completed a detailed series of animal experiments in both the intact and spastic spinal cord, and these results confirm this impression. The problem here is that no one has been able to carefully detail the differences between the response of normal human roots and rootlets, and the response of similar spastic roots and rootlets. Our laboratory work, recorded under ideal stimulation and recording conditions, established that sustained responses to 50 Hz stimuli at threshold in the segment being tested, in addition to sustained responses in adjacent or distal segments of the ipsilateral limb, may be found in the normal mammalian nervous system (Rivera, et al., unpublished data).

We completely agree with Dr. Peacock and colleagues' conclusion that we cannot assume that monitoring adds little to patient outcome when compared to random partial rhizotomy, in the results obtained. We have strongly urged that only a well-performed randomized clinical trial, comparing this surgery with and without the use of sophisticated monitoring, can answer this question. Nevertheless, if sophisticated electrophysiological monitoring adds little to patient outcome, then it should be eliminated. If it were not of value, then a substantial reduction in operative time and costs, in addition to a likely reduction in operative risk, might be realized. If, however, monitoring improves outcome when compared to random partial rhizotomy, then much work would have to be done to understand why, and a more consistent protocol would have to be developed to assist the practicing surgeon in performing this procedure.

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References

Natural History of Unruptured Aneurysms

To THE EDITOR: Dr. Juvela and his associates should be complimented with their interesting study on the natural history of unruptured intracranial aneurysms (Juvela S, Porras M, Heiskanen O: Natural history of unruptured intracranial aneurysms: a long-term follow-up study. J Neurosurg 79:174–182, August, 1993). However, a few points should be made. First of all, the authors relate not only the largest diameter of the aneurysms to the risk of rupture (as has usually been done in other studies) but also its estimated volume. They compute the volume of the aneurysm with the formula $2\pi r^3 (R - \sqrt{3}r)$, where $r$ denotes the smallest and $R$ the largest diameter of the aneurysm as seen on the angiogram. We do not understand why they do not use the correct formula for the volume of a spheroid body, $\frac{4}{3}\pi r^2 R$, which, like the first formula, reduces the special case of a sphere to $\frac{4}{3}\pi r^3$. When the diameter of the aneurysm $R$ is twice the size of its smallest diameter $r$, Juvela's formula overestimates the volume by 25% (substitution of $2r$ for $R$ gives $\frac{4}{3}\pi r^3$ instead of $\frac{4}{3}\pi r^3$).

More importantly, the authors conclude that "... an unruptured aneurysm should be operated on irrespective of its size, if it is technically possible and the age and concurrent diseases of the patient do not increase the risk of surgery." This statement is not based on an explicit consideration of the pro's and con's of neurosurgical treatment. The contraindications they mention seem to relate to surgical risks, but they need more nuance in our opinion. Clearly, patients with a limited life-span because of age and morbidity have only a low cumulative risk of rupture. Thus, for this type of patient the balance may turn against surgery, but for young and healthy patients the balance may swing in favor of aneurysm surgery, despite considerable surgical risks.

The overall annual risk of rupture of approximately 1.4% as estimated by the authors is higher than that reported by Jane, et al. Perhaps this can be (partly) explained by the rather large proportion of patients with multiple aneurysms. When this figure is (crudely) adjusted for the number of aneurysms studied, the annual risk per aneurysm amounts to 1.1%, which is more in accordance with the previous study. The authors report the effect of aneurysm size as a relative risk of 2.24 (95% confidence interval 1.01 to 4.90) for aneurysm rupture in patients whose largest aneurysm was at least 7 mm in diameter. It is unclear whether these patients were also the ones with multiple aneurysms, which would bias the estimated relative risk. Moreover, it would be interesting to know not only the relative risk but also the absolute risk of aneurysm rupture in the two categories. This information would...
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make this study even more important for the management of the patient with an incidental intracranial aneurysm.

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RESPONSE: We thank Drs. Dippel and Habbema for their interest and criticism. Answers to most of their questions can be found in our paper by a careful reading, which requires knowledge of basic principles of geometry and statistics. First of all, they seem to confuse radius and diameter. "Radius" is one-half of the diameter. We used the formula $2\pi r_1^2 + \pi r_2^2 (2R - 2r)$. In the case of a sphere, it is $\frac{4}{3}\pi r^3$. The formula $\frac{4}{3}\pi R$ suggested by Dippel and Habbema is a special form of ellipsoid (volume = $\frac{4}{3}\pi r_1 r_2 r_3$, where $r_1, r_2, r_3$ represent three respective radii in three dimensions). The overestimation of volume by our formula is smaller than is the underestimation by the "correct" formula suggested by Dippel and Habbema. This is, however, an academic question since we reached similar results by both methods.

We have not concluded that every unruptured aneurysm should be operated on. The surgery must be considered individually based on the diameter of the unruptured aneurysm, and on the age and possible diseases of the patient as well as on the surgical experience of the neurosurgeon. The younger the patient is and the larger the aneurysm, the greater is the indication for surgery of an unruptured aneurysm. As we pointed out, there seems not to be any critical diameter of aneurysm or patient age above or under which surgery is clearly indicated, but we hope that our paper can help the decision for surgery.

Dippel and Habbema thought that our estimated overall risk (1.4% annually; 27 first aneurysm ruptures during 1944 patient-years) was too high, but this was similar to the study by Wiebers, et al.7 (15 aneurysm ruptures during 1079 patient-years). These two studies are the largest to be published, and they are also the only ones that included life-table and multivariate analyses. A crude estimate of 1% annual risk of rupture was reported in a review article and was based on two smaller studies.3 The decimals of percentages are, however, not very important points of view.

Dippel and Habbema suggest that the independent observation unit for the statistics should be the aneurysm, and not the patient as we used. This would run against the statistical principles and yield biased incidence figures. Based on this suggestion, a patient with three (or two) unruptured aneurysms who suffers a subarachnoid hemorrhage (SAH) has only one-third (or one-half) of the risk of hemorrhage in the incidence calculations. A patient with three (or two) unruptured aneurysms is not equivalent to three (or two) patients with one unruptured aneurysm. The total follow-up times in the incidence calculations are based on patient-years or person-years, not on aneurysm-years. As pointed out in the article, the incidence of multiple aneurysms depends primarily on the completeness of the diagnostic procedures. Therefore, the true incidence of single or multiple aneurysms is seldom known in clinical studies. By the suggested method, we obtain an annual risk of 1.1% (27 events during 2434 aneurysm-years). In patients with a single aneurysm, it is 1.4% (22 events in 109 patients during 1538 aneurysm-years and also patient-years). However, in patients with multiple unruptured aneurysms, it is only 0.56% (five events during 896 aneurysm-years; five of 27 patients with two unruptured aneurysms but none of six patients with three unruptured aneurysms had a hemorrhage), although the true annual incidence was 1.2% (five events during 406 patient-years). This is because only one aneurysm in a patient (usually the largest one) may rupture at a time, while the other aneurysms remain censored observations concerning the follow-up time. It is very unlikely that these additional aneurysms could protect the largest one from rupture. For life-table analysis and Cox models, we followed patients and considered the number of aneurysms as well as the aneurysm group as covariates.

The estimated relative risk of aneurysm rupture in those whose largest aneurysm was at least 7 mm in diameter in the multiple aneurysm group was 2.19 (95% confidence interval 0.90 to 5.26, $p = 0.086$) compared with those with an aneurysm of 6 mm or smaller. The average annual risk of aneurysm rupture among those with a largest aneurysm of 7 mm or greater was 2.5% (nine first ruptures during 364 patient-years) and 1.1% (18 ruptures during 1580 patient-years) among those with aneurysms less than 7 mm in diameter.

We think that surgery of unruptured aneurysms and other preventive methods are important and should not be underestimated since, thereby, the overall results may be improved on the population level. A high mortality rate from a severe initial bleed seems to be a reason for the observation that the overall results are quite modest even when an aggressive neurosurgical treatment policy is adopted.1 The early recognition of mild symptoms of aneurysm rupture is also important.8 The most important modifiable risk factor for SAH or an-