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THE EDITORIALIST REPLIES: The topic of the editorial is prevention of awareness in patients undergoing general anesthesia with the use of an inhaled anesthetic gas, not other possible applications of BIS monitoring. Thus, the suggestion by Kelley and colleagues that there is a "preponderance of clinical evidence" supporting the value of BIS monitoring is misleading. In fact, only a limited number of prospective, randomized, double-blind clinical trials (as opposed to observational studies) have investigated the effectiveness of BIS monitoring in preventing awareness.[1] Furthermore, it is incorrect for Kelley et al. to claim that the study by Avidan et al. "confirmed that BIS-guided care achieves a 0.2% incidence of awareness." The study was not designed to determine whether BIS-guided monitoring alone influenced awareness. Rather, it determined the effects of BIS monitoring when used in conjunction with an active comparator, the monitoring of ETAG. In this context, there was no additional benefit of BIS monitoring. Given that ETAG monitoring is readily available in many operating rooms in North America, any additional benefit of BIS monitoring is questionable, considering the added expense. Finally, the suggestion that inadequate vigilance or poor protocol compliance by the anesthesiologist accounts for failures of BIS monitoring assumes that simplistic protocols can be used to govern anesthesia care, which is inconsistent with clinical experience.

In response to Cook's comments: patients in the study by Avidan et al. did not undergo randomization with respect to nitrous oxide treatment. In a prospective, randomized trial of 2012 patients, the incidence of major complications was higher among those treated with nitrous oxide, and there was no reduction in the incidence of awareness.[2]

Given the importance of this topic, the updated meta-analysis presented by Myles and colleagues should certainly undergo an impartial peer review to consider important factors such as group heterogeneity, study quality, and the different weighting of the two major trials despite similar sample sizes.[3,4] Is it appropriate to include an awareness study of 30 patients?[1] The meta-analysis might also analyze and discuss the incidence of possible awareness. The patients' perceptions constitute their reality, and the incidence of possible awareness was higher than that of definite awareness in both major studies.[3,4]

Finally, if the calculations by Kelley and colleagues are correct, hundreds of thousands of patients would be needed to determine the usefulness of the monitor as compared with ETAG. A better approach would be to focus research on understanding the underlying disorder, and eventually bring transparent technology to bear on this serious adverse outcome.

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Violence-Related Mortality in Iraq, 2002–2006

TO THE EDITOR: Since 2004, when my colleagues and I published a report estimating the number of excess deaths resulting from the invasion of Iraq,[1] we have made further assessments, published in 2006.[2] The report on the Iraq Family Health Survey (IFHS) (Jan. 31 issue)[3] contains

some findings that are similar to those of our 2006 study but some that differ. The IFHS report estimated a very low crude mortality rate before the invasion as compared with the rate we calculated (3.17 vs. 5.5). Arguably, the increases in mortality reported in both studies for the period after the invasion were more similar: $6.01 \div 3.17 = 1.9$ for the IFHS versus $13.2 \div 5.5 = 2.4$ for our study, a difference of 21%.[3] It is unfortunate that the IFHS focused only on violence-related mortality.

IFHS modeling estimated that only 38% of deaths were unreported, but comparisons with rates of death in Iran and Syria suggest underreporting of 55% and 70%, respectively. The growth balance method used in the IFHS is untested in volatile populations. How did the omission of data from approximately 10% of the most violent parts of Iraq produce a relatively small increase in the width of the confidence interval? Most sources (the Iraq Body Count,[4] the Baghdad morgue, and the Pentagon) show more violence in 2006 than in 2003 or 2004, which is not evident in the IFHS report. However, these data suggest that our 2004 estimate of approximately 100,000 excess deaths was too low.

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TO THE EDITOR: As the Perspective article accompanying the IFHS report points out,[1] estimating the number of people killed as a result of a conflict is challenging. The media has widely reported the large difference between the rates of violence-related deaths reported in the IFHS and the rates of death from all causes reported by Burnham et al. in 2006.[2] The IFHS study group reported the primary outcome of violence-related mortality and found an estimated 150,000 deaths between 2003 and 2006, whereas Burnham et al. appropriately reported the rate of death from all causes during a similar period and found an estimated 654,000 deaths. The majority of deaths that result from any conflict are attributable not to violence but to the complex dilapidation of the normally protective public health infrastructure.[3] Focusing on violence alone ignores many of the deaths that have occurred during this invasion. Using the crude mortality rates in the IFHS report, the actual excess mortality in Iraq between 2003 and 2006 was approximately 433,000 (95% confidence interval [CI], 354,000 to 523,000). Indeed, absent from the IFHS report is an acknowledgment that the combined totals actually approach those of the 2006 study by Burnham et al.

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TO THE EDITOR: The estimated mortality rate in Iraq before the invasion, as reported by the IFHS study group, was lower than the estimated rates in neighboring countries[1] and lower than half the rates estimated from other sources.[1-3] Although the survey's large size allows for more precision, systematic error (bias) is not avoided. The authors acknowledge the difficulties involved in surveying high-mortality governorates (HMGs), but reliable figures for these areas are essential for obtaining an overall estimate of violence-related mortality.

Discrepancies with previous estimates of the violence-related mortality rate in Iraq — a difference by a factor of almost 10 for HMGs (Table 1) — could be due to the low proportion of violent deaths attributed to HMGs as compared with the population in these areas (31% vs. 38%); the fact that the IFHS imputed missing data for HMGs using the Iraq Body Count, which probably underweights the HMGs as a result of publication bias; and use of a very long questionnaire, which has been shown to lead to underestimates of mortality.[4]

A reliable estimate of the violence-related

Table 1. Geographic Distribution of Violence-Related Deaths in Iraq during the Post-Invasion Period (May 2003–June 2006), According to the IFHS and Burnham et al.

Region	Violence-Related Deaths	Population at Risk	VMR	VMRR	Total Estimated No. of Violence-Related Deaths
	no. (%)	*no. of person-years (%)*			
IFHS					
Baghdad	176 (54)	40,886 (21)	4.3	Reference	88,747
High-mortality governorates	101 (31)	74,127 (38)	1.4	0.32	41,508
Low-mortality governorates	42 (13)	55,121 (28)	0.8	0.18	17,630
Kurdistan	7 (2)	25,536 (13)	0.3	0.06	3,115
Total	326 (100)*	195,670 (100)	1.7	0.39	151,000
Burnham et al.[2]					
Baghdad	78 (26)	9,964 (24)	7.8	Reference	156,267
High-mortality governorates	192 (64)	14,719 (35)	13.0	1.67	384,657
Low-mortality governorates	21 (7)	11,085 (27)	1.9	0.24	42,072
Kurdistan	9 (3)	5,899 (14)	1.5	0.19	18,031
Total	300 (100)	41,667 (100)	7.2	0.92	601,027

* The total number of violence-related deaths in the Iraq Family Health Survey (IFHS) was computed with the use of the violence-related mortality rates per 1000 person-years (VMR) and the person-time at risk, as provided in the IFHS report on page 484 and in the Supplementary Appendix (E-Table 4). VMRR denotes violence-related mortality rate ratio.

mortality rate in HMGs is urgently needed to clarify the conflict-related death toll in Iraq.

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TO THE EDITOR: The IFHS study group made a serious error by assuming no spatial bias in the Iraq Body Count database. The Iraq Body Count project does not purport to be a random sample; it reflects the distribution of reporters as much as it does the distribution of violence. The IFHS spatial results align closely with those of the Iraq Body Count not because of the survey findings (the survey group did not visit the most dangerous clusters) but because of the assumptions they imported from the spatial pattern in the Iraq Body Count. If we assume that the spatial distribution in the study reported by Burnham et al.[1] is correct (it remains the only study to scientifically investigate this distribution), that factor alone could double the number of violence-related deaths found by IFHS to 300,000. The authors acknowledge and attempt to correct for underreporting of deaths from nonviolent causes, but they make no allowance for the more serious underreporting of violence-related deaths to government-affiliated survey takers. This leads the IFHS to implausibly conclude that less than one third of excess deaths were due to violence. When these two sources of error are taken together, the IFHS results are easily in line with the finding of more than 600,000 violent deaths in the study by Burnham et al.

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THE AUTHORS REPLY: Our estimate of violent deaths in Iraq from March 2003 to June 2006 was based on the 2006–2007 IFHS, a cross-sectional, nationally representative survey of 9345 households. We reported only the analysis of violence-related mortality, not our analysis of deaths due to other causes.

Substantial underreporting of deaths is common in household surveys because of recall bias, the effects of migration, and missing households. The security risks in Iraq add to the reporting problems. The geographic heterogeneity of violence-related death rates may have further affected reported deaths, even though 971 clusters were sampled.

The preinvasion crude mortality rate of 3.17 (95% CI, 2.70 to 3.75) in the IFHS report is lower than the rate of 5.5 (95% CI, 4.3 to 7.1) reported by Burnham et al. Because the level of underreporting is almost certainly higher for deaths in earlier time periods, we did not attempt to estimate excess deaths. The excess deaths reported by Burnham et al. included only 8.2% of deaths from nonviolent causes, so inclusion of these deaths will not increase the agreement between the estimates from the IFHS and Burnham et al.

We imputed data for missing clusters in the Anbar province among the HMGs, and we included uncertainty in this imputation; it cannot account for the 10-fold difference between our rates and those reported in other HMGs. Burnham et al. selected only a few clusters in each of the HMGs, which may not be representative of those governorates. We estimated that 45% of violence-related deaths were in HMGs after the adjustment, as compared with 36% of the Iraqi population. This may even be an overestimate if the reports collected by the Iraq Body Count project concentrate on high-impact events closer to main cities.

Although the estimated number of violence-related deaths in the IFHS is approximately three times higher than those reported by the Iraq Body Count project, the results are consistent with the Iraq Body Count trends and distribution, based on collations of press reports for civilian casualties. As indicated in Table 4 of our report, the IFHS and the Iraq Body Count both showed a drop and a subsequent increase in violence-related deaths, but not a doubling and then more than a fourfold increase for the same time periods as reported by Burnham et al.

To reach the 2005–2006 death rate of more than 900 per day, estimated by Burnham et al., the IFHS would have had to miss nearly 90% of violence-related deaths. It is unlikely that a small survey with only 47 clusters has provided a more accurate estimate of violence-related mortality than a much larger survey sampling of 971 clusters. We may never know with any accuracy the effect of the conflict in Iraq on mortality, but all the evidence points to a high level of deaths due to violence.

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Slowing the Growth of Health Care Costs

TO THE EDITOR: The limitation of the article by Mongan et al. (April 3 issue)[1] on options for slowing the growth of health care costs is the acceptance only of reforms that will not irritate powerful, entrenched corporate and labor interests. Our system is costing us perhaps twice as much as it should. We need to target the fattest cats and slim them down. Hospitals currently seek consolidation rather than efficiencies, charge a fortune for routine services, and pay higher wages rather than taking strikes. Insurance companies add so little at so high a charge. Pharmaceutical companies advertise, develop frivolously repetitive drugs, and charge without restraint. Medical specialists in some areas operate and serve dying patients with little restraint.

The interventions suggested in the article are popguns against a profit-bound army. Restraint in our mixed system needs to come from government, not voluntary acts by corporations.[2] Schlesinger[3] has identified a cycle of government activism at 30 years, the last peak being 40